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THE AUDITORY MEMORY AFTER-IMAGE

By Claire Comstock Dimmick

The purpose of this investigation has been to secure a description of the memory after-image in order to determine whether it deserves a place of its own in a classification of images or whether, as has been suggested,[1] it is a blending of other images and merely appears to be unitary. The auditory rather than the visual field has been chosen to avoid the difficulties of dealing with positive and negative after-images; for whether or not there is any after-effect of auditory stimulation, it seems to have been conclusively shown that "there is no positive after-image of tone, analogous to the positive after-image of vision." [2]

Following the suggestion made by Titchener that "the name of memory after-image has been given to an experience which is most familiar, perhaps, in the taking of dictation",[3] we began our experimental work with the following instructions: "I shall dictate to you some sentences which you are to take down as accurately as possible. When I say 'Now', stop writing and describe as well as you can your experiences in taking the dictation." In the preliminary experiments *E* read from any book at hand; but for the systematic observations sentences, all of a standard length, taken from Locke's "Essay Concerning Human Understanding" were used. No effort was made to select complete sentences, for we desired to have the attention of the *Os* on the sound rather than on the meaning, so that the sentences were somewhat obscure in meaning and incomplete as to form. *E* read fast enough to keep from 4 to 6 words ahead of *O*, who was required to take down every word, though he was permitted to use abbreviations. When *E* said "Stop," she was always from 4 to 6 words ahead of *O*; that is, there were 4 to 6 words of the sentence that *O* had not yet written.

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[1] L. J. Martin, Die Projektionsmethode u. d. Lokalisation visueller u. a. Vorstellungsbilder, *Zeits f. Psych.*, 61, 1912, 346ff.

[2] H. G. Bishop, The Positive After-Image in Audition, this Journal, 32, 1921, 325.

[3] E. B. Titchener, *A Beginner's Psychology*, 1918, 74.

There were 5 *Os*: Mr. DeGowan (De), Miss Stewart (St), Miss Sutherland (Su), all undergraduate students in psychology at the University of Michigan; Mr. J. D. Finlayson (F), and Dr. F. L. Dimmick (Di), instructors in psychology. All the *Os*, with the exception of St who gave only 8 hours to the experiment, worked 2 hours a week during the second semester.

RESULTS

All of the *Os* reported the presence and use of auditory imagery in the taking of dictation.

"There seem to be 2 parallel processes . . . . the last words of a group of words seem to stick in my mind and reëcho at the same time that you are talking" (De).

"I could hear the last three words just as you said 'Stop' repeated in auditory imagery" (Di).

"When I stopped the rest of the sentence sounded vividly in mind. What I heard was your voice repeating the rest of the sentence" (F).

"First I heard you, then I wrote from hearing you again" (Su).

"I hear the words after you've stopped; I hear it all to the end" (St).

After a number of observations had been made, it was obvious from a consideration of the reports that the most frequently and easily observed auditory images were those which came immediately after *E* said "Stop." All of the *Os* reported that the sentences finished themselves out in auditory imagery, though they had been instructed to stop writing. This imagery was not called up, but was "just there." Some of the inexperienced *Os*, indeed, did not report in terms of imagery, but merely said that they "heard something." All of the *Os* said that they were surprised to hear the "Stop" at the end of the words (in imagery) for it was obviously irrelevant to the meaning. The "tagging on" of this word indicates how closely the imagery copied the perception and how spontaneously it came.

Since the auditory image which came after the command to stop was invariably present and always in *E*'s voice, the *Os* were next asked to describe this image attributively. The procedure was the same as outlined above, except that *O* was asked to observe only the auditory image at the end of the dictation. Reports on the intensity, duration, pitch and richness[4] of the auditory image were called for. With the exception of St, all *Os* gave at least 50 reports on each one of the above named attributes. St gave 25. A table of the results follows.

---

[4] The category "Richness" was used to include those descriptive phrases which, while not strictly attributive, indicate the make-up or timbre of the tone, which was not further analysable.

	INTENSITY					DURATION		PITCH		RICHNESS			
	Same	Slightly less	Less	Much less	Very much less	Same	*Longer	Same	Higher	Same	Slightly Less	Less	Much less
Su	29	32	2	—	—	46	4	49	1	33	13	4	—
De	38	16	—	—	—	14	36	51	—	34	10	6	—
Di	—	21	26	2	2	53	—	51	—	3	27	16	4
F	—	7	9	35	14	60	1	52	—	45	—	5	—
St	23	2	—	—	—	20	5	27	—	27	—	—	—

All descriptions of the image are in terms of its likeness to the perception, so that "Same" indicates of the same intensity as the perception; "Less" indicates of less intensity than the perception.

*The total duration of the image was lengthened by the 'dragging out' of the last word.

These reports show that the auditory image differs from the auditory perception chiefly with respect to the attributes of intensity and richness. Yet this difference is not great for all *Os*; Su, De, and St find the image as loud as (or only slightly less loud than) the perception, and all *Os* report that the image differs little from the perception in richness. F, whose images were much less in intensity than the perception, also reported later that his memory-images were very weak; whereas Su and St described their memory-images as being of moderate intensity. The duration of the image is the same as that of the perception, except in those cases where the dragging out of the last word made the total duration longer. The auditory memory after-image, then, under these conditions is an excellent copy of the perception in pitch and duration; and for three of the *Os* in intensity and richness, though two *Os* found the image less than the perception in these respects. Su, St, and De frequently reported that they could not note any attributive differences between the image and the perception and, indeed, would not have been able in many cases to distinguish between them except on a basis of stimulus difference.

The *Os* were also asked to report on the temporal course of the image. The only attributive changes described by any *O* were in 3 cases, in which the last word of the image lasted longer than the preceding words and decreased in intensity or faded out. All images came immediately or after a very brief interval after *E* said "Stop." For 3 *Os* the image came all at once or was "just there"; for 2 *Os* the image came "word by word" and went similarly, so that when each subsequent word came the preceding word was gone. The experimenter is inclined to believe that the "word by word" reports are the result of a logicizing rather than a purely descriptive attitude.

The question now arose how this image which we had been calling the memory after-image differed from the memory-image; if, perhaps, it might not be a memory-image. In order to secure memory-images under conditions as similar as possible to those under which we studied the memory after-image, we instructed the *O*s as follows: "I shall read you a sentence. Write it down as accurately as you can. You will be given further repetitions until you can write it perfectly." The same sentences that had been used before were also used here. We hoped, by dictating the sentences to be learned, to put a premium on the use of auditory imagery in their revival; and by having them learned by means of written rather than spoken repetitions to decrease the use of verbal-motor imagery and imagery of the *O*s' own voices in the reproductions. We hoped to get a reproduction in terms of *E*'s voice so as to make the two kinds of images more comparable. After *O* had memorized 3 sentences in this way, the following instructions were given: "I shall read you a part of one of the sentences that you have memorized. Complete the sentence as accurately as you can. When I say 'Stop,' stop writing and describe your experiences in completing the sentence." Each sentence was divided into six parts, so that there were six places where *E* might stop and *O* supply the following phrase. As soon as *O* had written the first 2 words of the supplied phrase, *E* said "Stop." Samples of the types of reports secured under these conditions are given for each *O*.

"The completion didn't come immediately and I repeated the first part of the sentence. Then the completion came in the form of auditory imagery, partly in your voice, partly in mine" (Su).

"I had an auditory image, but I said it along with the image. I don't know whose voice the image was in" (Su).

"I had to repeat some of the preceding words. The completion came in the form of a verbal image with an auditory component. The verbal-motor part was the more prominent" (De).

"The completion came mostly in verbal-motor imagery until I got to the second word and it came in auditory imagery" (De).

"I got the phrase mainly in verbal-motor imagery with a very weak auditory accompaniment. I can't say whether the image was in *E*'s voice or not" (Di).

"I got a very weak auditory image but not in *E*'s voice. This is not at all comparable to the other experience (memory after-image)" (Di).

"The phrase came in just meaning; no imagery" (F).

"Suddenly the meaning came. Then the phrase came in verbal-motor imagery" (F).

"The completion came in verbal-motor imagery and auditory imagery of your voice. The verbal-motor imagery was the more important" (St).

The foregoing reports show that when the *O*s were left to themselves to recall a phrase presented auditorily the general tendency was to recall in terms of verbal-motor imagery. Auditory imagery, if present, was weak and subordinate to verbal-motor. Thus we did not secure the description of auditory imagery that we were hoping for, but we did get evidence that these *Os* were not auditory-minded, so that the auditory images reported in the first part of the experiment did not occur because of any tendency to belong to the auditory type.

The instructions were now changed to provide for a report on auditory imagery and read as follows: "I shall read you part of one of the sentences you have learned. When I stop complete the sentence in auditory images of my voice. Report, however, upon the total experience from the 'Stop' to the disappearance of the auditory image." The reports given show that in most instances the phrase to be supplied did not come in auditory imagery but had "to be put into" auditory imagery.

"I said it until I could hear you saying it; it didn't come in auditory imagery. There was a feeling of effort" (Su).

"The phrase came immediately in your voice. I can stop this imagery anywhere and have it go on in your voice, but when I'm taking dictation I can't. I usually can't help myself but listen to it finish; but if I do stop it, it doesn't come back in your voice" (Su).

"I ran back over the sentence in verbal-motor imagery; I could by effort get a slight auditory image, but accompanying it was verbal-motor imagery. The auditory image was very faint but in your voice" (De).

"The last few words that you speak hang over in auditory imagery and verbal-motor imagery; then a verbal-motor image of the completion comes. Then I can get an auditory image by trying to accompany the verbal-motor imagery. I am always conscious of trying to imitate your voice" (De).

"I have to force the verbal-motor imagery into an imitation of your inflections. The other images (memory after-images) seem to persist from the perception. I get them without verbal-motor imagery" (De).

"I can put it into auditory imagery, but verbal-motor imagery is still there. If I were let alone I'd remember it that way and not auditorily. The verbal-motor images make the sounds into words" (Di).

"I thought the sentence through in verbal-motor imagery; then I put it into auditory imagery. It is always voluntary" (F).

"The completion didn't come in your voice, but I had to put it in it" (St).

Since all the *Os* could, though with effort, get an auditory reproduction, they were next asked to attend to the auditory image and to describe it attributively. It should perhaps be stated that the effort required was no greater than that usually necessary to call up an auditory image. The point we have been making above is that the *Os* were not of the auditory type though all could get auditory images. Inasmuch as there have been numerous studies of memory-images of all modalities and our time was limited, we did not try to make a throughgoing study of the auditory memory-image, but attempted merely to get from these *Os* a description of their auditory memory-images in order to have a basis for comparison with the images described in the first part of the experiment.

Accordingly we asked for only 10 to 20 reports from each *O*. These reports we have summarized.

For Su the intensity of the auditory memory image varied from "almost as intense as" to "moderately intense," the preponderance of cases being in the latter category; the duration, the pitch, and the richness were the same as in the spoken word. For De the intensity was either weak or very weak, the duration was the same as that of the verbal-motor imagery always accompanying it which could be prolonged at will, the pitch was the same as that of the perception, the richness was either less or very much less. For Di the intensity was for the most part weak, at times very weak, the duration and pitch were the same as those of the perception, the richness was always much less. For F the intensity varied between weak and very weak, the duration and pitch were the same as those of the perception, and the richness was sometimes the same as that of the perception and sometimes weak. For St the intensity varied between "almost as intense as", moderately intense, and weak; the duration and pitch were the same as in the perception; the richness was the same or "almost the same" as that of the perception.

In addition we secured reports on the localization of both the memory-image and the memory after-image. For Su both images were with one exception localized inside the head. Once the memory after-image was localized outside, above the head. For De the memory-image was "internal, in the front of the head," except once when it seemed peculiarly like *E*'s voice and was projected; the memory after-image was objective, external, projected out from a point about level with *E*'s head, and seemed to come from the direction in which *E* was sitting. For Di the memory-image was inside the head; the memory after-image came "from toward *E* though less definitely than the perception" and seemed partly external and partly internal, for there was "some ring in the ears themselves." For F the memory-image was inside the head; the memory after-image was outside the head and from the general direction of *E*. For St the memory-image was in general inside the head; the memory after-image was outside, though it once was reported as being "half and half."

If we compare the attributive description of the memory-image with that of the memory after-image we find that in no case was the intensity of the memory-image as great as that of the memory after-image for the same *O*. Su, whose auditory memory after-images were as loud or almost as loud as the perception, described her memory-images as only moderately intense, though a few of them were "almost as intense." De, who had reported over two-thirds of his memory after-images to be as loud as the perception, had only weak or very weak memory-images. The memory-images described by Di were weak in intensity whereas his memory after-images had been moderately loud. F's reports do not show so marked a difference, though he never had a memory-image better than "weak" while some of his memory after-images were only slightly less loud than the perception. For St the memory-images were less intense than the memory after-images. So far as intensity goes, then, we find a difference in the two kinds of images. The attributes of pitch and duration present no basis for distinction. Richness, in general, seems less in the memory-image than in the memory after-image. The memory after-image is localized by all but one *O* (Su) as outside the head, whereas all *O*s localize the memory-image inside the head. For all *O*s the memory after-image is "just there," "persistent," while the memory-image comes only when tried for and is accompanied in most cases by a feeling of effort. Of the three bases for differentiation (1) attributes, (2) perceptive modes, (3) conditions of arousal, the first offers the least generally applicable criterion, though one which is valid for individual *Os*; the second a more reliable criterion under the conditions of this experiment; and the last the most positive.

Relation to Previous Investigations

The term memory after-image (Erinnerungsnachbild) was introduced by Fechner.[5] He describes experiences in the visual field only and bases his description of the memory after-image mainly on his own observations, though he records a few reports from others. For him a memory after-image came after a brief attentive glance at an object, when upon looking away or closing his eyes he saw for an instant very clearly an image of the object looked at in full color and outline. This, he says, he was unable to do in the case of objects he had seen a longer time before. The image, however, very quickly lost the definiteness of its outline and its color.

The conditions of our experiment have provided for attention of short duration to a stimulus of moderate intensity. Under these conditions the *Os* got memory after-images in the auditory field which seem to be like those which Fechner describes in the visual field; that is, they were good copies of the stimulus and were of short duration.

Fechner further states that there were individual differences in the appearance and characteristics of the memory after-image. For some they closely resembled the after-image, while for others they were more like the memory-image. We found similar individual differences in this investigation. For Su, De, and St the memory after-images were sensory in character; for F they were more like the memory-image; while for Di they partook of the characteristics of both.

To further substantiate the uniqueness of the memory after-image experience, Fechner describes four conditions which set it off from the after-image on the one hand and the memory-image on the other. These criteria of differentiation were naturally of interest to us, but since we have worked in the auditory field where an after-image corresponding to the visual after-image is lacking, we have not been able to make any complete verification of these criteria. Differences in method due to differences between visual and auditory experiences have further rendered a detailed comparison impossible. Some of the criteria, however, have been general enough to warrant a comparison of our results with those of Fechner.

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[5] G. T. Fechner, *Elemente der Psychophysik*, 1860, II, 491 ff.

The first criterion[6] states that, while after-images exist whether one has attended to the stimulus or not, attention to the stimulus is necessary for a memory after-image as for a memory-image. It is to be noted that the degree of attention, whether focal or marginal, is not stated, though Fechner says that the sharper the attention to the stimulus the clearer the memory after-image. Exner,[7] who speaks of "das primäre Gedächtnissbild" but who is evidently referring to the same phenomenon as Fechner's "Erinnerungsnachbild," says that it appears whether attention has been turned to the preceding sensory impression or not. He cites as an example the fact that people otherwise attentively engaged sometimes do not hear the first strokes of the clock striking the hour. They are able, however, to count them from the beginning after a number have gone by. This is done through the use of "das primäre Gedächtnissbild." Here, then, is a disagreement as to the conditions under which the memory after-image appears. Daniels,[8] who refers to both Fechner and Exner, carried on an experiment in which the *O*s read "interesting stories in a loud voice and with the greatest possible rapidity. The image used was that aroused by the pronunciation of three digits. When the subject was well under way in his reading, the digits were announced by the operator, the three occupying about two seconds. After a determined interval, at a signal from the operator the subject ceased reading and endeavored to recall the digits that had been given." The purpose of the experiment was to determine the duration of the memory after-image. The thing in which we are interested, however, is the fact that the subjects could recall the digits after the interval, though there was distraction of the attention during the original presentation of them. Daniels states that "absolutely perfect distraction was not secured," but it is certain that a high degree of attention was not given to the sound of the digits. Daniels raises no question as to the nature of the memory after-image, but assumes that the digits were recalled by means of it. His results, so far as attention to the stimulus is concerned, agree with Exner's, not with Fechner's, description of conditions necessary to the arousal of the memory after-image.

In order to secure an answer for ourselves, if possible, the following instructions were given to the *O*s: "I shall give you some figures to add. Add them as quickly and as accurately as you can. While you are adding I shall use as a distraction a number of taps given in series. Try not to be distracted by them. When I say 'Now' stop adding immediately." *E* gave a series of taps in groups ranging from one to six in number. When *O* seemed engrossed in the task of adding, *E* said "Now" and asked: "How many times did I tap last time?" The results obtained from the first trial (the most significant, because the *O*s did not know that they were to be asked to report on the number of taps and so had no reason to attend to their number as was the case in later trials) follow. Su gave the right answer and said: "I can still hear the last group in imagery and count from that. I was not counting the groups as you tapped." De gave the right answer and reported that he was not counting the taps during the addition. He did not know how he knew the answer. Later he reported that as soon as *E* said "Now" the taps "seemed to come to consciousness." F did not give the right answer. He said: "I wasn't counting taps until you spoke but then I counted them in memory. I counted the auditory reproduction in verbal-motor imagery." St obeyed the instructions not to be distracted so well that at no time could she tell the number of taps. She reported: "I have no idea how many times you tapped. . . . It was just a racket, not groups of taps at all." Further observations will not be quoted. They substantiate the first, but the conditions were not so good, since the *Os* now knew that they would be asked to tell the number of taps and tended to give some attention to them; so that we were never sure that the taps were not counted as they were given. We have, however, evidence that a high degree of attention is not necessary for the arousal of the memory after-image, though it seems probable that some attention is necessary since St, and some of the other *Os* at times, attended so well to the adding that they gave no attention to the tapping and as a result had no idea of the number of taps in the last group. We have pointed out that we do not know to what degree of attention Fechner refers. If for him focal attention is a condition of the appearance of the memory after-image our results are not in accord with his. On the other hand we cannot agree with Exner[9] (within the limits of our experiment) that "this primary memory image is, whether attention has been turned to the impression or not, an extremely lively one."

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[6] *Ibid.*, 493.

[7] S. Exner, Hermann's *Handb. d. Physiol.*, II, 1879, 281f.

[8] A. H. Daniels, this JOURNAL, 6, 1895, 558ff.

Fechner's second criterion states that one must take the attitude of actively calling up the memory after-image as one does the memory-image. It will not come with the purely receptive attitude which suffices for the ordinary after-image. Our results do not fully agree with this condition. It may be remembered that all of the *Os* reported the occurrence of the memory after-image spontaneously. It is true, of course, that they were "set" from the first to observe any mental processes present during the specified interval. That they were not, however, actively searching for an image is evidenced in the following reports. "The image seems to happen. I sit here perfectly helpless and it just comes" (Su). "The last word of a group of words seems to stick in my mind and to reëcho at the same time that you're talking" (De). "When I stopped the rest of the sentence sounded vividly in mind" (F). "I could hear the last three words just as you said 'Stop' repeated in auditory imagery" (Di). "I'm not making any effort. I hear the words after you've stopped" (St). In later reports on the memory-image Su said: "I can stop this image (memory-image) anywhere and have it go on again, but when I am taking dictation (memory after-image) I can't; I usually can't help myself but listen to it finish." De reported that "these images (memory after-images) seem to persist from the perception. I can get them without verbal-motor imagery which I have to use to get the memory-image." The same sort of experience occurred in the case of Di. For F the memory-images were always "voluntary" while the memory after-images were not. We cannot say, of course, that our *Os* were purely receptive, since they were set to observe the auditory memory after-image, but all of them had actively to try to get a memory-image (as Fechner asserts), whereas the memory after-image came with only the intention to observe it. Considering the memory after-image from the point of view of our results with respect to the above criteria it seems to be more like the after-image (if there were one in audition) than the memory-image. But we have pointed out that Fechner says that for many people the "Erinnerungsnachbild" seems like the after-image. His criteria of differentiation seem to have been based on his own experience of the memory after-image, which was for him more like the memory-image.

The two remaining criteria deal with the difference between the after-image and the memory after-image and are applicable only to visual phenomena.

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[9] Exner, *op. cit.*, quoted by James, *Prin. of Psychol.*, I, 1905, 646.

There is one more aspect of the memory after-image described by Fechner with which we have concerned ourselves. He says that, when the memory after-image was dying out or losing its clearness, he could often call it back by "eine neue Anstrengung".[10] This revived image was less clear than the original. We asked our *Os* to call back the auditory image after the memory after-image had gone. This was done by all *Os* only with effort. For all *Os* the revived image was initiated or accompanied by some other kind of imagery, most frequently verbal-motor, though with some *Os* visual. The auditory component was described by one *O* as "only over-lying" the verbal-motor imagery, and this was the kind of experience that all *Os* seem to have had with the possible exception of Su who, after initiating the auditory imagery by means of verbal-motor imagery, sometimes got the auditory component to "go along alone." These revived images were for all *Os* weaker in intensity than the perception. For three *Os* (Su, De, St) the duration was the same, though for Di the image "dragged," and for F the duration was as long as he "wanted to make it." In all cases the pitch was the same as that of the perception. The richness was less for De, Di, and F, and the same as that of the perception for Su and St. Our *Os* could not, then, call back an image at all comparable with the first or true memory after-image. The recurrence always took on the full characteristics of a memory-image. There is no justification for calling it a memory after-image. Fechner himself noted that the revived images were always weaker and less distinct. Without doubt the fact that we have worked in the auditory field has put a premium on the use of verbal-motor imagery, for which there would be much less, if any, use under similar conditions in the visual field. It is interesting to observe, however, that all *Os* got excellent auditory memory after-images with rare reports of any verbal-motor accompaniment, while none got auditory memory-images without the aid of verbal-motor imagery.

L. J. Martin[11] has also described experiments dealing with the memory after-image. She was interested in the relation which exists between the memory-images described by her with the use of the projection method and Fechner's "Erinnerungsnachbild." She, too, worked in the visual field. A sample report from one of her *Os* will indicate the nature of her results.

> "When I looked out of the window and closed my eyes, a light flashed before me brighter than the light outside. In this I saw only the tops of certain trees in the scene. They are of a bright color in reality, but the flash of light made them even brighter. This flash was instantaneous, and as it disappeared I became aware of the memory-image of the scene, which

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[10] Fechner, *op. cit.*, 493.

[11] *Op. cit.*, 358f.

I feel I have had all the time, but which the flash of light had obscured. The memory image was clear, steady and accurate in detail . . . . On closing my eyes later, I could bring back the second image as it was by an effort of will, but the first image I found it impossible to bring back as it was. . . . . ."

This and other similar reports lead Martin to conclude that Fechner's memory after-image does not exist as an unique phenomenon, but that it may be considered a combination of the after-image (positive or negative) and the memory-image. "Denn wie die Resultate beweisen, findet sich in Verbindung mit visuellen Vorstellungsbildern meist nichts, was selbst Fechner als ein Erinnerungsnachbild bezeichnen könnte, wo aber derartiges zu beobachten ist, da erscheint es keineswegs als psychologische Einheit, sondern als ein Zusammenwirken von Spuren, die ihrerseits zwei verschiedene Arten von Bildern: Nachbild und Gedächtnisbild unterscheiden lassen."[12]

If, however, there is no positive after-image in audition analogous to that in vision, then, according to Martin's view of the memory after-image, we have not been dealing in this experiment with memory after-images at all. Yet we have been concerned with experiences very like those described by Fechner, Exner, and Daniels. In order to determine whether there was in the auditory field anything analogous to the disappearance of one image and the appearance or awareness of another, which led Martin to make her analysis, we asked our *Os* "to report on all that occurs from the time I say 'Stop' until I say 'Now.' " The interval was 10 sec. The same general procedure as used before was followed here. No one of our *Os* found that an auditory memory-image came spontaneously during the interval. The auditory memory after-image described here by all *Os* came, disappeared, and was followed by a blank interval.

St said: "The memory after-image finishes out. Then I hear the 'Stop' and then I think mostly of when you are going to say 'Now'. " F said: "I waited to see if the after-image would be repeated. It wasn't." De reported that there was nothing after the after-image disappeared and he "wondered what to do to fill in the time." Su got a second image, but said: "I think I call the images back after the first time. I have to remember that they are in your voice and I go along with them in verbal-motor imagery. There seems to be more effort." Martin's *O* reports that "both images came wholly involuntarily on closing the eyes."[13] Di also reported a second image, but like that of Su his was verbal-motor.

We have, then, nothing like Martin's two stages, granting even the possibility of a positive after-image. The image for our *Os* came and died out as we have shown in our description of its temporal course. After an interval, for two *Os* a second image came, but it was not there "all the time", and it did not come "wholly involuntarily." Certainly the memory after-image in audition does not submit to Martin's analysis. We must bear in mind, of course, the fact that she was writing only of visual images and indeed says: "Ueberdies besitzen wir noch keine experimentellen Daten, welche die Annahme, dass auch bei anderen als visuellen Vorstellungen ähnliche Phänomene auftreten, wahrscheinlich machten." [14]

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[12] *Ibid.*, 367.

[13] *Ibid.*, 359.

CONCLUSIONS

Our results show that in the auditory memory after-image we are dealing with a psychologically unique experience. We have assumed that there is no positive after-image in audition which would account for the experiences we have described. We have found that the auditory memory after-image differs considerably from the memory-image. It seems to partake of the characteristics of both the after-image (in vision) and the memory-image. Like an after-image (in vision) it is "just there," "persistent," and is localized outside of the head; like a memory-image it is less rich than the perception and for some *Os* less intense, though there are individual differences. Martin's analysis would in no case seem applicable to the auditory memory after-image, for we did not find a blending of after-image and memory-image, though we did have evidence of a memory-image coming *after an interval*, voluntarily, and in a different sense-department. If the "Erinnerungsnachbild" is to be denied existence as a "psychologische Einheit," our results seem to show that this can be done only by way of an auditory positive after-image (which the auditory memory after-image studied here closely resembles), and there is so far no evidence for such an after-image in audition.

In any classification of images the memory after-image belongs where its name implies, between the after-image and the memory-image. Whether it should be placed nearer the after-image or nearer the memory-image depends upon differences between the individual *Os*. This dependence upon individual *Os* seems to make it more like the memory-image, but its many sensory characteristics put it in the after-image group, so that the name "Erinnerungsnachbild" or memory after-image seems well chosen.

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[14] *Ibid.*, 367.

A PECULIAR TRANSFORMATION OF PERSONALITY DUE TO *ENCEPHALITIS LETHARGICA*[1]

By Donald McNeil

Transformations of personality are always interesting. A number have been noted due to psychogenic factors, hysteria, etc. The transformation of personality that comes after an epileptic seizure and lasts for a few minutes or a few days is a well known phenomenon. A transformation of personality due to a specific disease of the grey matter of the brain, and therefore clearly based upon organic factors, is such an interesting phenomenon that it is well worth studying in detail and reporting. It was thought, therefore, that the meager information we first had about the change of disposition manifested by one of our cases in the clinic was worth more thorough investigation. The resulting study seems so interesting that it is determined to publish the record.

To bring out the items of this transformation more clearly, we thought it advisable to make use of some definite method. The method followed in this study was that suggested by F. L. Wells, described in the following paragraph, and modified to meet the special needs of this experiment.

> "The manner of presentation is to enumerate under the title of each division, first, the topics that are arranged under it to form a part of the present system. They are not formulated in the simple names of qualities, as in the schemata of Cattell or Davenport, but in the form of questions, similar to the Heymans-Wiersma and the Hoch-Amsden; *e.g.*, how well does he keep his word, how sociable is he. This often gives us a sharper definition than is possible through single names of qualities. . . . . Immediately following these are quoted under each section the topics that best fit under it from (1) the revised scheme of Hoch-Amsden (those of a supplementary nature only), (2) the list of Heymans and Wiersma, (3) the series arranged by Professor Cattell, (4) selections from the 'Trait-Book' of Davenport . . . There is added a brief discussion of each division, more closely defining the topics of the present schema, and the relation of each section to the others and the entire series."[2]

The method of notation we have used is similar to that recommended by Wells. The excess of the attribute (that is, above the average) is indicated by an asterisk, a deficiency by a minus-sign. Where the excess or deficiency is very prominent, an exclamation mark follows the asterisk or the minus-sign; and where the excess or deficiency is but little above the average, or where there is some doubt, a question mark follows the sign. These signs, as will be noted, have been grouped under the three headings, BI, AI, and AT, meaning respectively before illness, after illness and after treatment.

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[1] From the Psychological Laboratory of The Catholic University of America.

[2] *Psychological Review*, 1914, 21, 298-299.

The treatment herein referred to consisted in a low protein diet with intramuscular injections of tethelin, a drug supposed to contain the active principle of the anterior lobe of the pituitary gland.[3] Though we noted the condition of the patient both after his illness and after treatment, we are by no means positive that the improvement noted was due to the treatment rather than the natural healing process of time. It would seem, however, that the patient acquired better control after a relatively short stay in the hospital; and although his friends from the union thought originally that he should be sent again to an asylum, after his stay in the hospital he was able to go back to work and resume his trade as a plumber.

After each question concerning the trait we have not only indicated its presence or absence in excess or defect, but have also attempted by brief remarks placed at the right to indicate little incidents which lead us to believe that the trait in question was present or absent.

Owing to the extreme difficulty of obtaining satisfactory answers to all the questions suggested by Wells, some of them have been purposely omitted. In general, however, the questions employed are in close conformity with the arrangements suggested by his scheme.

History of the Case

The subject of this study was born October 15, 1896. As will be seen by consulting the tables, the patient, when a child, was of a quiet, reserved disposition. He did not indulge in games or athletic sports to the extent that the average boy does. Probably one of the reasons for this was the fact that he was flatfooted and afflicted with a certain stiffness of the knees which interfered, to some extent, with his motor activities.

On February 15, 1921, the patient was admitted to one of the city hospitals suffering from a condition later diagnosed as *encephalitis lethargica*. The illness started with a pain in the right side of the abdomen and disappeared in a short time. It reappeared, however, on the left side and finally became centered in the head. A week later the patient saw double and began to get drowsy. During his sleep he was very noisy, shrieking and crying out as if in great distress. For the first five days of his stay at the hospital he experienced frontal headaches for about two hours a day. During these attacks he had strabismus, the eyes slowly regaining their normal position after the attack.

After twenty days of almost continuous sleeping the drowsiness began to disappear, and a short time afterwards the patient was discharged from the hospital.

A decided change was then noted in the disposition of the patient. His manner and outlook on life underwent a complete transformation. In spite of loss of appetite, the patient gained thirty-two pounds in four weeks, this being followed by a short period of no gain. This was followed by another increase of thirty-three pounds in the next five weeks.

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[3] T. B. Robertson, *Journal of Biological Chemistry*, 1916, 24, 409.

Several months later the patient was admitted to a sanitarium for mental disorders where he remained for a few weeks, finally escaping and returning home.

He first visited Providence Hospital Dispensary on January 4, 1922. The following were his complaints: vomiting, loss of sexual power, abnormal increase in weight, indigestion and a tendency to fall asleep. The patient came to the clinic accompanied by two members of his trade union, who told me that although previously he had been efficient, after his illness he was discharged from several jobs, sometimes for flirting with girls in the building, sometimes because he had bungled his work, ruined costly material, etc.

An x-ray examination revealed the following: sella turcica small and of the closed-in type, with no evidence of bone disease. There were definite shadows in the sellar space, due probably to calcification of the gland.

The family history, outside of the fact that the father was alcoholic, was negative.

DIAGNOSIS

The case was diagnosed as *dystrophia adiposa genitalis*. This was probably the result of the previous *encephalitis lethargica*.

At the direction of Dr. T. V. Moore, of the staff of Providence Hospital, the patient was admitted to that institution to undergo treatment and to furnish an opportunity to make a thorough examination of his case both mental and physical.

I. INTELLIGENCE

Question	BI	AI	AT	*Remarks*
How easily does he learn?	?*	?*	?*	Patient when a boy was a better mathematician than the average of his class.
Is he a good observer?	?*	?*	?*	
What fund of information relative to educational opportunities?	!.	*!	*!	An oral examination was given the patient in general information. The knowledge he had of geographical and historical facts was astounding, considering his limited school experience. He was also conversant with current events. An examination after cessation of treatment by the army trade-tests for plumber, gave him the rating of a middle-grade apprentice.
How good a memory?	*	*	*	Remembers minutiae in events of which the average person has no recollection.

Is his advice sought by others?				Never was in a position where anyone would seek his advice.
Does he plan with good foresight	–?	–?	–?	

2. Output of Energy

How much motor activity?	–	–	*?	Flat feet and stiffness of the knees hindered his movements when a boy. Since treatment he seems to move around more than usual.
How talkative?	–!	*!	*!	As a boy the patient was more reserved and less talkative than the average boy. Now he talks incessantly when he has someone to listen to him. Friends of his have heard him talk vehemently when alone as if disputing with someone.
How conventional?	*!	–!	–	Since his illness he does the most unconventional things. On one occasion he knelt before a young woman and with affectionate gestures asked her to accompany him to the theatre.
How skilful with tools?	*	–	*?	Before his illness he was a very skilful plumber. After his illness he bungled several jobs and was finally dismissed. After being treated at the hospital for a while, he performed several small jobs successfully.
What degree of bodily dexterity and grace?	–	–	–	The patient was always awkward in his movements and since his illness this awkwardness has been increased remarkably.

3. Self Assertion

How much effort to shape surroundings?	–	*?	*?	Used to assume a passive attitude. Is now more critical.
How independent of the opinions of others?	–	*	*	Patient is not susceptible to advice now, even to such as is to his best interests.
How much tendency to assume leadership?	–	–	–	Never has shown any such tendency.
How ambitious in material things?	*?	–	–?	Ambitious to save money before illness. Now he spends extravagantly.

How able to bear up under difficulties and misfortunes?	*	*?	*?	After illness he felt sure there was something wrong in the attitude of his friends. His excessive talking may be an outcome of this. He thinks that it will cover up his deficiencies.
How able to face a crisis?	*?			Before illness he had fair presence of mind; since his illness no opportunity has presented itself to note how he would behave in a crisis.
What inclination to face danger?	–	–	–	Has always kept away from sports and amusements which were dangerous.

4. ADAPTABILITY

When a child how did he get along with other children?	*			Very quiet. Not aggressive. Was fairly well liked but never a leader in boyish sports.
How does he get along with older people?	*	–!	–	Was very good to his parents before illness and was liked by older people. Since his illness, however, his parents have trouble with him.
Is he tactful?	*	–	–	Now has absolutely no tact.
Is he offensive?	–	*!	*!	Before illness he was most inoffensive; since his illness he is the reverse. Now he is continually intruding where he is not wanted and says things that give offence though he does not mean to be offensive. He makes advances to every girl or woman that he meets. On one occasion he was arrested for annoying a girl on the street.
Is he quarrelsome?	–	–	–	Never had a fight in his life.
What tendency to be guided by advice?	*?	–!	–!	In the hospital he has to be watched carefully to see that he carries out the advice of his physicians.
How resourceful? Does he adapt himself to new environments?	–?	*	*	Is quite at home in the hospital; he does not seem to care particularly where he is situated.

5. MORAL SPHERE

How well does he keep a given word?	*	–!	–!	After his illness he promises anything with no intention of keeping his promise.

How truthful in matters relating to present or past?	*	–!	–!	After illness he told his parents stories about people which were not true. Now they cannot rely on his word at all.
Is he trustworthy in money matters?	*	*?	*?	As far as can be ascertained he still seems to be fairly honest.
Is he frank with opinions?	–?	*!	*!	Since his illness he is decidedly so. He is not the least bit backward in telling anyone what he thinks of them.
How conscientious in the performance of duty?	*!	–	–	Before illness he always told employer if he broke anything, but after illness tried to conceal these facts.
How discreetly careful of the reputation of others?	*	–	–	Not at all careful now. Says things about people that are not true.
How mindful of the equal rights of others?	*	–	–	On one occasion he fell asleep in a theatre with his head resting on a young lady's shoulder. This occurred after his illness. Because he was ejected for this action he wanted to sue the management, asserting that they had no right to complain of his conduct.

6. General Habits of Work

How prompt in reactions to situations?	*?			No situations have arisen in which such reactions could be noted.
How systematic in work?	*	–!	–?	Was very systematic in work before illness; after illness would start a new job before the old one was finished. Shows improvement since treatment.
How persistent?	–?	*!	*!	As a boy when he wanted anything, and was told that he could not have it, he would forget about it; in the hospital, when he is told not to do a thing, he persists in asking permission to do it.
How punctual?	*!	–!	–?	After illness he would arrive late for work, would sleep on the job, etc. He was finally dismissed from service.

7. Recreative Activities

Sports requiring quick and continuous action.	–	–	–	
Less active sports.	–?	*?	*?	Likes to take long walks now.
Hunting and fishing.	–	–	–	
Camp life.	–	–	–	
Games of intellectual character.	–	–	–	
Gambling.	*!	*?	*?	Before illness was a heavy gambler and was fairly lucky. Since illness has not gambled very much, owing more to the lack of money than anything else.
Alcohol.	–!	–!	–!	Has never taken a drink of any intoxicant.
Tobacco.	*?	*!	*!	Smokes cigarettes; several packages a day since illness.
Other drugs.	–!	–!	–!	
Reading.	*?	*?	*?	Reads some light literature.
Music.	*?	*?	*?	Enjoys listening to popular music. Does not play any musical instrument.
Pictures (moving).	*	*!	*	
Artistic creations.	–	–	–	
Delicacies in eating and drinking.	*	*!	*!	Very fond of ice-cream. After illness used to eat ten plates a day.
Sports involving physical danger.	–	–	–	Has never indulged in these sports, because he was afraid that he might get injured.

8. General Cast of Mood

Is he cheerful?	–	*?	*	At first the patient seemed despondent but now he is extremely cheerful. Although he is anxious to get better his illness does not make him despondent.
Does he get despondent without apparent reason?	*?	–	–	
Does he enjoy his discomforts?	–	–	–	

Has his mood been changed by any occurrence or circumstance?				His mood has been changed completely in many respects by his illness.
Did he have tantrums as a child?	*?			
Is he easily consoled?	*	*?	*?	Requires greater consolations now than formerly.

9. Attitude Towards Self

Is he self-conscious?	*!	–!	–	Before illness was very self-conscious. After illness casually addressed three hundred people at a union meeting.
Is he conceited?	–!	*!	*!	The patient now has the idea that every girl should fall in love with him.
Is he self-satisfied?	*?	–?	–?	Patient wants to lose weight. He thinks his appearance will improve with loss of weight.
Is he concerned for the present or future?	*	*?	*?	
Is he inclined to pay much attention to his aches and pains?	–!	*!	*!	He wants to tell everyone the history of his illness. Before his illness he would never complain, no matter how grievous his affliction.
Is he patient?	*	*?	*?	Becomes impatient with his mother when she does not have his meals ready when he wants them.
Does he talk of self?	–	*!	*!	Will talk about himself all day if anyone will listen to him.
Is he sympathetic?	*!	*?	*?	Used to be very sympathetic before illness. Now the question of self seems to interfere with his being sympathetic.

10. Attitude Towards Others

Is he generous?	*!	*	*	Asked cousin who visited him at hospital for money. When she admitted not having any, he wanted to give her what little he had. Was the support of his parents before illness.
Is he jealous?	–	–	–	
Is he sensitive?	*!	–	–	It was very easy to hurt his feelings before his illness. Nothing seems to hurt them now.

Is he forgiving?	–?	*	*	Forgave his girl for going back on him even though he knew she thought him crazy.
Is he grateful?	*!	*?	*?	At times he shows gratitude, other times the very opposite.
Is he considerate?	*!	–	–	Before illness would never say anything that might cause his mother to worry; now he does not seem to consider her feelings at all.
Is he selfish?	–	–?	–?	Now has a greater desire for his own comfort to the discomfort of someone else.
Does he keep his friends long?	*!	–?	–?	Had friends for over fifteen years. But since his illness the majority of them will have nothing to do with him.
Is he fault-finding?	–	–?	–?	When he goes home from the hospital for a visit he complains of everything which does not meet with his approval.
Is he suspicious?	–?	*!	*	Since illness is rather suspicious. Suspects people of making fun of him; also thinks that certain persons are plotting against him.

II. Reaction to Attitude Towards Self and Others

How scrupulous of personal appearance?	*	–	–	Was an extremely neat dresser; now, especially around hospital, does not care in the least about his appearance.
How sociable?	–	*!	*!	Will make friends with anyone he can.
How socially forward?	–!	*!	*!	Since illness thrusts himself upon people and does not realize when he is not wanted.
How demonstrative of emotion?	*	*!	*!	Likes to fondle children, especially since his illness. Will demonstrate affection for anyone who will let him.
What tendency to unburden?	–!	*!	*!	Patient after illness would tell absolute strangers all about his troubles.
How great a demand for sympathy?	–	*	*	Wants to have people sorry for him.

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How much pleasure in the enjoyment of success of others?	*?	*?	*?	
How much given to witticisms?	–	–	–	
How even-natured?	*	*	*	Never seems to get angry or become nervously excited.
Has he a different manner towards superiors and inferiors?	*	*	*	Is very respectful to superiors, but tries to boss those who he thinks are under him. Likes to talk in a loud commanding tone to the elevator boy at the hospital.
Is he more frank to certain people.	*?	*	*	Will admit certain things to one person which he will absolutely deny to another.
Is he open?	–!	*	*	Will tell the most intimate things about himself with the utmost unconcern.
Is he secret?	*!	–	–	
Is he forward?	–!	*!	*!	Very forward now; will make advances to girls on the street or elsewhere, even though he does not know them. Nurses at the hospital had much trouble with him on this account.
Is he bashful?	*?	–!	–!	Will do and say things since his sickness that he wouldn't even think of before.
Is he dull?	–	*?	*?	

12. Position Toward Reality

What capacity to take things as they are?	*	*	*	
Is he practical?	*	–	–?	Patient wants to get married although he is not working and has no prospects of a job.
What capacity to acknowledge mistakes or transgressions?	*	–	–	Patient is on a diet at the hospital, but ate outside at other places. When questioned he denied having eaten anything he was forbidden to eat, but finally admitted that he had.
Is there any tendency towards day-dreaming?	*?	*?	*?	Given to imagining how things would be if he were married and had a home of his own.

How influenced in actions by likes and dislikes?	–	*	*	

13. Sexual Sphere

How forward toward the opposite sex?	–!	*!	*!	Before illness went with but one girl and wanted to marry her; now he wants to make love to every girl he sees.
Is his personal attitude in harmony with his own sex?	*	*	*	Patient is not effeminate.
Is he attracted by older persons of the opposite sex?	–	*!	*!	
When love affair was broken off what was the reason?				Had only one love affair. This was broken off by the young lady because she thought him insane.
Was he decided when question of marriage came up?	*	*	*	Patient was and is still anxious to get married.
Is there a desire for children?	*?	*!	*!	Desire for children is very strong especially since patient knows that the likelihood of his having any is very doubtful.
Is he flirtatious?	–?	*!	*!	
Does he have sexual trends in conversation, art, etc.?	–?	*?	*?	Books and magazines containing questionable stories have a certain amount of attraction for him.
Did he ever practise masturbation?	–	*?	*?	Patient denies the practice but admits having allowed "tactus ex parte puellarum."
Is he prudish?	–	–	–	
Is he content with existing sexual adjustments?	*!	–!	–!	Does not understand why the opposite sex should not be strongly attracted to him. Does not wish to stay single.

14. Balancing Factors

Is he firm in religious belief?	*	*	*	Patient seems to live up to his religious beliefs.
Is he active in church work?	*	–	–	
Is he superstitious?	–	–	–	

Is he religious?	*	*	*	
Is his conduct consistent?	*	*?	*?	He does things which formerly he would think not just the proper thing to do.
Has he great schemes?	–	–	–	

SUMMARY

From a study of the above tables we may make the following summary:

(1) *Intelligence.* As the study revealed, the patient, considering his educational opportunities, was of more than ordinary intelligence. Although dull and stupid during, and for a time after, the attack of *encephalitis lethargica,* he seems to have recovered to a large extent. A number of questions intended to test his knowledge of current events was asked him, his answers exhibiting a fair amount of information about the recent war, politics, etc. The uniform ability he showed throughout the test to deal with questions of this character seems to justify the conclusion that his illness has not left his intelligence seriously impaired.

(2) *Output of Energy.* As a boy, the patient showed but slight inclination to exert himself physically. Unlike most children of the adolescent and pre-adolescent period, he indulged to but a very small extent in the ordinary activities peculiar to that age. This was probably due to the physical defects from which he suffered (flat feet and stiffness of the knees), although he did not even participate in such activities as fishing, camping, hunting, etc., which are usually a part of every normal boy's life. This peculiarity is all the more noticeable since the patient lived near a large river that afforded good opportunities for such sports.

In his youth, the patient was more than normally quiet; now he is extremely talkative. This change, which he himself appreciates, is one of the most profound noted. However, he seems to be improving with the tethelin treatment; but this may be due, of course, to the change in environment.

Though he was discharged from several jobs after his illness because of inefficiency, he is now so far improved that recently he was able to perform several pieces of work for the hospital authorities with success. He has since been working regularly at his trade of plumbing. The probabilities, therefore, are that his dexterity as a workman, which suffered deterioration during his illness, may eventually be fully restored.

(3) *Self Assertion.* Under this head will be noted a very profound change; it represents a radical change of attitude and disposition on the part of the patient from that exhibited previous to his illness.

Before his *encephalitis lethargica* the subject was remarkably quiet, thoughtful and thrifty. These were leading traits in his character, his shyness and bashfulness being especially pronounced. Since his illness the patient seems to have assumed a new personality. Now he is very loquacious, heedless and extravagant. One explanation for his excessive talkativeness might be found in his desire to obscure and forget his past difficulties and misfortune. It might be due also to a new confidence that the patient has in himself. On the other hand, though he talks a great deal about what he is going to do, he remained for some time very inert. While at the hospital he made but feeble efforts to change his surroundings or improve his condition. In this respect he is very careless. Since his illness, however, he is very self-assertive and tenacious in his views. These fundamental changes in his disposition are especially apparent to friends intimately acquainted with him before his illness. Many of them on this account do not esteem him as much now as formerly, and for the same reason he has lost the regard of many of them. At the present time, however, he gives a slight indication of becoming his former self.

(4) *Adaptability.* Previous to his illness, the patient, because of his mildness and thoughtfulness, was liked by young and old. Now, however, owing to his change of disposition, he is generally disliked or simply tolerated by older people with whom he comes in contact. The chief traits in his changed manner that seem to be responsible for this circumstance are lack of tact and undue inclination to talk about his own ailments. While he is not quarrelsome, he will nevertheless adhere stubbornly to his own opinions, not hesitating even to argue with his superiors on matters with which he has no concern. Since beginning the tethelin treatment he seems to have improved somewhat in this respect, and it has been noticed that at least two patients of his ward show a liking for him.

(5) *Moral Sphere.* Several marked changes are noticeable in the life of the patient. Before the affliction he was uniformly conscientious, truthful and discreet; after his illness these characteristics were no longer present. Although some improvement in this respect is apparent, the subject still lacks these elements of reliability which he exhibited formerly. It is possible, however, that some recovery in this direction may take place.

(6) *General Habits of Work.* A marked effect of the patient's illness was a radical change in his habits of work. Prior to his illness, the subject was a competent, thorough and reliable workman. After his recovery he no longer possessed the same punctuality and systematic habits of procedure. He became very untrustworthy and would abandon a job before its com-

pletion to begin something new. He would frequently be late for work and, when the opportunity came, would sleep during working hours. For these things he was finally dismissed from the service of his employer. A change for the better appeared after a course of treatment with tethelin. The patient was given several jobs to do at the hospital, all of which he completed very satisfactorily, and is now employed regularly at his trade of plumbing.

(7) *Recreative Activities.* Under this heading several marked changes are noted. A slight change toward more active interest in the less active sports appears. This may be due to the fact that the patient has been confined for some time; and consequently the less active sports, such as automobile driving and walking, appeal to him more than they otherwise would.

A slight increase in the number of cigarettes smoked per day is also noted. This may be accounted for by the fact that a person when occupied will not smoke as much as when idle; the patient being idle will naturally smoke more than was his custom.

The abnormal fondness for ice-cream is a probable result of the disease. He was always very temperate in his habits and tastes. He never used alcohol or other drugs, and for diversion has shown a preference for games of the less intellectual character.

(8) *General Cast of Mood.* A change in the mood of the patient due to his illness is very discernible, although the variation of this trait from the normal is not so decided as it is for some of the others studied.

So far as can be ascertained, the patient during his boyhood and youth was very quiet and moody, bordering even on the melancholic. He rarely showed any exuberance of spirit. One would expect, therefore, that under the present circumstances this condition would manifest itself in more aggravated form. Nevertheless, in his whole illness he has never been despondent. He takes things lightly and shows no disposition to brood over his difficulties.

This peculiarity, exhibited by the subject, of aspiring to situations beyond his means of realization would be interpreted by some psychologists as indicating a disordered personality. In an article contributed to the *Psychological Review* by a student of psychology in 1914, the following conditions are laid down as the essentials of a perfectly balanced personality:

> "Among the essentials of a perfectly balanced personality are that its acts shall accord with experience rather than with a contrary feeling, that it shall react to situations objectively rather than with a contrary feeling, that its aspirations shall be within and in accord with its capabilities, that its power of judgment shall not be impaired in crises of danger, or the incidence of good or evil fortune, and that there shall be no conflict between

morals and conduct. . . . . In the ill balanced personality we find essential disharmonies between the affective reactions and those of the proper objective value."

Applying this norm to the case in hand, it is apparent that the aspirations are not in accord with the capabilities: the patient is desirous of getting married when his condition precludes the possibility of his doing so; his power of judgment is defective; and, considering his moral ideals, his conduct on some occasions would seem to contradict his better moral views. Accepting the principle enunciated above as correct, we are led to the conclusion that the personality of the patient is ill-balanced. Further force is lent to this view by the fact that the patient, though he realizes his condition, shows no tendency to be depressed or despondent.

(9) *Attitude towards Self.* The effect of the illness on the patient is conspicuously shown by his changed attitude towards himself. Before he became sick, the subject was unassuming, modest and very reticent in matters relating to himself. In personal matters he would confide but little even to his parents. Now he seems to take a delight in telling everyone about his sickness and about his broken love-affair. Since his illness he is known to have addressed a large audience apparently without experiencing any embarrassment. Such action is entirely out of keeping with his former conduct. These instances illustrate the breaking down of former inhibitions. While it might appear that his abnormal actions are due to an excessive degree of self-confidence, it seems more correct to attribute them to an immoderate amount of self-esteem or an enhanced opinion of his own capabilities. It is very evident that he thinks he is much more gifted than he really is.

As regards his feelings for other people, the patient is not nearly so sympathetic as he used to be. Whereas before his illness this was a leading trait in his disposition, his attitude now in matters relating to self is one of marked egotism. Likewise his former modesty has given place to extreme forwardness. He thinks that he possesses an attractive personality and that he is looked upon with favor by members of the female sex. He is not so loquacious as he used to be, neither is his inclination to make advances to the opposite sex as noticeable as before treatment.

(10) *Attitude towards Others.* Before his sickness the patient stood high in the estimation of his friends. He was easy to get along with and was considerate and unselfish. He was especially mindful of the feelings of his parents, being careful never to cause his mother any worry or annoyance. But after his illness his actions caused her a great deal of anxiety and distress. A peculiar result of his illness was the idea he conceived

that certain people were conspiring to do him harm. Especially was he obsessed by the belief that those people were trying to bring about an estrangement between himself and his sweetheart and ultimately procure his downfall. This caused him to say things to his friends that were the cause of his losing their friendship. For the same reason he failed to cultivate new associates.

This characteristic attitude of distrust has recently disappeared to some extent. He perceives now that people are trying to aid rather than injure him. He realizes also that he himself was the cause for the disaffection of his lady friend.

As regards the transformation in the personality of the patient, it is accepted as characteristic of an unbalanced personality that it suspects where it should trust and trusts where it should suspect. By the manner in which the patient treats his friends of late it might be inferred that a better adjustment in this respect is gradually taking place. The patient now is more prudent in placing trust where it should be placed, and his suspicions are not so unduly acute.

(11) *Reaction to Attitude toward Self and Others.* In this division of the study many changes in the mental life of the subject were revealed. From being bashful and altogether undemonstrative before his illness the patient developed opposite traits equally conspicuous. Scrupulous as he was of personal appearance prior to his illness, he became lax in the matter after his recovery. Recently, however, he has corrected this abnormality to some extent. He gives evidence now of a desire to look well, this being shown by his anxiety to reduce in weight. He thinks that a slight figure will add to his appearance. Likewise, now that he realizes his forwardness, he shows a desire to improve this condition. Although he is still rather forward, a definite improvement is shown. If we interpret this forwardness as being a device employed by the patient to obscure the handicap from which he suffers, we have a means of explaining why this condition has persisted so long.

Although the disposition of the patient has changed in many important respects, basically he is, as formerly, even-natured. He likes to give orders to those whom he thinks inferior to him, showing in this respect a departure from his former non-aggressive attitude, but he does so with no quarrelsome intention.

(12) *Position toward Reality.* Before his sickness the patient was essentially practical. He was a practical man with a practical outlook on life, and his conduct was consistent with his position socially. Morally, he was upright and honest. If he made a mistake he would readily acknowledge it, and he would feel grieved if he transgressed. Neither would he allow his prejudice to affect his better judgment.

His conduct since his illness is not consistent with his earlier record. He is not honest with himself or others and cannot be trusted as before. Though he expresses anxiety to get well, he will not obey instructions if the occasion arises for him to disobey them. At the same time it is difficult to get him to admit that he has broken the regulations. In all there seems to be a certain lack of restraint; for, in some cases at least, he seems to be conscious of the fact that he is doing wrong. In other cases, however, it is evident that his judgment is somewhat impaired. He would, for instance, have married, had his fiancée consented to the ceremony, though at the time he had no means of earning a livelihood and his prospects for the future were poor.

Recently, however, he has developed a more normal condition of mind. One fact that will probably hasten his return to a normal condition is that he is keenly aware of, and sensitive to, his deficiencies. Though he is still unreliable and indiscreet, he seems at the present time to have a will to do what is right.

(13) *Sexual Sphere.* The transformations that occurred in the individual in relation to sexual inclinations are very pronounced. They surpassed all other changes that took place in the intellectual and emotional life.

Prior to his illness, the patient had kept company with but one girl. He loved her and would have married her. In the presence of others of the female sex he was very bashful and ill at ease. With the exception of this one young lady, he was not attracted by members of the opposite sex, and he would never attempt to speak to a young woman to whom he had not been properly introduced. He had no inclination to be flirtatious, differing in this respect from his present abnormal proclivity for females. He has no recollection of ever having practised masturbation as a boy, though he admits that since his illness he has, at least on one occasion, experimented with himself while taking a bath. While he denied secret practices of this kind, he admitted that he had allowed "tactus ex parte puellarum." Since his illness he has been functionally impotent.

(14) *Balancing Factors.* The patient before his illness was very active in church work; and although he does not take a corresponding part in work of this kind since his illness, he is still interested in it. In the matter of attendance at church rites and exercises his conduct is superior to some normal persons. He does not seem to be the least bit superstitious and, so far as can be learned, has never been.

Conclusions from the Study of the Personality

The following is a brief summary of the characteristic changes noted in the patient. They are arranged in two groups. One group gives the traits that have appeared since the *encephalitis lethargica*; the other, those which have disappeared.

Traits appearing since encephalitis lethargica.

Talkativeness
Frankness with opinions
Persistency in trying to get what he wants
Cheerfulness
Conceitedness
Tendency to talk of self
Sociableness
Tendency to thrust himself on others
Tendency to unburden
Tendency to talk of his intimate affairs
Forwardness
Forwardness towards opposite sex
Interest in opposite sex
Flirtatiousness

Traits disappearing since encephalitis lethargica.

Conventionality
Skill with tools
Ability to get along with older people
Tactfulness
Integrity
Trustfulness
Truthfulness
Carefulness for reputation of others
Mindfulness of rights of others
Systematic method of work
Punctuality
Tendency to despondency
Self-consciousness
Sensitiveness
Considerativeness
Tendency to keep friends
Carefulness of personal appearance
Secretiveness
Bashfulness
Contentedness with existing sexual adjustments

Careful study of the above summary gives us the following conclusion concerning the intellectual characteristics of the patient.

There is a paralysis or inhibition brought about by the loss of the power to perceive delicate relations of which the normal man is usually aware, and to take into consideration the possibility of more or less remote consequences.

The increased motor activity may be due also to the breaking down of inhibition, as in the case of acute alcoholism. The cheerfulness may be secondary to this.

Although many character-traits have been noticed as having undergone transformation, it is not impossible that all of these may be reduced to one and the same factor, namely, a paralysis of inhibitions. This paralysis of inhibitions is due to a great extent to the loss of intellectual insights into relations. The patient is not tactful because he does not see the relation of his conduct to ends that would be more readily perceived by a normal individual; he is forward and bold because he has lost due appreciation of the meaning of conduct. Those things that have been built up by education, that act as a restraining influence upon others, have been paralyzed. His behavior resembles very much that of a man slightly under the influence of alcohol. He is clumsy with his tools; he is awkward in his manner; he is talkative, he is cheerful; he has lost all feeling of shame and restraint; he comes late for his job and has no appreciation of what this may mean; he does not care; he has no bashfulness; he has none of the finer sensibilities. The loss of all of these things and the appearance of others does not mean that his *encephalitis lethargica* has produced a change in many attributes of character, but only in one, *i.e.*, control. This control demands, for its perfect exercise, the perfect functioning of a very elaborate cerebral mechanism. It is this cerebral mechanism that has been injured by the *encephalitis lethargica*; and, because of its injury, this peculiar transformation of character has taken place. Such an injury may happen in other ways, and frequently does appear as a transitory disturbance in alcoholism and epilepsy. Unfortunately, with *encephalitis lethargica,* the injury is more permanent. It is not likely that this patient's character will ever again return to what it was before his sickness.

SOME RELIGIOUS CULTS IN JAMAICA

By Martha Warren Beckwith, Vassar College

I. The Revivalists

In the year 1860, a great wave of spiritual excitement swept the island of Jamaica and resulted in a religious revival among the negroes. Its cause no one seems able to explain. Perhaps the impulse came from the United States in response to the high tension of the South on the eve of rebellion. Perhaps the antagonism of the more stable English and Creole population against the methods of the evangelistic orders had at that period reached the fighting point. Its result was the formation of religious groups who gathered about prophetic leaders and organized themselves into bodies of worshippers independent of the established churches on the island. The Revivalists, the Isaiahs, and the Bedwardites are examples of such organizations.

The sight of women going about the streets of Kingston wearing large white turbans, and in the country districts the distant sound of drumming at night were my first intimation of the presence of the Revivalist cult in Jamaica. From outside sources I gained certain facts about these Revivalists. 'They have no church building. They consist in a company of men and women who claim to be inspired by the spirit. The women (called *mesdames*) dress in white and carry wands. They march, sing and pray in the streets at night. They hold their service about a table with a white cloth on which they set a glass of water, flowers and a bible. They rock their bodies in singing and fall down in frenzy. Also they claim to possess cures for driving away sickness caused by the "duppy" (or ghost). They take money from the people for this service.' All this was hearsay; from the people themselves I got nothing. Accordingly, I took some pains to come into touch with individual members of the cult, to attend their meetings, and to learn from their own lips the story of their religious experience.

One night in the little village of Lacovia, in the remote country parish of St. Elizabeth on the south coast of the island, I followed the sound of drums to a revivalist meeting which was held in a booth set up in the bush some distance back from the village street. The worshippers were singing hymns, which began with a Moody and Sankey tune and continued in a dizzying iteration of syncopated rhythm to which they danced with a peculiar rocking motion, raising first one foot then the other, at the same time lifting the arms and letting the body sway from side to side. Wands held upright in the hand waved also with the beat of the singing.[1] The leader, a man named Granville, wore a uniform resembling that of the Salvation Army and carried a sword-like staff shaped out of wood. Near him stood the leading woman or "Mammy" of the company, a stout, good-looking married woman of thirty-five named Margaret Williston. She wore a white turban and bib-apron and a shoulder-sash of green marked with the figures 66 across the breast. Her wand was made of a lithe vine called "supple-jack," twisted into a ring at the top and tied in a knot. From time to time during the service she spun about slowly or bowed herself. Once, as the singing took on a wilder rhythm, she dropped into a hypnoidal condition which showed itself in a loud rhythmical breathing in sound like the bark of a dog. This is called "trooping" and denotes possession by a spirit. Sometimes it is prolonged until the devotee falls unconscious, but on this occasion Mrs. Williston had herself well in hand and in a few moments was offering a prayer with biblical phraseology. There followed more singing, prayers, testimonies to religious experience. Such a meeting as this, I was told, might last for hours, and the more emotion of the hysterical sort it aroused, the better satisfied were its leaders.

Some days later, Margaret Williston gave me the story of her religious experience, repeating it in a sing-song fashion as if custom had given it rote form. In writing it down here I have not altered her own words, although occasionally their order is changed because some of the facts came out in answer to questions.

> I was not a great student. I left school in the first book. And after that I got spiritual at twenty-five. The Mammy was from Westmoreland. When there was shouting, I felt that I was almost shut out of the gate of heaven and I closed my eyes and I said "Lord, if it is thy wish that I be separated, let it be thy will." And I dreamt that I saw an angel and he bids me get a Bible and a Sankey and a rod with a double fold and a lantern which I'm to light on my journey.
>
> I didn't know to pray until I got the Spirit, and the Spirit teaches me to pray and sends me on the high-ways and the hedges to bid others to come and to tell what a sweet Savior I found. Jesus is Savior in need and Savior indeed. There is not a friend like the loving Savior.
>
> The angel which I saw had a bunch of flowers in his hand, white for peace. He bid me get a large turban. He bid me get one larger, but I can't afford it—to make it five yards. He bid me make an apron in white. The sash of green with the number 66 means your shout. You stamp 66 times

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[1] H. Junod (*Les chants et les contes des Ba-Ronga de la baie de Delagoa*, Lausanne, 1897, 33) describes the common form of the dance among the Ba-Ronga as performed by soloists and chorus, the chorus forming a circle or half-circle about the soloist and singing the refrain, at the same time holding wands upright in one or both hands which they sway rhythmically to the music while marking time by tapping with the foot upon the ground.

with your foot. Some have a sash of other colors, some a sash without apron. All who carry the wand have a sash, but not all wear the apron. The turban is red or white. The red turban means that you don't feel to work.

Sometimes [at the meetings] as I am in the Spirit moving, the Bible drops and falls open at a chapter and I read and read. I feel to pray and there isn't any chance to pray at the time and I bow to squeeze the prayer-key. When you are in the Spirit, you feel that the world is going around and you turn without you want to turn. When the Spirit overflows within you, you have to shout and shout and shout. You have to shout with joy. When you feel that you can't shout with your voice any more—feel as if it were shut down and locked—then you cease shouting.

Sometimes when a brother or sister is going to depart [die] I feel a fullness within me, I have to speak up, I don't get any ease. But I don't know who it is. If I had the chance to know who the person was, I would go and kneel and make him prepared.

We call our leader "Mammy". She is about forty. She lives in Manchester. But I do not "work" with Mammy, I work for myself. We have not up to now received healing. Once water broke out about five miles away and the people travelled miles. We heal the people according to the Descender. As yet the Descender has not come [to the group at Lacovia] to teach healing.

A few evenings later, in the cock-pit country above Maggotty about fifteen miles from Lacovia, the Revivalists of that district happened to hold a meeting at the turn of the road directly opposite the house where I was staying. I could sit on the upper verandah and from my hill-top catch across the valley the words of exhortation and the reiterated lines of song chanted to the accompanying beat of the drum and the almost incessant bark of the trooping. Afterward, Alexander Edwards, the leader of the company, told me his story. He had been born and reared in the district, like his forbears back through old slave days. A near relative was the leading elder in the Scotch Presbyterian church which he himself attended regularly on Sunday morning. I have no reason to doubt the accuracy of his simple narrative.

I am the leader of the meeting, I and X—because he owns the drum. The leader of the meeting has the dream and he gives it to all the others. My dream was this. I saw angels with these rods in their hands, and they gave one to me. I was lying down in bed sleeping. Often the angel appeared to me. I found myself singing. I waked up and I was fervent in prayer. The song was Sankey, 338,—

What can wash away my stain?
Nothing but the blood of Jesus.
What can make me whole again?
Nothing but the blood of Jesus.

My spirit felt rejoicing. When the angel appeared, I felt the whole of my body light and rejoicing and I saw the light and from the shining of the light I awoke and began prayer. Everybody heard.

Then [another time] I found myself in school in my sleep and from schooling years I brought examination. And when I went to examination the second time, they told me that I was a student. That year I found examination again and they put a puzzle before me. And after I was able

> to see who had harmed him. The dead spirit came back. They had to call him off or he would have killed the man.
>
> One boy found that someone had meddled with his fish-pots. The Obi-man told him to call up his dead father, didn't warn him, and he went and handled the fish-pots. So when the dead father came, he hurt his own son. They had to get some one to call him off. The boy's name was Campbell at Red Hill.

Another old woman, Mercilla Hopkins at Brownstown, said of those who practise the art of sorcery in order to harm others:

> Before they die they must confess everything. When Mary Gulla was dying, all her friends went to see her. She told them they must stand from the door and let her sins come in. And they must look beneath the bed and see some pins and needles; they must bring them within view. So now in every wicked act she had a part she must confess. A little boy fell off his horse in the race-course and she confessed that she did it. But she couldn't see the Lord's face and she said her soul was lost.
>
> John Taylor confessed many wicked acts, said he killed many people. He was a hearty sound man [when he died].

I asked her how the sorcerer got into touch with the dead spirit. She said:

> The *myal* is what they do to compel the dead. They boil rice and put it for the dead and sing some funny sing. They [the dead] must come up and eat the rice. Then they catch the shadow of the duppy—can't catch the soul but catch the shadow. They can't catch the body—that rots; nor the spirit, for that goes up to God; but they can catch the shadow. They put it into a bottle and cork it up.

From these accounts it would appear that what the Revivalists are doing is to take over the demon-driving business of the *Obi*-men. Having got hold of a more powerful *Obi* than was known to the old enchanters, they are disposed to work it for the exorcising of all demons of evil abroad in the world. And no one can hear the beat of the drum and the rhythm of the Revivalist song after it has slipped the leash of its English cadence and become a repeated cry out of the heart of the supplicant; can see the rocking swaying bodies of the worshippers in the dim light, dizzying the senses; and listen to the sharp hysteric barking of those who are possessed by the Spirit; without feeling that here we are touching near the bed-rock of that emotional heart of the race upon which our own religion has reared its superstructure of Christian theology.

How close the Revivalist methods are to African beliefs and practices may be seen by comparing them with the reports of the best first-hand observers in the field. A. B. Ellis, the chief authority on the negroes of the West Coast, says that among the Tshi-speaking negroes of the Gold Coast, from which group the Jamaica negroes are largely drawn, dancing was a special branch in the education of priests and priestesses and was engaged in for the purpose of curing sickness. It was always accompanied by the drum. During the performance, the priest was supposed to be entered by a god; if he fell down, it was imagined that a second god had entered him and was engaged in a struggle with the first.[3] According to Weeks, a missionary in the West Congo, men and women parade at night to remove sickness.[4] Tremearne describes the highly specialized ghost dances practised among the Hausa in order to persuade the *Bori* or spirits to enter the dancers. Hysteria is induced as a sign of spirit possession and the person thus possessed is able to foretell the future.[5] Moreover, a similar color symbolism is observed. Ellis says that white is the proper color for the clothing of priests and priestesses at festivals of this character. According to Tremearne, ghosts of the dead are clothed in white. Among the *Bori*, each spirit is supposed to be attracted by a special color, just as among the Morocco Jinns color is a distinguishing character of the special spirit who enters a man.[6] The hysteria or trance is induced for purposes of healing. Klunziger mentions "a form of religious intoxication to which the Soudanese are particularly susceptible, in which dancers fell into ecstasy from the repetition of the name of Allah." This happens when they hold a dance at the home of a sick woman "under the direction of the *shecha* of the *sar* or Jinn who produces sichness." [7] A writer in the *Journal of American Folk-lore* for 1913 finds among the mountaineers of East Tennessee "a custom of those already converted to 'die' for their friends." This consists in "falling into a trance and remaining in such a condition for some hours (in one instance eight); the time of course varying with the hard-heartedness of the one for whom the exertion is made."[8] Compare this Christian sacrifice with Monk Lewis' account, in his *Journal* for 1816, of a *myal* performance in Jamaica, in which the sorcerer first pretends to kill and then restore to life again a man who has offered himself for the purpose.[9] "To show his power," says Lewis of the sorcerer's motive in presenting this spectacle of death and resuscitation; but there must be in the negro's mind a belief that the spirit of the man in trance goes out of him to war with the demons who are tormenting the sufferer. Margaret Williston and the other Mammies "work" in the same way. In symbolic fashion they "die" for those for whom they are struggling, whom they believe to be literally possessed of a devil.

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[3] Ellis, *op. cit.*, 121-126.

[4] *Folk-lore*, XX, 1909, 466.

[5] Tremearne, *op. cit.*, 20, 145-151, 424-426, etc.

[6] *Ibid.*, 142, 150; Ellis, *op. cit.*, 124, 222.

[7] K. B. Klunziger, *Upper Egypt*, New York, 1878, 394-396.

[8] *JAFL*, XXVI, 1913, 145-146.

[9] M. G. Lewis, *op. cit.*, 354-356.

II. The Isaiahs

The Baptist clergyman's wife at Brownstown, in the parish of St. Ann on the north coast of the island, directed me to a religious group living some eight miles out of town in a place called Tricey and designating themselves "Isaiahs" because of their leader's claim to direct inspiration from that prophet. The lady told me that, when her husband first came to Brownstown, he found the Isaiahs worshipping in the open, a rock as their altar, upon which they sacrificed a goat and performed other strange rites. This happened in the old days of Edward Alexander, the first Isaiah, who was converted "just after slave-time" (in 1838) and predicted the coming of the revival in 1860. His son James who succeeded him, the present Isaiah, was already an old man at the time of my visit. His small congregation met on Sunday morning in a neat church built after the English pattern. Except that some members of the choir carried wands of the Revivalist pattern, although with a simpler twist to the knot at the top, and rocked their bodies as they sang, the service differed in no way from that of any independent negro religious meeting. But within the churchyard stood an old tree and the historic stone slabs about which the earlier Isaiahs had worshipped. And in substance the story which old James Alexander told conformed closely to the Revivalist pattern. First came the conversion through vision, as if under direct control of the Spirit. Then the continued proof of inspiration in the power of fore-knowledge and of healing.[10] It was clear that the leader must, as Edwards said, first get the dream and then give it to others. His leadership depended upon his acting as a vehicle through which the will and the knowledge of God were imparted to the people. I give the story in his own words.

> It is exactly forty-one years since I was converted. I was called by the spirit of God. As I was called, I was down on my back in one place in the room eleven months. I couldn't come out at all, only lying in one place, the spirit taken away from me and I learning the mystery of God.
>
> Then one day I got Psalms 116. Then I rose and came outside the door and saw my two little boys there, and I stayed from eight in the morning until four o'clock. After that I went back to my bed and lay down. And after I lay down, in the night, I was taken away in the spirit to Brownstown. I didn't walk 'pon the earth; I flew in the air to Brownstown, I and the Master in the spirit. And when I got to Brownstown, I saw the Master. And he had a little book and he spread it upon the earth and I couldn't see. And he read out of it, "Parson Clark, your time is up on earth." And I saw a beautiful vessel waiting for him. And then he read and he read out of that book. I couldn't understand and he said, "No, indeed, for it is Latin and parable; but I will speak plain that you may understand." And when he spoke, I saw the foundation of the house move off. And he said to me, "It is finished with Parson Clark." And the foundation of Brownstown groaned.

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[10] Cf. J. M. Fletcher, *The Miracle Man of New Orleans*, this Journal, XXXIII, 1922, 113-120.

And when we rose, he brought me to the church in Tricey and we didn't come 'pon the earth. And we saw a number of children like sheep. And he said, "Now take charge of these sheep and mind them for me." And a number of these children lay down in one place and some were away off. And I said, "Some of these sheep are very trespass." And he said, "You desire to bind these trespass sheep with cords, and the cords are the word of God." And I said, "Sir, when it become dark, how shall I see to mind those sheep from the Lion and the Wolf—from the Devil and Satan?" And he burst out with a great light which passed to the four corners of the world. Then the Master departed and I saw him no more.

So I am bound by the spirit of God with these people. I take no money for these forty-one year, take no money from nobody. I go from one place to another—quite down to Macedonia; but no night work, all in the day, on Sunday. The band goes along with me and must be back before night.

The great earthquake that is past [in 1907], the message was given me to warn the people. And I warned them three weeks. And the Bible opened at Numbers 16—"To-morrow ye shall know who is the Lord."

And an old lady [an elder in the church], the last that is gone away, was in the church with me and the spirit said to her [through him], "Mother Hall, the angel comes for you." And she rose up and took the message. And the next Sunday she was not able to come out and she lived only two weeks. Old? Not too old.

My dear wife here, she was sick; vomited blood. I went to the field to work and fretted lest she die behind my back. One night I took a journey into a world I didn't know and I saw a great house before me. When I entered the door, I saw door, door, door, door, but I heard no voice of anyone. And I saw a lady coming with her hand up *this* way. And as she came to me she said, "Take this," and I received it. And she said, "Isaiah, take this and boil it, and go to the doctor's shop and buy a six-pence worth of paregoric. And after you boil it, measure how many wine-glasses the bottle contains. When you boil it, don't put it on your table, lay it under your bed-head and let Mrs. Alexander take a wine-glass in the morning and one in the night. And I say, this shall keep down the stomach from bleeding." And I did so. And just as the bottle ketch in the middle, she said, "Father, I don' want any more." And the blood stopped until this day. No more blood.

III. THE BEDWARDITES

Upon reaching Kingston in December 1920, I found the city in a state of half-amused, half-expectant excitement because a certain negro prophet named Alexander Bedward had predicted his own ascent into heaven, the destruction of the whites, and the reign of Bedwardism upon earth. These climactic events were to take place on the last day of December of that very year. The faithful were exhorted to gather into the village of August Town, at the foot of the Port Royal mountains near the river Mona, a few miles out of Kingston, where stood Bedward's church and where for thirty-two years had centered those religious activities of the prophet which had made the "healing stream" of the Mona river and the baptisms held in its life-giving waters celebrated all over the island and had gathered a large number of converts to his teaching. In response to the prophet's call, those living in other parts of the island were now rapidly selling out whatever of their property was not transferable and flocking to the central camp at August Town to be ready for the predicted destruction, which was to fall upon none, white or black, who stood within the sacred enclosure destined to become the center of the new Jerusalem.

In the city, the wildest rumors were afloat in regard to the prophet's real intention. It was confidently asserted that the man was a swindler and meant to steal away out of the island on the fateful day with the wealth which he had wrung from the credulity of the people. If a fire-balloon went up at night, the jest went round that this was Bedward ascending into Heaven. Even the intelligent whites believed that something out of the ordinary was about to happen. Among educated colored people I heard one reminding another wistfully of another Christmas day and of another prophet who was to the wise foolishness but whom the common folk had heard gladly. The negro entertainer at the Flamstead estate where I spent Christmas day, enumerating the marked events in his life said, "I remember the St. Thomas rebellion in [1865] and the Revival of 1860. It was taken up by the whole world. Now to-day there is Bedward!" The Kingston *Gleaner* wrote under large headlines:

> Yesterday there was great excitement at the Railway. Every train brought in a large number of people, men, women and children all bound for August Town. But they come from Colon as well, on a steamer, travelling across seas to see their 'Lord of August Town' ascend. In King Street all tram-cars travelling to Hope Gardens were besieged by men, women and children, some infants in arms, others hardly able to help themselves, but they were bundling in with with their clothes, baskets and fowls. All have sold out to come up and see the Lord and Master do the disappearing trick. There are still more to come.[11]

Many feared that the occasion would give pretext for a negro uprising against the whites.[12] One and all warned against visiting the prophet at August Town at this time. Nobody could tell what the negroes might do in their excited state.

When, on the morning of December 26, I walked over alone from the end of the Hope Garden tram-line by a foot-path to August Town, I found the churchyard crowded with campers, and the bed of the river—a mere trickling stream because of the drought—alive with folk. Women were washing clothes, rubbing them clean against a rock. Old men lay stretched on the bank asleep. Children went to and fro with water-cans for the camp, and donkeys ambled along panniered for the same purpose to the shout of "Donkey! donkey!" to clear the way. The guard at the gate who at first refused entrance to anyone un-ready to "bow before the Lord and Master" was easily pacified, and an escort was found to lead me to "the Lord." The prophet was at the moment exhorting the people from the verandah of his house. So great was the press about him that it was ten or fifteen minutes before I could gain an interview. Meanwhile the folk scarcely observed my presence. On their faces was one fixed and consuming desire—to see their Lord, to hear him speak. "Lay hold on the Lord's word," was their assurance.

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[11] *The Gleaner* (Kingston), December 29, 1920.

[12] The St. James rebellion of 1816 was supposed to have been incited by the inflammatory preaching of the Methodists. Cf. M. G. Lewis, *Journal of a West India Proprietor*, London, 1834, 226.

Bedward had at this time changed his title from that which he had so long borne as the "Shepherd," and announced himself as the incarnation of Christ himself. He was to be addressed as "Lord." When I finally sat beside him inside his house, he pressed this point with a touch of asperity. "I myself am Jesus Christ; I was crucified," he answered sharply when I hazarded an objection based on the teaching of Christ.

The man had a grievance. Several times he had petitioned that his own pastors be licensed to perform the marriage ceremony. In this way the marriage fees from members of his congregation would come into the hands of his own church instead of into those of outsiders. This privilege had at length been definitely denied him and he had accordingly decided to bring the world to an end. He showed me his black hands and told me how, in the new Heaven and the new Earth, they were to be as white as my own. He was a large fine-looking negro with the rapt brightness on his face of a visionary, clouded by the peevishness of tyranny. I looked into his face in vain for any hint of craft.

He told me of his past. "As a young man I committed every sin but murder; of murder I was not guilty;" and the story of his conversion followed as it is told by his biographer. He spoke with great simplicity of the symbolism of his church rites—the candle and the three fast-days a week—one as a symbol of the Trinity, the other designed for the purification of the soul in the spiritual life as of clothes in the secular. He said, "The tallow is like God; the wick within the tallow is like Christ in the bosom of the Father; the flame is the Holy Ghost who came from the Father and the Son." And of the fasts, "A day for washing, a day for drying and starching, a day for ironing; so the heart is made clean by the fasts."

All was gently, even reasonably spoken except for that moment of pettishness about his identity with the Lord. He spoke like a man who says a thing over until the repetition itself fastens conviction upon him. Far from being, as the scoffers say, a scheming pretender fattening upon the credulity of his followers, he was himself the most duped among them. He was a dreamer, now so far gone in senility—he must have been about sixty-two years of age—as to have dreamed the impossible.[13]

That this view of Bedward is correct was later proved by the events of the fateful 31st. He must have done absurd things those last days. It was said that he had a woman tied to a tree all day learning to fly, and reproached her that her faith was after all insufficient. On the morning of the day predicted for the ascent, he took a chair and stationed himself in the yard in full view of his followers, who grouped about him dressed in their fast-day clothes of white. As the hours passed and nothing happened, he told them that the event had been postponed. From time to time he set another date. Still many remained faithful. In May, he determined to make a public demonstration of his claims. On the march into Kingston, the government seized the opportunity to arrest the leaders for vagrancy, consign Bedward to the Insane Asylum, and disperse the prophet's following.[14]

It is likely that this event will close the history of Bedwardism in Jamaica. The story of its founding, organization and doctrine may be read in a small pamphlet published in Kingston in 1917 and written by an intelligent negro missionary named Brooks, who had become a convert to Bedwardism.[15] According to this account, the founder of the religious group was a negro named Woods, but commonly called Shakespeare, who came from America some time before 1879, made his home in a stonehole and gained some credit for his prophecies. He first predicted a flood, which presently followed. In December 1888 he visited August Town and threatened to sink the place unless the people should repent. They gathered to a meeting. Woods promised them the Spirit, and a strong wind coming up at the moment, his success was complete. He organized the church at August Town by appointing as elders twelve men and twelve women, one of whom he predicted would succeed him. Alexander Bedward was the destined successor.

Born in 1859, Bedward spent his youth on the Mona estate where he was known as "a good workman but a wild liver." After two years in Colon, where he got relief from a disease that had been troubling him for thirteen years, he returned to Jamaica only to be seized once more with the disease. At Colon, once more, he had a vision ordering his return on a mission to August Town. Hesitating to obey, as Brooks writes:

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[13] The Editor reminds me of Lord Frederic Hamilton's reference to Bedward in his *Here, There and Everywhere* (4th ed., 156) in connection with the earthquake of 1907, which suggests a relation between earthquakes and religious conversion.

[14] *The Gleaner* (Kingston), Thursday, May 5, 1921.

[15] A. A. Brooks, *History of Bedwardism or The Jamaica Native Baptist Free Church*, Kingston, Jamaica, 1917.

> During the evening, he and several others were sitting outside in the open air when he alone saw a man in white apparel approach him, and said "Did not I send you to those men for money to return to Jamaica? Why have you not obeyed?" Said Bedward, "Yes, but I know they are not going to help me, therefore I did not ask them." Then the man produced a whip with which he inflicted so severe a chastisement on Bedward as made him agonize. Those standing by observed him with extreme astonishment, hearing him speak to some, to them, invisible person and seeing his agony.

Bedward began his public ministry in August Town by performing baptism at the Mona river and giving the water as "medicine." Brooks says:

> The Lord told him saying "Once I made water wine. Behold, now I make water medicine. And you have I ordained my Dispenser, Watchman, Shepherd and Trumpeter."

Of the two hundred persons who took the water that day, only seven had the faith to take it as "medicine," and these seven were instantly cured.

In 1894, the corner-stone was laid of the "Jamaica Baptist Free Church," the attractive stone building 61 by 91 feet in size which now stands within the enclosure of the Bedwardites in August Town. When Woods died in March, 1901, he left Bedward ordained as "Bishop" of this church, with the title of "Shepherd." There were appointed, besides, two pastors, twenty-four elders and seventy-two evangelists. "Station guards" and "Mothers" were also named, and in various parts of the island "Camps" were established for affiliated congregations.

The church ritual laid emphasis upon fasting and upon special purification ceremonies to precede baptism. It appointed every Monday, Wednesday and Friday as a fast-day from midnight until one in the afternoon. At five in the morning the fasters assembled in the church for prayer. At twelve noon, those dressed in white gathered about a table covered with a white cloth on which were set white cups and saucers; others found seats elsewhere. Bread and water were distributed. After a praise service, at the minister's words "Are all in peace?" a grace was said, and all lifted their cups and broke their fast.

Immediately preceding the ceremony of baptism, which occurred every three months, a three days' fast was imposed participation in which was voluntary. It began on Tuesday at five and lasted until Friday at three. About noon on Friday, the participants, dressed in white, gathered in the yards with their cups. Here they marched and sang while the cups were filled from jars of water set ready for the purpose; then, at a given signal, all tossed the water into the air and marched into the church, where the usual ceremony of breaking fast was observed. Just before baptism, a "vow ceremony" was observed, when the "earnest inquirers" ranged themselves at the altar and received a lighted candle. Then all repaired to the river where the immersion took place, the candidates being escorted by a singing procession dressed in white and carrying lighted candles in their hands.

Bedwardism taught obedience as its foundation principle. Of the "three heavens" prepared for the believer, the first was to be gained through "blood," the second through "love," the third only through "obedience to the teachings of Bedward." Much in these teachings emphasized the Holy Ghost as the third revelation of the Trinity. "Judaism, Christianity, and Bedwardism are in the order of the Father, Son and Holy Ghost for the retrieve of fallen man," says Brooks in his pamphlet.

Much about Bedwardism as laid down in this source, such as the organization of the church on the Apostolic plan into Shepherd, ministers, elders, and evangelists; the emphasis upon the outpouring of the Holy Ghost; the constant fasting and communion, suggests the influence of the Apostolic Church, branches of which have, as we know, survived in America.[16] Woods may have got his ideas from this communion. But there is also much to be found in common with other religious cults which have gained power over the negro mind in Jamaica, much that harks back to an earlier religious symbolism. It was in vision that Bedward received his call, and many of his converts testify to the "vision of the white cup" before conversion. The power of prophecy, the power of healing, are his guarantees to the spiritual calling, and it is by the use of medicinal water that the healing is effected. The white dress and the candle are symbols of harmony with the spirit. The ceremony of tossing water into the air precedes that of Christian baptism. In the church enclosure stands a cottonwood tree whose roots clasp a rock. "A fountain will break out from this stone" says a prophecy: and although this, say later interpreters, must be taken in a spiritual sense, it is worth observing that the rock and the tree and the spring of water enter into the religious symbolism of the church and are preserved side by side with the house of worship built after the Christian pattern.[17]

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[16] United States Census. Religious Bodies: 1906, II, 179-181.

[17] Murray, in her *Witch-cult in Western Europe*, Oxford, 1921, refers to an interdict published in England in the 8th century against "vows paid, loosed, or confirmed at wells, stones, or trees" (22); to dances about a stone in witch rites (131); to candles employed in the ritual (144); and to rain-making (172). Cf. A. J. N. Tremearne, *Ban of the Bori*, London, 1914, 234; A. B. Ellis, *Tshi-speaking Peoples of the Gold Coast of West Africa*, London, 1887, 82.

A STUDY OF THE MÜLLER-LYER ILLUSION, WITH SPECIAL REFERENCE TO PARADOXICAL MOVEMENT AND THE EFFECT OF ATTITUDE[1]

By MARJORY BATES, Ohio State University

The history of the Müller-Lyer illusion dates from 1889 when Müller-Lyer published his article on "Optische Urteilstäuschungen."[2] In the year following, Brentano called the attention of psychologists to the problem and for several years thereafter the illusion was much discussed. The first serious experimenting and the most important research made on this particular topic in the last century was that of Heymans in 1895.[3] The most important work done in the present century is that of Benussi, 1904-1918.[4]

The problem of the Müller-Lyer illusion has never been fully defined. Either it has not been clearly thought out by the investigators and theorists who have dealt with it, or at best it has been variously stated, according as it issues from different systematic schools or represents different theoretical points of departure. Indeed, to state the problem is usually to hint at its possible solution. Everyone would, however, agree that the problem is to discover why two lines of equal lengths are reported as of different lengths when accompanied by certain *Nebenreize*. Not everyone would agree, however, that this statement is precise, or that it does not include more than is relevant to the problem. The psychologist, unless he is a behaviorist, asks: "What are the psychological processes which underlie the verbal report, and how are they dependent upon the *Nebenreize*?" Or perhaps: "Does the bare visual extent of the Müller-Lyer figure vary with the *Nebenreiz*, or is it merely the judgment upon the psychological extent that depends upon the *Nebenreiz* and thus renders the experience 'illusory'?" Even this latter statement, however, implies a systematic belief and does not entirely overcome the difficulty, for certain psychologists assert that judgment and bare experience can not, at least in practice, be separated from each other, and deny that simple sensations are directly correlated with the physical stimulus. It would seem, then, that any attempt to state the problem with precision launches us at once into the larger issue of what perception is, and that a study of the Müller-Lyer illusion can be undertaken only from a particular point of view. Indeed, it is not so much the fact that the theories about the illusion are not all directed towards the same end or that they have grown out of different preconceptions as to the fundamental nature of the problem, as it is that the basis of many of these theories has not been described nor the meaning of the terms used adequately defined. Nor in systematic matters is agreement so much to be desired as clarity in disagreement.

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[1] From the Psychological Laboratory of Clark University.

[2] F. L. Müller-Lyer, Du Bois-Reymond's *Archiv*, 1889, Suppl. Bd., 263ff.

[3] G. Heymans, *Z. f. Psychol.*, 1895, 9, 221-255.

[4] V. Benussi, Meinong's *Untersuchungen zur Gegenstandstheorie*, 1904, 303-448; *Z. f. Psychol.*, 1916, 42, Abt. 1, 22-55; *Arch. f. d. ges. Psychol.*, 1912, 24, 31-62; *ibid.*, 1914, 32, 50-57; *ibid.*, 396-419; *Z. f. Psychol.*, 1914, 69, 256-292; *Arch. f. d. ges. Psychol.*, 1918, 37, 233-282.

In general there may be said to be two principal types of theories: (1) those theories which assign as the cause of the illusion some further phenomenon which is separate and distinct from the actual phenomenon of the illusion but in the absence of which the illusion should not occur, and (2) those theories which have as their basis certain mechanisms under which the phenomena of the Müller-Lyer illusion can be subsumed, and which are therefore simply names for a group or groups of well established facts. Under the latter head can be placed that type of explanation which makes its appeal to laws rather than to mechanisms—laws which state merely the conditions of occurrence of the illusion and show, when possible, how they are identical with the laws of optical illusions in general.[5]

Benussi holds to the second type of theory and rejects the hypotheses thus far put forth.[6] He contents himself with a statement of the laws of what he calls 'the inadequate perception of form.' Only by reference to a careful summary of fact, so he feels, shall we be able to arrive at a satisfactory explanation of the reason why illusions occur. One may add that the problem of modern science is not so much the 'cause' of a phenomenon as to be able to state as accurately as possible the conditions for its occurrence. "The word 'theory' no longer means an hypothesis deliberately added to observed facts but an actually observed law or set of laws from which deductions may be drawn." [7] According to this view, then, the laws which Benussi has formulated are quite as deserving of the term 'theory' as the more picturesque explanations of earlier investigators.

The difference between the more recent theoretical work done with the Müller-Lyer illusion and the earlier work seems to represent a change which has taken place in fields of scientific inquiry in general. The feeling seems now to be that generalizations must wait upon the discovery of facts, and that while speculation may not be harmful it must be judged only for what it is worth.

It was not expected in the present investigation to make discoveries which would lead to a new theory of the Müller-Lyer illusion but rather to contribute some additional facts to the body of knowledge on the subject. Especially was it felt that new facts might come to light if the problem of the Müller-Lyer figure were undertaken in a laboratory which had at its disposal subjects trained in the introspective technique.

I. Paradoxical Movement

The first part of the experiment was undertaken in order to study introspectively paradoxical movement between two members of the Müller-Lyer figure shown successively in a tachistoscope. Such a study had already been conducted by Koffka and Kenkel,[8] but it was hoped to get some further light on the exact structure of such a phenomenon, and especially to study the content-aspect of the visual experience.

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[5] For a general discussion of theories see: V. Benussi, Meinong's *Untersuchungen zur Gegenstandstheorie*, 1904, 414-448; V. Henri, *L'année psychol.*, 1896, 3, 495-510; 1897, 4, 538-542; H. Piéron, *Rev. phil.*, 1911, 71, 245-284; E. B. Titchener, *Qual. Instr. Man.*, 1900, 321-328. A full discussion of theories from the point of view of the paragraph above is contained in a bound manuscript, by the present author and with the present title, that is deposited in the Clark University Library and may be borrowed under the usual courtesies of library exchange.

[6] *Arch. f. d. ges. Psychol.*, 32, 403ff.

[7] E. B. Holt, *The Concept of Consciousness*, 1914, 129.

The *O*s who took part in the experiment were *B*, Dr. E. G. Boring, *D*, Dr. L. D. Boring, *Y*, Dr. M. Yokoyama, and *H*, Dr. H. M. Halverson, all highly trained in introspection.

The exposure apparatus used was a modification of the Dodge tachistoscope.[9] It was built so that three separate fields were successively exposed, the first containing the fixation point. Each experiment began with the exposure of this field, during which the ready signal was given. A pendulum controlling shutters was then released. After an interval, which did not exceed 20σ, the field containing the first figure appeared. This field was exposed for about 7σ and then after an interval of about 30σ the second field appeared for the same length of time as the first. Each stimulus field was optically 42.5 cm. away from the eye of the *O* and the maximum length of any of the stimulus figures was 2.8 cm., so that the greatest visual angle subtended by any figure was 2°20′. The total field was 3.6 cm. square and subtended a visual angle of 3°3′.

A preliminary series was tried with the figure in which the extents to be judged were horizontal black lines, but this attempt proved unsuccessful chiefly because it was necessary to make the lines so wide in proportion to the size of the figure that the exact points of termination of the extents were indefinite. The main series was done with figures which consisted of black dots from 10 to 18 mm. apart and about 2 mm. in diameter. Certain of these figures had arrow-heads attached, others had feather-heads, and the rest had nothing at all. The *Nebenreize* were all 5 mm. long and ½ mm. wide, and made an angle of 30° with the horizontal. Seven pairs of stimuli were used as shown in Table I. In the presentation of the stimuli the *Konstanzmethode* was used.

The instruction was as follows (general): "At the signal 'ready' you are to fixate the black dot in the centre of the visual field. Immediately after the signal 'now' you will be presented visually with two successive pairs of dots, black on a light background. According to your impression, you are requested to make judgments of the comparative extent between those dots, reporting 'greater,' 'equal,' or 'less,' according as the second extent is greater, equal or less with respect to the first. In case of inability to make a judgment, report failure and then dictate your reason to the experimenter." Additional instructions: "Be sure to speak out during the experimenting whenever you think there has been anything unusual about your experience, or whenever any change in your attitude or mode of judgment is noted."

After Series II had been completed *O* was asked to make comment on every judgment and not merely on those he considered unusual. For this reason it was not found possible in Series III-VII inclusive to obtain more than ten judgments on each stimulus. Accordingly the points of subjective equality had to be approximated graphically rather than accurately determined by the usual mathematical procedures.

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[8] F. Kenkel, Untersuchungen über den Zusammenhang zwischen Erscheinungsgrösse und Erscheinungsbewegung bei einigen sogenannten optischen Täuschungen, Koffka's *Beiträge zur Psychologie der Gestalt- und Bewegungserlebnisse*, 1919, 6-98.

[9] R. Dodge, *Psychol. Bull.*, 1907, 4, 10-13; F. L. Dimmick, this JOURNAL, 1920, 31, 318f.

Table II shows the relative frequencies and Table III the upper and lower limens, as well as the points of subjective equality (S.E.), obtained for each of the *O*s in each of the seven series. The values for Series II in the case of *D* were not obtained. S_1 for *D* in Series III was omitted because the stimuli used did not contain the upper limen; the same was true with S_2 in series V. In Series I there were 50 judgments on each stimulus for each *O*, and in Series II also, except for *B* who made 100 judgments on each stimulus. In Series VI and VII with *H* and in Series VI with *B* only 5 judgments on each stimulus were given. The comparison stimuli in Series VI for *B* and *D* were 12, 13, 14, 15, 16, instead of the normal 14, 15, 16, 17, 18 mm.

Table I
Arrangement of Series

Series	Standard Stimulus mm.	Comparison Stimulus mm.	Order of presentation
I	16	14,15,16,17,18	Dots followed by dots.
II	16	14,15,*16,17,18*	Dots " " arrow-heads.
III	16	*14,15,16*,17,18	Dots " " feather-heads.
IV	16	*14,15,16*,17,18	Arrow-heads " " dots.
V	16	14,15,*16,17,18*	Feather-heads " " dots.
VI	18	*12,13,14,15,16,17,18*	Arrow-heads " " feather-heads.
VII	14	*14,15,16,17,18*	Feather-heads " " arrow-heads.

Table IV shows, first, the amount of divergence of the points of subjective equality from the points of objective equality in percentage of the latter, and secondly, the amount of divergence of the points of subjective equality of the Series II-VI inclusive in percentage of the points of subjective equality of Series I. These latter figures are the true percentages of the illusion in all the different cases, because they take into consideration the normal error made by every individual in the judging of extents under these conditions.

Charts I and II show graphically the data of the second part of Table IV. In all these presentations the illusion is said to be 'negative' when the effect of the *Nebenreize* is to make the second extent (comparison stimulus) relatively greater than the first (standard stimulus). The illusion in this case is negative because under such conditions subjective equality is less than objective equality. Conversely the illusion is 'positive' when the *Nebenreize* tend to make the comparison stimulus relatively less than the standard. The averages of Table IV and Chart II show the general relationship. (In the case of Series II, where a value was missing, the average was corrected in accordance with the average individual differences between *O*s in order to make it comparable.) Chart II shows clearly that the positive illusion is always less than the negative. Both the positive and the negative illusion follow the same law: namely, the effect of the feather-heads alone (III and V) is least, the effect of the arrow-heads (II and IV) is slightly greater, and the total illu-

TABLE II

Relative frequencies of judgments in each series for each observer, based on 10 judgments on each stimulus unless otherwise indicated. G = "greater"; L = "less." The number in parenthesis before the relative frequencies for *B* and *D*, series VI, indicates the change in comparison stimuli which had to be made in this series.

Series	I[1]		II[2]		III		IV		V		VI[3]		VII[4]	
Comparison Stimuli mm.	G	L	G	L	G	L	G	L	G	L	G	L	G	L
Y														
18	84	0	92	0	90	10	90	0	70	0	90	0	90	0
17	74	0	30	14	60	0	90	0	50	0	40	10	80	0
16	26	12	10	38	40	20	60	10	10	40	30	40	30	20
15	2	34	2	72	20	30	20	10	20	70	0	90	20	50
14	2	86	0	100	10	90	0	70	0	90	0	90	0	100
B														
18	98	0	98	1	100	0	100	0	90	0	(16) 60	0	100	0
17	90	0	66	1	90	10	100	0	60	0	(15) 20	0	70	0
16	20	28	31	18	50	0	60	0	10	30	(14) 20	0	20	10
15	6	50	3	77	0	30	30	10	0	90	(13) 0	80	0	70
14	0	92	0	94	0	60	10	50	0	100	(12) 0	100	0	90
H														
18	88	6	64	6	100	0	100	0	80	0	100	0	20	40
17	86	2	14	40	90	0	100	0	20	0	100	0	0	60
16	6	70	2	58	70	10	60	0	0	20	80	0	0	100
15	2	86	0	90	10	0	20	10	0	70	40	0	0	100
14	0	96	0	100	10	60	10	60	0	100	20	40	0	100
D														
18	80	0			100	0	100	0	40	0	(16) 100	0	90	0
17	58	0			100	0	100	0	10	0	(15) 80	0	50	0
16	14	20			90	0	90	0	10	70	(14) 40	0	30	20
15	8	20			70	0	40	0	0	90	(13) 0	100	10	70
14	0	74			40	40	10	60	0	100	(12) 20	80	0	100

[1] 50 judgments on each stimulus in this series with both *O*s.

[2] 50 judgments on each stimulus in this series with *Y* and *H*; 100 judgments with *B*.

[3] 5 judgments on each stimulus in this series with *H* and *D*; standard 18mm. for all *O*s.

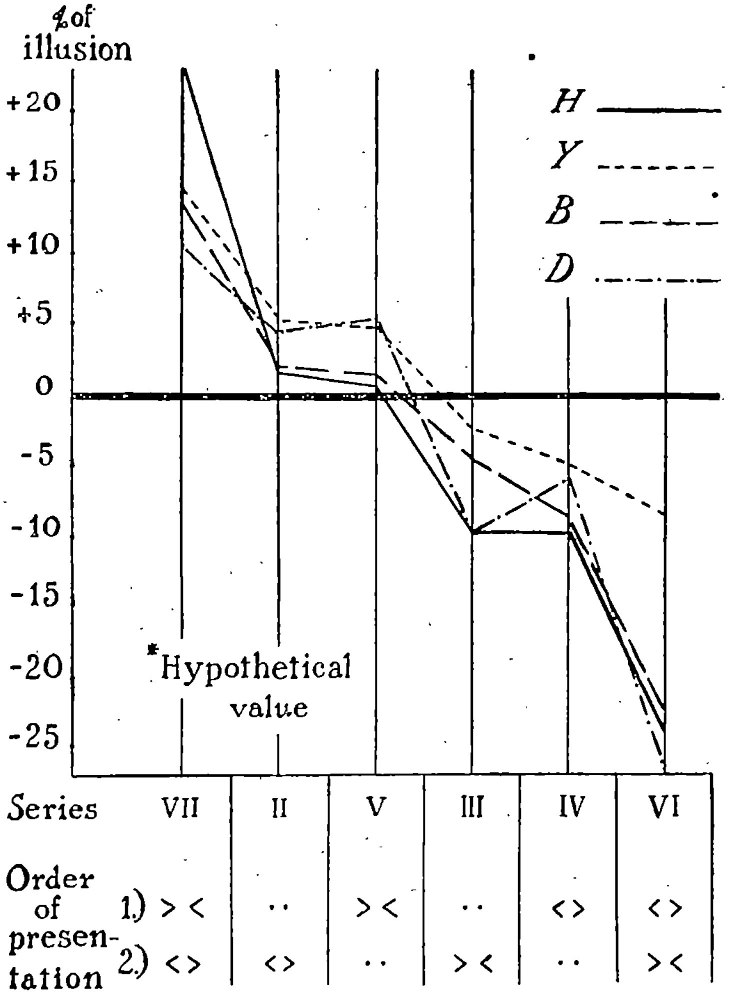

Chart I: Amount of the illusion, positive and negative, in percentage of extent of subjective equality without the *Nebenreize* (*i.e.*, corrected for time-error). Data from the second part of Table IV.

sion between arrows and feathers (VI and VII) is greatest of all, in fact greater than the sum of the corresponding two parts. Heymans[10] and others have shown that the arrows are less effective than the feathers and our results are in this respect consistent with others. The fact that the effect is more than doubled when feathers and arrows are opposed may be due to the fact

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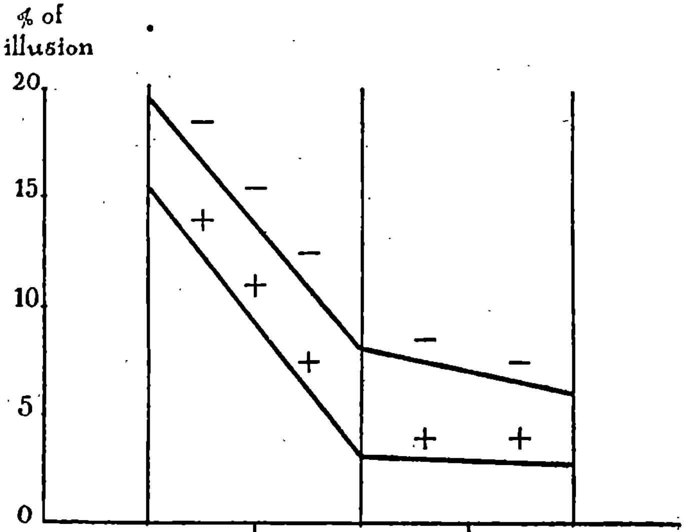
[10] G. Heymans, *op. cit.*, 235f.



Series		VI	IV	III
Order of presentation	1.	< >	< >	. .
	2.	> <	. .	> <
Series		VII	II	V
Order of presentation	1.	> <	. .	> <
	2.	< >	< >	. .

Chart II: Average amount of illusion, from Table IV and Chart I, arranged to show comparison of positive and negative illusion.

that in this more complex presentation (VI and VII) it was more difficult for the *O*s to maintain the attitude of judging process-extent, which they did in general by keeping the dots focal.

Summary of Introspections

The typical experience for all the *O*s was of a moving, changing figure, in which the first form changed into the second, a succession of separate figures.

Observer B. This *O*, while he worked with knowledge of the purpose of the experiment and often of his own results, showed such agreement with the other *O*s, not only as to the frequency with which he gave the various judgments but also as to the materials of his protocols, that his statements were taken at their face value.

TABLE III

Upper and lower limens and points of subjective equality based on values in Table II and determined graphically. S_1 = lower limen; S_2 = upper limen; S.E. = subjective equality; O.E. = objective equality (standard).

Os		*H*	*Y*	*B*	*D*	
Series						
I	S_1	16.2	15.0	15.2	14.6	
	S_2	16.8	16.4	16.2	16.9	
	S.E.	16.4	15.7	16.1	15.5	O.E. = 16
II	S_1	16.3	15.8	15.7		
	S_2	17.1	17.2	16.3		
	S.E.	16.7	16.6	15.8		O.E. = 16
III	S_1	14.3	14.8	14.3		
	S_2	15.6	16.0	16.0	14.4	
	S.E.	14.9	15.3	15.4	14.0	O.E. = 16
IV	S_1	14.2	14.4	14.2	14.3	
	S_2	15.8	15.7	15.6	15.2	
	S.E.	14.9	14.9	14.7	14.6	O.E. = 16
V	S_1	15.4	16.0	15.8	16.4	
	S_2	17.6	17.0	16.8		
	S.E.	16.5	16.4	16.3	16.9	O.E. = 16
VI	S_1	14.0	16.0	13.4	13.7	
	S_2	15.0	17.0	15.8	14.3	
	S.E.	14.3	16.2	13.8	13.8	O.E. = 18
VII	S_1	15.0	15.4	15.4	15.6	
	S_2	14.0	16.3	16.6	17.0	
	S.E.	14.0	15.8	15.9	15.9	O.E. = 14

The three criteria of judgment used by *B* were (1) relative displacement of the dots, (2) extent between them, and (3) movement outward or inward of the dots. Movement was either inferred or sensory; the sensory movement was that in which something was 'given' in visual quality, usually a gray, blurry connecting streak, presumably the process-basis of the phi-phenomenon.

I. In Series I most of the judgments were based on movement. The dots seemed actually to move in or out and the judgments were accordingly given as 'less' or as 'greater,' respectively. There were several cases, however, in which the judgment was contrary to the movement of the dots. Some indescribable criterion seemed to be present by which the *O* was assured that a particular movement was really illusory and should therefore be discounted. The dots were often reported as jerking, bobbing, or floating before they settled into place. It may be that this 'jerking' was the type of movement interpreted as illusory.

II. The judgments in Series II were made on the relative position or movement of the dots, but never on extent. Certain cases occurred in which the dots seemed to be left behind when the wings moved out or in. This made the judgment difficult for it meant that the dots in the original position could be compared either with the vertices of the wings or with the dots in the final position.

III. In Series III judgments of displacement and of extent were about equally involved in the critical judgments which showed the presence of

TABLE IV

<table>
<tr><th>Series</th><th>I</th><th>II</th><th>III</th><th>IV</th><th>V</th><th>VI</th><th>VII</th></tr>
<tr><td colspan="8">Amount of the illusion in percentages. The sign indicates the direction of the illusion. When the point of subjective equality is above the point of objective equality the sign is plus, and conversely.</td></tr>
<tr><td><i>Os</i></td><td></td><td></td><td></td><td></td><td></td><td></td><td></td></tr>
<tr><td><i>H</i></td><td>+2.5</td><td>+4.4</td><td>−6.9</td><td>−6.9</td><td>+3.1</td><td>−20.5</td><td>−20.5</td></tr>
<tr><td><i>Y</i></td><td>−1.9</td><td>+3.8</td><td>−4.4</td><td>−6.9</td><td>+2.5</td><td>−10.0</td><td>+13.0</td></tr>
<tr><td><i>B</i></td><td>+0.6</td><td>−1.2</td><td>−3.8</td><td>−8.1</td><td>+1.9</td><td>−23.3</td><td>+13.6</td></tr>
<tr><td><i>D</i></td><td>−3.1</td><td></td><td>−12.5</td><td>−8.8</td><td>+2.5</td><td>−23.3</td><td>+13.6</td></tr>
<tr><td colspan="8">The amount of the illusion as measured by the difference between the amount of divergence in series I and that in all the other series, between the point of subjective equality and the point of objective equality.</td></tr>
<tr><td><i>H</i></td><td></td><td>+1.9</td><td>−9.4</td><td>−9.4</td><td>+0.6</td><td>−23.0</td><td>+23.0</td></tr>
<tr><td><i>Y</i></td><td></td><td>+5.7</td><td>−2.5</td><td>−5.0</td><td>+4.4</td><td>−8.1</td><td>+14.9</td></tr>
<tr><td><i>B</i></td><td></td><td>+1.8</td><td>−4.4</td><td>−8.7</td><td>+1.3</td><td>−23.9</td><td>+13.0</td></tr>
<tr><td><i>D</i></td><td></td><td></td><td>−9.4</td><td>−5.7</td><td>+5.6</td><td>−26.4</td><td>+10.5</td></tr>
<tr><td>Average</td><td></td><td>[+3.4]</td><td>−6.4</td><td>−7.2</td><td>+2.9</td><td>−20.3</td><td>+15.3</td></tr>
</table>

the illusion. Except in one case movement was observed only with those stimuli where the effect of the illusion was in the direction of the objectively correct judgment. There were, however, many cases where a 'flashing' or 'pseudo-movement' was reported, but was discounted by *O* as a criterion. There were also several other cases in which movement was reported but did not act as a criterion. These latter movements were not characterized as 'pseudo-movements' but may nevertheless have been such.

IV. Movement was reported in Series IV as the basis of several of the 'illusory' judgments. No contrary criteria were reported.

V. In this series, as in III, extent and displacement were the chief bases for the 'illusory' judgments; movement was given as a criterion only twice and then with the critical stimulus. Moreover, in one of these instances extent as a criterion operated in a direction contrary to the movement. In another instance extent as a secondary criterion opposed displacement as the primary, and in two cases just the reverse happened.

VI. All judgments in Series VI, whether illusory or not, were made on the basis of the criterion of extent.

VII. Extent was in Series VII the most frequent criterion, although it was relatively less frequent in those cases where the objective difference was reversed by the illusion. Movement was reported only where the comparison stimulus was of the same length or only 1 mm. longer than the standard. In these cases, however, movement was got distinctly and without reservation, and seemed to be used as a criterion nearly as frequently as extent and quite as frequently as displacement. All three criteria were readily effective and became equivocal only where the objective difference between the comparison and the standard began to be so great that the effect of the *Nebenreize* was weakened. *O* stated that he was most sure of those judgments which were opposite to movement. Objectively, such judgments were much more apt to accord with the facts than those which were made in correspondence with movement. He also reported 'pseudo-movement' which came both with the 'greater' and with the 'less' experiences, so that he distrusted its validity as a criterion for a judgment of 'less.' Here again this pseudo-movement may be related, at least, to the 'flashing' and 'darting' reported above.

Except in Series VII the criteria reported as having been at the basis of the illusory judgments were chiefly extent and displacement. In VI, however, we find movement reported as having occurred in the direction of the illusion without correction by any other criterion as in the other series. In series VI, which has the greatest illusion of all, extent was used entirely as a criterion, and in the other series it was used more than relative displacement for those judgments which showed the presence of the illusion. It would seem, then, that with extent focal the illusion is rather more likely to occur for this *O* than in any other case, except in VII where the use of the criterion of extent seems to have little influence on the judgment in either direction. According to this conclusion the strict following of the instructions rather seems to diminish the illusion than to aggravate it. The explanation probably is that bare extent is less easy to observe than either relative displacement or movement, and that possibly the *O* really was not able to narrow himself down to simple extent, but, when he used this as the basis of his judgment, was judging the impression more as a whole than when he used the other criteria. Indeed, the *O* himself said that it was more easy to report on movement, since movement was something 'given,' whereas one makes a judgment *about* extent. It might be noted here, however, that a certain amount of judging had to be done in deciding whether movement was true or 'pseudo.' On the other hand *B* remarked on one occasion that he was surer of judgments made contrary to movement as a criterion.

Observer H. *H* had the same three criteria as *B*, extent, movement, and relative position of the dots; but the first criterion he used only in the first series. His experiences were usually complex and full of conflicts which he could not always resolve.

I. In this series *H* reported having judged on the basis of the successive positions of the dots rather than on the extent between them. He instructed himself to observe movement and reported many cases of simple movement, and also experiences in which no movement occurred. The gray of the phi-phenomenon came often, as well as a kind of jerking and dancing of the dots. There was usually a 'hang-over' of the first sensation which lasted into the second. Eye-kinaesthesis of contraction and of expansion took place. Both, especially the former, aided judgment considerably.

II. Judgments in this series, unlike those in the first series, were ordinarily made on the basis of extent between dots. Movement of the dots and the wings, however, was a factor in the judgments. Compromise often had to be made when the dots appeared to move out at the same time that the wings moved in, or when what seemed an illusory pulling in of the dots by the wings occurred. This pulling in, or 'tendency towards pulling in,' happened frequently, even sometimes with the largest comparison stimulus. In one case, however, *O* reported that, although there was movement inward, the comparison stimulus actually seemed larger in consequence, because the wings 'succeeded so miserably in bringing their dots closer together than the standard dots.' A good many different process-descriptions of the movement were made. There was also a shutting up of the wings like scissors, especially with the smaller comparison stimuli. 'Hang-overs' from the first presentation were still experienced.

III. In the first series movement was experienced, but not used as a criterion; in this series it was used as the basis for most judgments of 'greater.' These judgments were usually univocal, and occurred chiefly with the objectively greater stimuli; the judgments of 'less,' or 'equal,' which occurred most frequently with the objectively smaller stimuli, were based almost entirely on the position of the dots. There were some cases, also, with the smaller stimuli, where an outward movement of the wings was present, although the judgment was 'equal,' and one case of an equal judgment was given in spite of an outward movement of the dots themselves.

IV. Here, except in one case, the only stimuli judged on the basis of movement were the larger ones, where movement, along with position and occasionally extent, was one of the criteria. Position was the chief criterion with the 'greater' judgments, and practically the only one with the 'less' judgments. There were many reports of rather irrelevant 'flutterings,' 'flashings,' and 'shootings' of the wings. Many of the 'less' judgments were made *in spite of* these phenomena. Both these 'flutterings,' and the 'shutting up' of the wings reported in the second series are probably akin to *B*'s 'flashing' and 'darting.'

V. In this series relative position of the dots again becomes the chief criterion. However, it is likely that many of the judgments of 'less' and 'equal,' even when not so reported, were based at least partly on movement of the second pair of dots inward after they had appeared to be farther apart than the first pair. Several cases of this kind were indeed described. The only equivocal judgments, including those just mentioned, where a conflict occurred between relative position and movement, appeared where the influence of the illusion would naturally lead to objectively incorrect judgments.

VI. Some sort of movement was reported as present all through this series although ordinarily the 'shooting' and 'flashing' of the wings was at least partly abstracted from. Actual movement came in mostly with the larger stimuli, where the dots seemed to move outward even though objectively the largest of the comparison stimuli was smaller than the standard. No report was given after two exposures of the smallest comparison stimulus, possibly because of the lack of a positive criterion.

VII. Equivocal judgments were present in this series only where the influence of the illusion might be expected to lead to an objectively incorrect judgment. Most of the judgments were based on movement of the dots in or out. A 'flashing' of the wings, described here as like the closing up of the leaves of a book, and in Series II as like the shutting up of a pair of scissors, was always present and sometimes influenced the judgment. There were several compromise judgments where the dots moved out, but the closing up of the wings gave the impression of contraction, and in consequence the judgment 'equal' was usually given. There were also two cases where relative position as a criterion was set over against movement of the dots, and a compromise judgment of 'equal' given.

Throughout all of the series *H* reported conflict between his naive impression of the figures, in which movement seemed to play a large part, and a more analytical attitude in which attention was paid to the relative position of the dots and an attempt was made to abstract away from the movement.

The particular conflicts which occurred for *H*, where illusory judgments were given, were of a number of different kinds. There was the conflict involving 'pseudo-movement,' conflict involving movement inward of the dots after they had appeared farther out, and conflict between relative position and movement. Very likely several of these conflicts could be resolved into one, but *H* spoke of them in his introspections as if they were all quite different.

Observer Y. *Y's* attitude, in striking contrast to that of *H*, was relatively simple and consistent, and admitted of very few conflicts and secondary criteria.

I. In series I the judgments were mostly based on relative position of the dots. *O* instructed himself to observe movement and reported this as well as illusory connecting lines between corresponding dots in successive exposures. These lines evidently took the place of palpable movement when they occurred.

II. *Y* reported in this series also that he made most of his judgments on relative position of the dots. Here there was no actual movement of the dots, but there were the same illusory connecting lines as in Series I, vague, grayish things running from each dot to the corresponding dot in the next exposure. Movement was reported, but only movement of the wings as they seemed to shoot out from the dots.

III. In this series *O* speaks for the first time of judgment on the basis of more extent between the dots. This criterion as well as that of relative position was used with the larger and with the smaller stimuli. Equivocal judgments, where extent and position did not both lead to the same judgment, or where the judgment was not made merely on the basis of one criterion, were found only with the critical stimuli where the illusion would naturally occur. In these cases the judgment made on the basis of relative position was always the objectively correct one, whereas that made on the basis of extent gave evidence of the influence of the illusion.

IV. All but two of the judgments in this series were made on the basis either of extent or of position. One of these judgments was made on what *O* called 'immediate impression of extent,' which seemed to be distinguished from the usual extent judged in the former series in that it was less analysed than those other impressions of extent; whereas the other of these judgments was made on a 'narrowing down of the brightness field' which immediately led to the judgment 'less.' Judgment on 'immediate impression' was 'greater' by this criterion and by relative position; that is, it was reported as being equivocal.

V. In this series extent, which the *O* now called 'immediate impression of extent' but which seemed to be merely the sort of thing he had always been judging, was the main criterion, although relative position came in often to reënforce the judgments made on the basis of extent. *O* always seemed very well satisfied with the judgments which he made on the basis of extent alone; but, as a matter of fact, those made on both extent and position were more likely to be correct objectively.

VI. The main criterion here was also extent. Secondary criteria came in very seldom, although change both in the brightness field and in relative position of the dots was occasionally spoken of as helping out the judgment. The wings were reported throughout the series as rotating from one to the other position.

VII. Movement of the dots and change in their relative position were mentioned several times, but the greater number of the judgments were made solely on the basis of extent between dots, or 'immediate impression of extent' as it was still called. Rotation of the wings was experienced in this series as well as in the one proceeding.

For *Y* the main criterion throughout was evidently that of extent, which came to him as something absolute. The first figure was simply of 'that extent,' the second was at once seen as either greater or less than this first figure, or equal to it. Some of the introspections show, however, that the relative position of the dots had a good deal to do with this immediate impression of extent, although *O* thought that he would have got even fewer secondary criteria than he actually did, had he not been asked to make detailed reports on his experience. *Y*'s illusory judgments taken alone show only one thing which may be significant and which distinguished these judgments from the others: in Series V position as a criterion seemed to be more nearly 'correct' than extent.

Observer D. I. *D* took a passive attitude and judged mostly on the basis of movement of the dots in or out. She spoke of eye-kinaesthesis of contraction and expansion which paralleled this movement but which was never the basis for the judgment. She reported also 'flickers' of the dots which might be considered a movement but was not at all like the straight movement in and out.

II. Series II was not given to *D*.

III. In this series the larger stimuli were in every case judged solely on the basis of movement. Movement was also experienced with the comparison stimulus of the same size as the standard, and the judgment was made on this basis, except in one case, where the movement was recognized as false because it did not involve the dots, and the judgment 'equal' was consequently given. All other experiences where the judgment 'e qual' was given involved some sort of movement, which was, however, always discounted. With the smallest comparison stimulus two judgments were made on relative position of the dots, and contrary to the direction of movement. Movement, all through the series, was, however, the criterion most frequently used.

IV. Distinction was made in this series between 'slightly greater,' 'greater,' and 'decidedly greater,' by the self-instruction of the *O*. The resulting judgments followed very closely the objective values. The higher values were judged 'greater' univocally by only one criterion, namely movement. Different kinds of movement were, however, distinguished. There were the simple movement, the 'jumpy' movement, and an actual 'jump.' The 'jump' did not appear below the largest comparison stimulus, nor the 'jumpy' movement below the next smaller. The judgments of 'equal' were made on the basis of absence of movement. Eye-kinaesthesis was reported throughout the series but was not used as a factor in determining the judgments.

V. The judgment 'less' was given on the smaller comparison stimuli in this series mostly in the presence of contraction, or of the 'jumping in' of the dots towards the centre. This was true also with the comparison stimulus equal to the standard. When this stimulus was judged 'equal,' movement had been experienced but was reported as unanalysable and did not seem to influence the judgment. In one case this stimulus was reported 'greater' because expansion of the dots had occurred after a contraction had already taken place. There were several cases where the larger comparison stimuli were judged 'equal' in spite of what seemed to be a contraction of the dots, and there was one case where this judgment was made in spite of both movement out and movement in. There were also several judgments of 'equal' made on the larger stimuli where *O* experienced no movement at all or only the unanalysable movement which she always discounted.

VI. Movement was the chief criterion in this series. Here all the comparison stimuli were smaller than the standard. With the smallest of these, where the illusion was present, the wings were seen performing a complete rotation, but with the larger stimuli they simply jumped from one position to another. Rotation of the wings without jumping from one position to another was reported where the illusion was overcome; the dots in these cases seemed to move towards the centre. The judgments of equal were made on the basis of rotation of the wings unaccompanied by expansion or contraction of the dots. Eye-kinaesthesis was experienced all through the series but never was made the basis of a judgment.

VII. Here the smallest comparison stimulus was just equal, subjectively, to the standard. All others were greater. With the smaller comparison stimuli, however, many cases of contraction of the dots were reported. Equality was judged on the basis of absence of expansion or contraction and the presence only of rotation of the wings or of rotation and unanalysed movement, which was never considered a genuine movement of the dots. This movement occurred also with the smaller stimuli when they were judged 'less,' even though they were objectively larger than the standard or equal to it; and very likely in these cases as well as in those cases where it was not reported this movement was responsible for the presence of the illusion. With the larger stimuli the wings seemed to change their position by jumping instead of by rotation, and the dots also seemed to jump. In several cases the dots gave the impression of movement outward after the rotation of the wings inward had taken place. Eye-kinaesthesis was reported throughout the series.

D's attitude was passive throughout. Her judgments were made wholly on the basis of contraction and expansion of the dots. This contraction and expansion took place in several different ways. Some movements were described as 'jumps,' some as 'jumpy movements,' others as simple movements. There was also an unanalysed movement which was, however, discounted in the making of the judgment. Rotation of the wings also occurred in the last two series. Most of the illusory judgments were followed by reports of 'pseudo-movement' as well as of contraction and expansion of the dots.

It is evident that the experience indicated by the judgments 'greater' and 'less' may sometimes accord with the objective displacement of the dots, and sometimes, under the influence of the *Nebenreize*, be in contradiction to the objective displacement. This fact is what is meant by the existence of an 'illusion.' The further problem lies in an understanding of the illusory displacement, *i.e.*, of the experience which contradicts the objective displacement.

It appears in the first place that although judgments according with the objective displacement and judgments of the experience contradicting it may appear identical there is, nevertheless, if the total consciousness be taken into account, a difference, or at least a tendency to difference, between the two types. The judgment which corresponds with the objective displacement tends to be univocally determined, to issue from a number of criteria reënforcing one another. The illusory judgment, on the other hand, tends to be equivocally determined, to issue from a conflict of criteria. The conflict may be between the impression of extent and movement, between relative position of the dots and extent, between relative position and movement, or between movement and 'pseudo-movement.' Of course some of these criteria may be effective in 'correcting' the illusion, so that it frequently happens that a judgment in accord with the objective displacement issues from a conflict. Relative position of the dots seems most likely to counteract the illusion, whereas movement of the wings and 'pseudo-movement' almost invariably tend to enhance it.

Although the illusory experience tends to be characterized by conflict and non-illusory experience by univocal determination, exceptions occur in both ways. As we have seen, a conflict may be won in the direction that corresponds with objective displacement. On the other hand, there seem to be numerous cases of illusory determination that is univocal. Most noteworthy are the cases of judgments based upon simple movement which is seen in the direction opposite to the direction of displacement of the dots.

Table V shows the frequency of occurrence of such 'illusory' movements, *i.e.*, of movement reported in the direction opposite to the objective displacement ('1' of the Table) or movement in the direction of the illusion when there was no objective displacement of the dots at all ('2' of the Table). It will be noted that movement occurs much more frequently when it is not in opposition to the objective displacement. When there is opposition between the objective displacement and the subjective there is, of course, likely to be conflict of criteria, and it is natural that there should be fewer cases of reported movement where the conflict has been resolved in favor of the movement as a criterion than where there is less chance for conflict. There are also large individual differences in report. *Y* never reported movement, whereas *D* under the more favorable conditions experienced it in from 66% to 100% of all cases.

The question naturally arises as to the exact psychological nature of this reported movement. An objective displacement in one direction must be accompanied by a corresponding displacement in the image upon the retina. It seems almost incredible that a sensory experience of seen movement could take place in the direction opposite to the direction of shift in the retinal image. One is inclined to wonder whether the movement is after all actually seen as a process-experience or whether it may not be merely inferred on the basis of some static criterion. The answer seems to be that it is actually seen in sensory terms. It is certainly not a gross inference of the order of the stimulus-

TABLE V

Percentages of total number of illusory judgments which were based on movement of the dots as a criterion. *Y* did not report this criterion. A very few cases of movement contrary to the effect of the *Nebenreize* when there was no objective displacement of the dots are omitted.

		Percentages		Total number of illusory judgments possible for each *O*
Series	*B*	*H*	*D*	
1. Movement reported as opposite to the direction of objective displacement.				
III	0	5	20	20
IV	0	5	20	20
V	0	0	0	20
VI	0	25[1]	24	50
VII	10	35[2]	22	40
2. Movement in the direction of the normal illusion reported when there was no objective displacement of the dots in either direction.				
III	10	33	100	10
IV	20	33	90	10
V	25	0	66	10
VI	—	—	—	0
VII	33	33[3]	100	10

[1] 25, instead of 50, possible illusory judgments.
[2] 20, instead of 40, possible illusory judgments.
[3] 5, instead of 10, possible illusory judgments.

error, for the *O*s gave reports of process and fractionated the total experience into these various criteria of which movement was only one. The reported movement is therefore something more than a mere report of movement, than a bit of verbal behavior; movement is the name of a definite experience distinguished from displacement and change of extent. The bare fact that movement is thus separated off from the other criteria in itself tends to establish its status as sheer experience of the order of mental process. This indication is further supported by the expression of the *O*s who describe the movement as 'seen.' Ideally the final test of its sensory nature would be its equation either as meaning to the phi-phenomenon, or as process to the gray blur that Dimmick[11] showed to be the process-basis of the phi-phenomenon. The conditions of the experiment are, however, peculiarly unfavorable to the observation of the phi-phenomenon, and the *O*s moreover were untrained for such observation. The matter can therefore probably not be regarded as finally settled, but the indications are that purely illusory movement is sensory like ordinary movement and that under favorable conditions it would be found to reduce to the phi-phenomenon.

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[11] F. L. Dimmick, *op. cit.*, 317-332.

Summary and Conclusions

1. According to the results obtained from our data the illusion is consistently less on the average when 'positive' than when 'negative.' (The illusion is positive when the *Nebenreize* tend to make the comparison stimulus relatively less than the standard; and conversely.)

2. The illusion was given about equally by the arrow-heads and by the feather-heads as *Nebenreize,* although the feathers were slightly more effective. The total illusion, however, when measured directly, was more than the sum of these two parts when measured separately, presumably because of the greater difficulty of keeping the dots of the figure focal with the more complex stimulus.

3. The illusion occurs strikingly (average = 16% of standard extent) even with short exposures and figures almost wholly in foveal vision, although under such conditions eye-movement is practically eliminated.

4. Illusory or paradoxical movement of the dots is in the single instance not different introspectively from movement in the direction of the objective displacement.

5. There are, in the critical region where the largest number of illusory judgments are given, many more cases of conflicts of judgment due to opposing criteria than in those regions where the effect of the *Nebenreize* is in the same direction as the objective displacement or is entirely counteracted by it.

6. The different kinds of criteria upon which the judgment may be based are (1) seen movement of the dots, (2) displacement of the dots without movement, (3) total extent between the dots, (4) eye-kinaesthesis of contraction and expansion, (5) movement of the wings and (6) 'pseudo-movement' (usually not definitely localized). The last two kinds of movement are usually not employed consciously but are reported to have been influential and to have played a part in many of the conflicts of judgment.

7. The presence of more than one criterion does not always mean opposition; sometimes it means reënforcement. Opposition comes chiefly in the critical region.

8. Movement is not invariably reported by any observer, although it is reported frequently, especially in the last two series. *Y* gives no reports of movement whatsoever.

9. The average amount of the illusion is 1.5 mm., or 16% of the standard length.

10. The illusion, in degrees of visual angle corresponding to the average amount of the illusion, is 1°14′. This displacement is about as great as the space occupied by one hundred retinal cones, or 50 cones at each end.

11. There is no obvious physiological explanation of the manner in which the seen *Nebenreize* produce a seen sensory movement without any corresponding displacement of the retinal image.

II. Relative Effects of the Total Impression and the Analytical Attitude

The 'critical' or 'analysing' attitude as distinct from the attitude of 'total impression,' under either of which it is possible to regard the Müller-Lyer figure and each of which gives different quantitative results, has been studied by Benussi[12] and by Koffka and Kenkel.[13] They have shown that the attitude of total impression is most likely to bring about the greatest amount of illusion; and also that anything in the stimulus which favors this attitude makes the illusion more likely to occur, and conversely that anything in the stimulus which favors the analytical attitude hinders the appearance of the illusion.

The object of the second part of this experiment was to discover, under certain conditions, the relative importance of each of these factors, the subjective and the objective, in bringing about the illusion.

The apparatus included a large square of white cardboard, 57 x 72 cm., secured to a solid wooden background of somewhat larger area. In the middle of this sheet of cardboard was a small horizontal slit about 5 cm. by 1 cm., behind which were the poles of a horse-shoe magnet. Each stimulus, a normal Müller-Lyer figure of cardboard, had attached to it on the back a darning needle by means of which the figure was held by the magnet against the slit in the cardboard. The arrangement gave the effect of a figure standing out clearly against a very large background and prevented judgment of the extent of the stimulus in relation to the extent of the background. The horizontal part of each figure was covered with velvet-black paper and the wings with one or the other of two different tints of gray (photometric values respectively equal to 67.5% and 26.7% of baryta white) or with velvet black (photometric value equal to 1.7% of baryta white). By the 'wings' is meant not only those parts of the figure which go off at angles from the main line but also the parts of the main line which would be included by perpendiculars dropped across it from the intersections made by the edges of the wings with the main line. Thus the main line extends only to the points of intersection with the wings, and is broken by the middle pair of wings. The standard part of this line was 5 cm. long and was bounded by arrow-heads. The comparison extent ranged in length from 4.5 to 5.5 cm. by intervals of .25 cm. for every one of the three kinds of stimulus (black wings, medium gray wings, and light gray wings).

There were, therefore, 15 different figures in all, which were presented in a haphazard manner in series of 15. *E* sat behind a screen to one side of the cardboard background and controlled the two 75-watt daylight lamps (type-C2 Mazda) just above the *O*'s head. The light was kept on until

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[12] V. Benussi, *Arch. f. d. ges. Psychol.*, 1912, 24, 31-62; Referat über Koffka-Kenkel, *ibid.*, 1914, 32, 50-57.

[13] F. Kenkel, *op. cit.*; K. Koffka, Zur Grundlegung der Wahrnehmungspsychologie, *Beiträge zur Psychologie der Gestalt- u. Bewegungserlebnisse*, 1919, 203-282.

TABLE VI

Relative frequencies of judgment 'greater' (G) and 'less' (L) for Observers *P* and *M*, instructions I (total impression) and II (analytical attitude), and the three sets of stimuli, *i.e.*, light gray wings (L), medium gray wings (M), and black wings (B). These data are the observed psychometric functions from which the points of subjective equality of Table VII are interpolated.

	Instruction I			
	M		*P*	
Comparison stimuli mm.	G	L	G	L
L 6.0	100	0		
5.5	82	0	100	0
5.0	7	76	89	0
4.5	1	93	24	1
4.0	0	99	0	76
3.5			0	100
M 6.0	100	0		
5.5	99	1	10	0
5.0	3	15	83	0
4.5	1	65	46	1
4.0	0	100	1	64
3.5			0	99
B 5.5	100	0		
5.0	78	2	99	0
4.7			53	0
4.5	0	73	23	2
4.2			3	39
4.0	0	99	0	70
3.5	0	100		

	Instruction II			
	M		*P*	
Comparison stimuli mm.	G	L	G	L
L 5.7	71	0	38	0
5.5	13	0	7	17
5.2	0	87	0	72
5.0	0	87	0	98
4.7	0	90	0	98
M 5.5	47	0	30	2
5.2	7	23	0	58
5.0	0	87	0	99
4.7	0	94	0	98
4.5	0	100	0	100
B 5.5	83	0	67	0
5.2	17	1	7	17
5.0	2	5	1	45
4.7	0	93	0	100
4.5	0	100	0	100

the *O* had made his judgment; it was then turned off and the next stimulus put in place. During the intervals between exposures the room was completely darkened except for a small shaded red light which *E* used in adjusting the next stimulus. The *O* was seated 1.9 m. away from the stimulus, which was on a level with his eyes and subtended an angle of about 3°.

Three *O*s took part in this experiment, but the final results were obtained from only two. These two were Dr. C. C. Pratt (*P*), who was very highly trained in introspection, and Miss E. F. Möller (*M*), who was only slightly trained in introspection. The third *O* was Dr. Halverson (*H*), who had already taken part in the experiments with the tachistoscope. The reasons for retaining only *P* and *M* will be discussed in connection with the introspective results.

I. *Instruction for attitude of total impression*: "When the stimulus is presented you are to pay attention to the total impression and to report on the length of the horizontal line at the left as compared with that of the horizontal line at the right."

II. *Instruction for analytical attitude*: "When the stimulus is presented you are to pay attention to the two horizontal linear extents, abstracting as far as possible from the 'wings,' and report on the length of the left extent as compared with the right."

Table VII is a comparison of the points of subjective equality obtained with each of the different tints of wings and with each of the instructions. Chart III is a graphic representation of the same data. The figures represent the points of subjective equality obtained from 100 judgments on every stimulus, determined graphically by linear interpolation between the observed frequencies.

TABLE VII

Points of subjective equality for observers *P* and *M*, instructions I and II, and stimuli L, M, and B (*cf.* legend to table VI). These points of subjective equality are the stimulus-values of the points of intersections of the psychometric functions for 'greater' and 'less' given in Table VI. They were determined graphically.

	Instruction I (total impression)			Instruction II (analytical attitude)		
	L	M	B	L	M	B
M	5.2	5.0	4.7	5.3	5.3	5.0
P	4.3	4.2	4.3	5.5	5.3	5.2

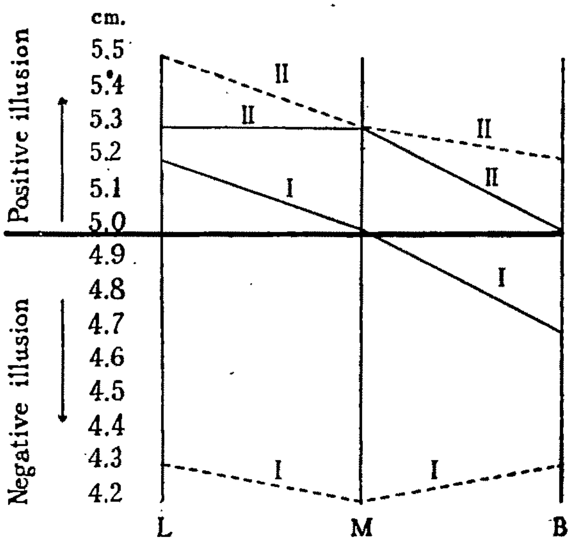
Both instructions (average)

	L	M	B
M	5.2	5.0	4.7
P	4.9	4.7	4.7

For *M*, L gives on an average about 2% less illusion than M;
M gives on an average about 2% less illusion than B.

For *P*, L gives on an average about 4% less illusion than M;
M gives on an average about 6% less illusion than B.

For *M*, Instruction II gives about 2% less illusion than I.
For *P*, Instruction II gives about 20% less illusion than I.



CHARTA III. Amounts of the illusion as indicated by points of subjective equality (Table VII) for *M* (solid line) and *P* (dotted line) for instructions I (total impression) and II (analytical attitude), and for stimuli with light gray wings (L), medium gray wings (M), and black wings (B). Since the standard stimulus is 5 cm., the illusion is positive when subjective equality is less than 5 cm., and conversely.

Summary of Introspections

Observer H, Instruction I. *H* experienced a great deal of eye-kinaesthesis and seen movement in the visual field, so that it was usually hard for him to come to a decision; when he did he was often quite dissatisfied with his judgment and felt that it might equally well have been for one of the other two categories. A special instruction was finally given him, limiting his judgment to his first total impression. This instruction, however, seemed to have little effect. It neither gave *O* more confidence, since he was always remembering that he had overlooked an impression previous to the one on which he had judged, nor did it seem to make any great difference to the objective results. The special instruction was given him partly because it was thought that it might reduce the number of 'equal' judgments of which there was an exceedingly large number. The frequency of these judgments seemed to accord with the fact that *O* took more time in reflection than did the other *O*s. The number of 'equal' judgments did decrease somewhat under the changed instruction, but at the same time the illusion continuously became very much larger, so that in order to use the method of constant stimuli a fresh start had to be taken frequently with certain stimuli discarded and other new ones added. Always there was much conflict and doubt whether a horizontal of 3 cm. was equal to one of 5 cm. or less than it, even when the stimuli with the light gray wings were used. It was therefore decided at this point that it would be more satisfactory not to try to use *H* in this particular experiment.

Some of *H*'s reports are interesting, however, because they show him to be an entirely different type of *O* from the other two. He reported a visual experience of movement, which took the form of a rapid, sometimes almost simultaneous, contraction-expansion process, although his experience differed for the light gray, medium gray, and black winged figures. The comparison lines to which the light gray wings were attached seemed to expand only a very little, and most of their movement was contraction. The lines with medium gray wings seemed to expand in 'puffs,' although they became fixed when they were in focal vision. The lines with the black wings seemed to expand even when they were fixated, so that the illusion here was greater than in the case of the figures with medium gray wings as the illusion with the medium gray wings was in turn greater than with the light gray wings. These reports were actually borne out by the accompanying judgments with fair accuracy. Later on, all three kinds of figures gave for *H* about the same amount of illusion; but he evidently was not aware of this change; at least he failed to report any change in mental process, and continued to speak in general terms of the contraction-expansion process. In contrast with the other *Os*, he was always alive to his experiences, always reflecting on them. Since his quantitative results were constantly shifting, while those of the other *Os* were relatively stable, there may be a certain significant connection between such aggressive awareness of process and instability of objective judgments. All this sets him apart from the other *Os* who showed under both instructions a fairly rapid and thorough-going mechanization.

It might be added that during one experimental period *H* several times saw the figures in perspective and reported that the horizontal line on the over-estimated side of the figure looked like the end of a tunnel (the wings being receding walls), which came nearer or went farther away according as it seemed to expand or contract. The contraction and expansion seemed, however, to be the primary phenomena, so that this additional experience of depth could hardly be used in support of a perspective-theory.

H was not given Instruction II.

Observer M, Instruction I. *M* was at first extremely conscious of eye-movements, which started at the middle wings and went first to the left and then to the right. Her judgment was generally made on the basis of the length of the sweep of the eye-movement. *M* made her judgments more quickly as time went on, and experienced increasing difficulty in taking the figure as a whole. By the time the preliminary series was over and the experiment proper was begun, her judgment-process had become so mechanized that she found nothing more to say about it. She did not realize how she was making her judgments; they came automatically. At times she reported that it was difficult not to allow a knowledge of the illusion to influence her decisions and also that she was sometimes slightly prejudiced by the recognition of figures and the memory of previous judgments.

Instruction II. At the very beginning of this series *M* remarked that she thought she must have been abstracting away from the wings even under the instruction for total impression because she seemed to have no difficulty now in neglecting them. The only report of process after this time was a statement to the effect that 'equal' judgments, which were usually doubtful, were the only ones whose correlated conscious processes could be described.

Observer P, Instruction I. At first a certain amount of description of processes was given. The figures with the light gray wings were always hard to judge by total impression. The black horizontals persisted in standing out much more clearly and the *O* involuntarily squinted so as to exclude the wings altogether. If this did not succeed he usually closed and then opened his eyes and then made another attempt to get a true total impression. This total impression was characterized by a more nearly equal distribution of clearness than the impression most naturally aroused by the figures with gray wings, in which clearness was largely concentrated on the black linear extents. Little eye-movement was noted except at the very beginning.

Instruction II. A statement made just after the second instruction was given will suffice for the whole series. *P* was not aware afterwards that his attitude had undergone any change.

"The *Aufgabe* was quite conscious in the fore-period, sometimes a vocimotor image, or a horizontal or telescoped visual image of the lines; especially towards the end, it was present in a muscular set of the eyelids, in squinting. The judgment itself was fairly easy to make. Consciousness was much more specifically determined now than it was before, because now there was a narrowing down to one object. First there was very rapid narrowing of the visual field, culminating in a focal visual perception of a horizontal line at the left; then a rapid shift of focality to the right. Sometimes the judgment came quite automatically on the basis of this first experience. The following kinds of judgment are apt to be delayed: first, judgments where the two stimuli appear almost equal; second, judgments which depend on the sharpness of outline of the horizontal line. Sometimes, however, this seems to run imperceptibly into the wings. Then it is hard to get it focal. There are four degrees of difficulty: first, when all the figure is the same tint and the horizontal lines are nearly equal; second, when the wings are slightly different in tint all the way down to white (*O* evidently did not note the fact that there were only two tints of gray!) and the lines are the same length; third, when the wings are the same tint but the lines are very different in length; fourth, when the wings and the lines are both different."

Summary and Conclusions

1. It is obvious (Chart III) that the effect of a change of observational attitude dependent upon instruction may be of the same kind and magnitude as the effect of an objective change in the stimulus. The effect of the assumption by the observer of a critical or analytical attitude (Instruction II) instead of the attitude for total impression (Instruction I) decreases the illusion just as does a change in the tint of the wings in a direction to make them less similar in quality to the lines (from light to black wings when the lines are black). In noting this relationship in Chart III or Table VII, it must be kept in mind that the change in the illusion is superimposed upon a large constant space error, so that in most cases an increase in the illusion appears as a decrease of the negative illusion.

2. There is perhaps little reason to suppose that similar objective results necessarily imply similar psychological bases, but in this case it would appear that the changes due to the two causes (stimulus and instruction) depend upon like psychological mechanisms. One might expect in advance that qualitative similarity between the wings and the lines would conduce to a taking of the figure as a whole and that contrast between the wings and the lines would aid analytical observation. *P* actually does report this situation. With black wings upon a black line he was obliged to squint in assuming the analytical attitude, whereas it was natural for him to take the figure by total impression. Conversely the light wings naturally led him to analysis, and it was only with difficulty that he could include them in a total impression. *M* shows the same objective relations, though she gives no adequate account of the attitudinal basis.

3. There are differences between the two observers with respect to the effect of the tint of the wings. *P* under the instruction for total impression (I) shows no general effect of change of tint, and under the analytical attitude (II) a difference of 0.3 cm. for a change from light gray to black wings. *M* gets a difference of 0.5 cm. under Instruction I and of 0.3 cm. under Instruction II for the same change in the wings.

4. There are much larger individual differences in the effect of changed instruction. For *P* the changed observational attitude results in a shift of the point of subjective equality of 1.07 cm. on the average; whereas for *M* the corresponding shift is only 0.23 cm. on the average.

5. *P* is therefore more affected by the attitudinal change than by the change of stimulus, in the ratio 1.07/.15; whereas *M* is more affected by the change of stimulus than by the change of instruction, in the ratio .40/.23. This difference may in part be due to the fact that *M* did not radically alter her attitude when the instruction was altered for her; she reported under Instruction II that she was not even sure that she had changed her mode of observation since the series under Instruction I. That she did change slightly, the objective results taken in the light of *P*'s introspection would signify.

6. It will be seen that these results indicate the possibility of an objective measurement of attitudinal change. For either *M* or *P* separately it is possible to say what degree of photometric difference between the wings and lines should be equivalent to the attitudinal change involved in the change from one instruction to the other. This relatively indirect measurement of the effect of attitude has the advantage of being in kind and is more significant, therefore, than the more direct measurement in cm. of the shift of subjective equality. One induces by instruction attitudes that tend towards confluence of the figure into a whole or towards a separation of the figure into its analytical parts. Similarly one induces confluence or contrast objectively by reducing or increasing the qualitative similarity of the wings to the lines. Specifically in our two cases we may say that the analysis of the figure obtained by instruction to analyse was, in the case of *M*, half as great as the analysis obtained by the qualitative separation of the wings from the lines by a photometric distance equivalent to 65.8% of baryta white; and that, in the case of *P*, the degree of analysis induced by instruction was about seven times as great as was induced by the same photometric difference.

7. In general it seems reasonable to suppose, and our two cases support the supposition, that variability would be greater in the case of the induction of attitude by instruction than in the case of its induction by the objective conditions of the stimu-

lus. An instruction is notoriously uncertain in its effect; our results indicate that occasionally it might be abandoned in favor of some more objective and constant determiner.

GENERAL CONCLUSIONS

In the formulation of the present problem it was the intention to apply modern introspective technique for the accomplishment of the isolation of the bare sensory extent upon which judgment is supposed to be operating when the illusion is in force. It seemed, in advance, as if the illusion might readily turn out to lie entirely in an inadequate statement of the problem; as if a judgment, purporting to be a judgment of principal extents, might actually prove to be a judgment in part of the *Nebenreize*. A substitution of description of conscious content for the indeterminate judgment should test this assumption: in an adequate description it ought to appear at once whether the principal extents were changed in themselves by the *Nebenreize* or whether the change consisted merely in an extension of the judgment to include the *Nebenreize*. Benussi's demonstration[14] that the analytical attitude reduces the illusion tends to support the latter view, but it seemed prudent to press the matter further to the full description of the ultimate data.

The original programme, however, was thwarted by the discovery that it was impossible to reduce the consciousness to a bare simple experience of extent, to a mere process-attribute. With a relatively simple stimulus, an extremely rapid exposure, and an instruction focussed upon a single aspect, the essential data determining the judgment remained very complex as regards their structure. It was possible to push analysis far enough to render the component data univocal as regards judgment, that is to say, to obtain a pair of extensive data which meant 'greater' or 'less' without equivocality; but even such univocal determiners of judgment both actually and potentially are complex for description. To reduce experience to pure extensiveness proved impossible.

This result is undoubtedly as aspect of a much larger problem. The conscious ultimates as they enter into description are not after all bare unanalysable processes, but rather mental objects, occurring in a particular experiment and functioning under the determination of the problem for a given psychological meaning. We need not go into this question here, but simply point out that our failure to arrive at a simple extensive consciousness is consistent with Koffka's exposition of the psychological *Gegenstand*,[15] and is also in accord with certain experiments, as yet unpublished, by Dr. Yokoyama in the Clark Laboratory, which indicate that the descriptive yield of introspection is in accordance with the psychologically objectifying determination under which the observation is made. Nevertheless the failure to obtain a simple extensive consciousness need not of itself have prevented the anticipated outcome. It would have been enough that simple mental data, even though given as mental objects, acted as univocal determiners of the extensive judgments and were individually subject to uniform description. We might still have expected that a judgment of the principal extents, from which the appearance of the *Nebenreize* was excluded as a basis, would have shown no illusion. We found, on the contrary, that the illusion was directly conditioned upon the principal extent as given visually. Paradoxical displacements and paradoxical movements of the dots directly determined the illusion. These displacements and movements were descriptively vivid as visual data. They were not inferred but 'seen.' They were caused by the presence of the *Nebenreize*, but existed as mental data in and of themselves and were the immediate basis of judgment, or else a discarded basis when some other criterion was preferred in a case of equivocal determination. The original hypothesis was therefore not borne out: the extents as seen do expand and contract by seen displacement or by seen movement of the delimiting dots. The judgment does not issue from the *Nebenreize* directly, but only indirectly as the *Nebenreize* determine the extent of the primary sensory impression.

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[14] V. Benussi, *Arch. f. d. ges. Psychol.*, 1912, 24, 31-62.

[15] K. Koffka, *op. cit.*, 249ff.

Here then is a puzzle that awaits explanation. It is not unlike that old puzzle concerning the seat of the discrepancy between stimulus and judgment that is expressed by Weber's law, and we should naturally seek an answer in similar terms. Roughly speaking, this complex phenomenon may be separated into five successive phases: (1) the stimulus; (2) peripheral excitation or the retinal image; (3) central excitation, a vague region of the series where attitude and *Einstellung* may be supposed to get in their work, or where some hypothetical central form of nervous confluence might be imagined to be operating; (4) the primary mental datum, sufficiently simple in the given situation to act univocally as criterion for report; and (5) the judgment or report itself, not sharply differentiated from the primary datum but consequent upon it and sufficiently explicit for the descriptive record of the experiment. Between which of these terms does the illusion occur? Where does the discrepancy enter in?

It can not occur between the primary mental datum and the judgment. The illusion involves an alteration of the datum, not merely a change in the judgment of an unaltered datum. Thus the illusion is not 'psychological', as would have been said of such an explanation of Weber's law.

Does the change occur between the stimulus and the peripheral excitation? If the *Nebenreize* led to some sort of retinal confluence, so that the retinal image was shortened or lengthened over fifty cones at each end, then we might regard the illusion as situated here; but such an assumption seems highly improbable. Moreover, we should have then also to explain contrast at the level of the retina, since spatial contrast and confluence seem to be two extremes of the same phenomenon that conditions the Müller-Lyer illusion.

Accordingly we must interpolate the effect of the *Nebenreize* between the retina and the mental datum. It is of little use to inquire whether the interpolation is physiological, lying between peripheral and central excitation, or 'psychophysical,' lying between central excitation and the datum. The double-aspect view of psychology would preclude the latter hypothesis, and the former hypothesis is not explicit because there must be many levels of central excitation amongst which we can not distinguish. Somewhere here, however, one expects to find attitude effective; certainly attitude is not likely to be effective at the retina and we have shown (contrary to our own expectations) that is is not effective at the level of the judgment. It is in the central regions that Wertheimer[16] supposed the short-circuiting that explains seen movement to be, and movement is one of the criteria of the illusory change. The effect of qualitative difference in tint of the wings might perhaps be supposed to be retinal if there were any theory of retinal confluence ready to assimilate the facts; but it seems natural that this difference too is comparable or even, as has been argued above, similar to the attitudinal factor, and is effective only as it conduces to analytical observation or the reverse.

If we say that the fact of the Müller-Lyer illusion is a fact of confluence between the *Nebenreize* and the principal extent, we have to conclude further that it is neither an excitatory nor a judgmental affair, but that it exists as a presensational and post-excitatory confluence of central *locus*, dependent upon both stimulus and observational attitude for its realization.

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[16] M. Wertheimer, *Z. f. Psychol.*, 1912, 61, 248.

AN EXPERIMENTAL STUDY OF THE PERCEPTION OF STICKINESS

By M. J. ZIGLER, Princeton University

In a recent article[1] Cobbey and Sullivan call attention to the fact that relatively little experimental work has been undertaken by psychologists in the way of analysing the 'touch blends' into their ultimate elements. The present report of our attempt to isolate, analyse, and determine the essential conditions underlying the perception of *stickiness* is a response to this need. By perception of *sticky* or *stickiness*[2] we here mean such tactual experiences as are called forth when a portion of the skin to which a sticky object (*e.g.*, a pencil dipped in liquid glue) has been applied tends to adhere to, and is elevated by, that object during its removal.

A. Analysis of Stickiness

Our experimental work was carried on from October to May, 1921-22. All our *O*s were relatively untrained in cutaneous work; consequently the first two or three months were consumed in training them in general tactual observation and in striking upon a suitable method of attack for our particular problem. The seven *O*s were: Professor H. C. McComas, Mc; Miss Margaret Macdonald, M, a graduate of Vassar, now employed by the Psychological Review Company; Mr. C. H. Rice, R, graduate student; Mr. M. G. Scheck, S, assistant in psychology; Mr. J. D. Jackson, J, a trained psychologist connected with the New Jersey Department of Institutions and Agencies at Trenton; Mrs. M. J. Zigler, Z; and the writer, Zi. R was unable to complete the work, but finished most of the analytical part. Mc did not observe during the first three months, but was able to complete the work in a briefer period because of training. J was used in the last experiment only, that of synthesis, and knew nothing of the earlier work, not even that the experiment was running in the laboratory. He therefore served as a valuable check against any possible suggestion which the other *O*s might have incurred from the previous work. The writer served as *E* excepting when he observed, when Z acted as *E*; thus, Z knew something regarding the procedure at every step of the experimentation, but in all other respects she worked in ignorance; Zi wrote his own introspective reports, so as to avoid suggestion to Z.

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[1] L. W. Cobbey and A. H. Sullivan, An Experimental Study of the Perception of Oiliness, this JOURNAL, 33, 1922, 121ff.

[2] The term *sticky* is not wholly unequivocal, since we colloquially speak of a 'sticky' day in referring to a warm and humid atmospheric condition. A usage more likely confusable with that here intended is the use of *sticky* in the sense of a 'sticky' thistle or needle. While there doubtless are common elements in these two kinds of tactual *sticky*, the predominance of the pain quality in the case of the thistle or needle is sufficient to distinguish it from 'sticky' in our sense.

(1) In our earliest work we used the very simple procedure of immersing the rubber end of a lead-pencil, the cone-shaped tip of which was cut off up near the point where the rubber meets the wood (here it measured 5 mm. in diam.), in sticky substances such as liquid glue, prunes, molasses, jelly, etc., and of applying the immersed end by a gradual downward freehand movement to the volar surface of the tip of *O*'s index finger or to the back of his hand between the third and fifth metacarpals; after allowing the stimulus to rest firmly on the skin about 3 sec. we removed it by a gradual freehand upward movement. Excepting a brief series of presentations in which the parts stimulated were active, the member stimulated was kept passive during the experimentation. 20 to 30 stimulations were made at each sitting, and the application was made at approximately the same place on succeeding presentations so as to avoid distraction. *O* was both blindfolded and screened from the hand which was stimulated, and was instructed that the stimulus would set up a tactual perception, which he should name and describe as fully as possible, giving all the sensory attributes that the experience exhibits.

From the beginning all *O*s distinguished between the pressure complex which was set up while the stimulus was being applied to and rested on the finger or hand, and the complex experience which was evoked by the adherence of the skin to the stimulus and by the separation of the skin from the stimulus during the removal. All *O*s reported that stickiness was the most prominent component in the latter complex, and asserted that this was aroused only during the removal of the stimulus. As the *O*s convinced themselves of this fact they were instructed to confine their descriptions to the experiences which were set up during the removal of the stimulus, unless those of the earlier phase of the stimulation had a direct bearing or threw light upon the experiences in the latter phase. We thus hoped to simplify the *O*'s task and to secure a fuller account of the experiences occasioned during the removal.

Under these instructions the *O*s designated the experience stickiness' from the outset; but sooner or later they began to distinguish between a 'pull' or 'pulling' or 'traction,' and a 'breakaway' or 'breaking loose' or 'tearing loose' or 'superficial release.' The temporal distinction between these two aspects of the total experience was not clean-cut; as a rule the 'pull' or 'traction,' which usually involved the entire area occupied by the stimulus and often extended outward into the adjoining parts somewhat, preceded the 'breakaway' phase, although the latter set in before the former had subsided. Sometimes the 'pull' was most intense at the periphery of the stimulated area during the initial stages, and the point of maximal intensity shifted toward the center during the course of the experience; in other instances the 'pull' was uniformly intense over the entire area, the peripheral parts releasing or breaking away during the course of the experience. In either case the 'breakaway' phase set in in the peripheral parts of the area before the pulling at a slightly more central region had fully transpired; thus the two components, the 'pull' and the 'breakaway,' overlapped temporally.

The 'pull' was described as a deep-seated experience, whose intrinsic quality was dull pressure. During the earlier work there were rather frequent reports of strains, faint aches, and pains, as constituting a rôle in the 'pull' aspect, but as the *O*s progressed in training these were designated as irregular and inessential features of the experience. The 'breakaway' component was described as purely superficial, and of a light pressure or contact quality. Ticklish pressure and ticklish contact were often reported, but the ticklish characteristic was not invariably present. At this stage of the work all *O*s were inclined to regard both the 'pull' and the 'breakaway' as intrinsic and indispensable components of the sticky experience.

Different kinds of stickiness were also reported from the very beginning, the labels given being *smooth*, *rough*, *tough*, *dense*, *brittle*, *dry*, and *wet* or *moist*. None of the *O*s reported all of these qualities, but every *O* gave at least 4 different kinds.

(2) A brief series of experiments was carried out with two *O*s, Z and Zi, with the view of determining whether the perception of stickiness shows any essential change when the member stimulated is active. A bit of sticky substance was applied to the tip of the index finger, *O* being instructed to bring the thumb in firm contact with the tip of the finger and then to separate the two digits. Under these conditions stickiness manifested no new characteristics. Here also the experience of stickiness was occasioned only during the phase of separation; hence we are able to conclude from both of the foregoing experiments that this perception is called forth by a moving stimulus only.

(3) The procedure employed in the foregoing work was very crude, and permitted only the roughest sort of analysis. In order better to control the conditions of our experimentation and to simplify *O*'s task we built a piece of apparatus which we shall designate by the name 'pull gauge.'

This was built on the principle of an ordinary spring-balance scale. It consisted of a hollow, thin-walled brass cylinder, 20 cm. long and 12 mm. in diam. The upper end of the cylinder was closed by a metal cap. A small coiled spring was suspended inside the cylinder by being soldered to a short metal rod, fastened to the center of the metal cap. A longer small metal rod was attached to the lower end of the suspended spring and protruded through a small hole in the center of another metal cap, which closed the lower opening of the hollow cylinder. This small rod bore a flat metal disc of 12 mm. diam. on its lower end, just outside the cylinder cap, and the spring was adjusted so that it held the metal disc firmly up against the cylinder cap. A slit cut lengthwise in the cylinder wall permitted an indicator, which rode on the second metal rod at the upper end where it joined the spring, to protrude through the cylinder wall and to move downward as far as the spring could be stretched. The instrument was graduated along the margin of the slit in intervals of 5 gr. and was capable of measuring pulls as high as 80 gr. By spreading a bit of glue on the bottom surface of the metal disc and applying it to *O*'s finger tip, the strength of the adherence during the removal of the stimulus was registered by the extent to which the spring was stretched downward. Thus *E* was able to read off directly the amount of pull exerted upon the skin in units of 5 gr. at every stimulation.

One limitation of this instrument consisted in the fact that we were unable to determine beforehand the strength that the pull would register. Thus, when we desired to give a series of weak stimulations, it could not be predicted beforehand that the pulls would be 5 gr. or less, as we desired; occasionally a 10 or a 15 gr. pull resulted.

To improve the manner of applying the stimulus, the pull gauge was mounted vertically on the ends of two parallel horizontal brass rods, which were hinged at the other end to a vertical baseboard. The parallel rods were connected by a brass spring, which was so adjusted that it held the pull gauge at a position of rest 15 cm. higher than the position taken when the parallel rods were in the horizontal plane, the position assumed when the stimulus was applied to *O*'s finger. By this means the instrument was applied at a uniform rate by pressing downward gently on the upper one of the parallel rods, thereby stretching the spring connecting them. A slow and regular removal was effected by the elastic recoil of the spring as *E* gradually released the pressure on the rod.

With this improvement in the method of procedure the *O*s confirmed their earlier distinction between the deeper seated pressure complex, which was variously termed a 'pull,' 'pulling' or 'traction,' and the series of superficial contacts or light pressures, which was designated the 'breakaway' phase. Moreover, all *O*s now identified stickiness as belonging to the 'breakaway' phase, several stating explicitly that the pull serves merely as a cue suggesting that stickiness is coming, others merely remarking that stickiness is not aroused until the 'breakaway' phase sets in. The following are samples of the reports upon which the foregoing generalizations are based.

"The stickiness is at its best and is perceived as sticky at the moment when the skin begins separating from the stimulus" (Mc). "The pull made me expect stickiness, but it was not really sticky until the breakaway came" (M). "When the pull comes I think it is going to be sticky. The pull seems to act as a cue as to what is going to happen. Then when the breakaway comes I know it is sticky" (M). "Stickiness was not perceived until toward the end of the experience when the breakaway came" (Z).

"Stickiness is confined to the series of light touches that are felt just as the stimulus and skin separate" (R). "The sticky comes when the skin is released from the stimulus" (S). "The release gives the stickiness as it breaks away in patches" (S).

The quality of stickiness was again described as a series of light pressures or contacts, which often, though not invariably, have a ticklish or tingling character. The series of light pressures resulted from our use of an areal stimulus; the 'breakaway' of the areal stimulus, which set up the light pressure complex, took place progressively from the peripheral parts of the stimulated area toward the center giving a series of 'breakaways' that were temporally closely connected. Simple 'sticky' seems to be made up of a field of light pressures, whose intensive, durative and clearness attributes show characteristic modifications. The light pressures first become clearer or more prominent and more intense, waxing to a moderate and sometimes to a fairly high intensity, from which there is a rather sudden drop to a low or zero intensity. The duration of the pressure quality in stickiness is sustained much longer than in the experience of simple pressure. These attributive modifications characterize the superficial cutaneous part of the experience and do not pertain to the deeper seated 'pull' experiences. The following sample reports indicate the general nature of these attributive changes.

"One strand of sticky remained long that time. It seems that the basis of the experience is a series of contacts, which fluctuate in intensity. The contacts increased in intensity a little and then decreased rather suddenly. The phase of increase was at least two or three times as long as the decrease" (R). "Three or four light contacts held on and were long drawn out and fluctuated in intensity as usual, increasing a little and then decreasing. That was the essence of the stickiness" (R). "The experience lasts much longer than when the stimulus is removed without giving sticky" (M). "The intensity of the series of contacts in the breakaway changes. This time it changed very gradually and while one was dying out another was getting stronger" (M). "Sticky came from a group of individual points, which were stimulated irregularly. There was a rather marked lessening of pressure as it passed from one to another" (S). "After the pull had well subsided the feeling of sticky came in. It was like a series of touches in which the intensity came into prominence more noticeably and then dropped out rather quickly" (S). "The sticky has the quality of pressure, it can't be anything else. Only the pressure lasts a little while longer than in mere touch and has a continuous change. It changes in intensity and in vividness." (Mc). "Stickiness seems to end rather suddenly. It seems to have a rather high intensity and then goes out rather suddenly" (Mc). "It was just as if a light feather brush moved over the finger and the skin adhered to it a little as it passed along. This adherence was right on the skin surface and was distinct from the *pull*. It was just a patch of light contacts and as it passed from one to another it gave a smooth sliding sticky" (Z). "The characteristic feature is the continuous change in intensity. A little area comes into clearness, increases in clearness a little and also increases in intensity a brief time and then dies out rather gradually. Usually the phase of dying out is rather brief but this time it was rather gradual" (Zi). "The most significant thing is the sustained duration during which the individual contacts become significantly clearer and gradually more and more intense and then suddenly drop out from a moderate intensity and high clearness to practical obscurity. This all took place within a very brief interval of time" (Zi).

The *Os* also began reporting certain criteria, which characterized the different qualities of stickiness. Almost invariably a cold or cool component was reported in the *wet* or *moist* sticky, and there was uniform stimulation over the whole area occupied by the stimulus; that is, there were no unstimulated spots in the pattern. *Rough* sticky consisted of a pattern of scattered or discrete sticky spots, among which intervening unstimulated areas were irregularly distributed. These discrete spots of sticky were described as being of considerable size and of patch-like pattern and were stimulated in rapid succession as the stimulus separated from the finger. *Smooth* sticky, as contrasted with *rough,* occurs when the breakaway of the stimulus gives a series of tiny, closely compacted sticky stimulations. It was designated as a massed pattern; the cluster of points was closely grouped and the points themselves contiguous. In this regard *smooth* was like *wet* or *moist* sticky, although it lacked the cold component which is distinctive of the latter variety. *Dense* sticky also is composed of a closely aggregated field of sticky points; it differs from the *smooth* in that it practically always occurs at high intensities. *Tough* sticky occurs at high intensities also; the distinguishing feature here is the very gradual decrease in the intensive phase during the breakaway. It is sometimes massed like the *wet, dense* and *smooth* varieties; at other times it has the scattered or discrete pattern of the *rough* variety. *Dry* sticky was reported very frequently, but no positive distinguishing criteria were found. It was negatively differentiated from *wet* or *moist* sticky in that it lacked cold, and it usually had the scattered or discrete pattern. It was reported very often in connection with *rough* sticky, and perhaps has no true sensory basis.

These different kinds of stickiness do not always occur separately; very often two varieties were assigned to the same experience. Thus, a sticky experience may be both moist and smooth, dry and rough, tough and dry, etc. Certain combinations occur together very frequently, other combinations never occur.

There are some indications that visual association plays a secondary rôle in distinguishing the various kinds of stickiness. Thus, *tough* sticky was reported as being made up of strands, strings or cords of glue or gum, *dry* of a group of tiny threads of dry glue, *rough* of irregular patches of sticky substance, *smooth* of slippery and spongy substances, and the *dense* variety betrays by its name that the massed pattern was in all likelihood partially, if not largely, visually represented.

Two *O*s made occasional reports that a good sticky experience resulted when the preceding pull was so weak as to be imperceptible and when there was no perceptible elevation of the skin. Consequently we gave a series of weak stimulations, in so far as our procedure permitted us to regulate the intensities of pull, in order to work out this point. The *O*s reported that good sticky experiences are set up at such low intensities of pull as 2.5 gr., when neither the pull nor the elevation of the skin is perceived. True, movement or elevation of the skin is usually perceptible in the stronger sticky stimulations; but all *O*s reported more or less frequently and more or less positively that the perception of elevation of the skin is not intrinsically essential to the perception of stickiness. A corroboration is found in the relatively infrequent mention of movement or elevation of skin in the reports, although the *O*s were instructed to give a full report of all sensory components of the experience. In experiments (1) and (2) we found that stickiness is realized only when the stimulus is moving; hence it appears that movement of stimulus is essential, while conscious representation of movement is not an indispensable condition of the perception.

(4) We next took a series of observations under the same experimental conditions excepting that a punctiform was substituted for the areal stimulus. A pointed needle was dipped in a sticky substance and applied to the finger. This procedure simplified the conditions of observation remarkably, in that *O* was permitted to give attention to the temporal course of a single adhering point in its simplest form; with the areal stimulation, all stages of stickiness were realized at any particular moment during the stimulation, since some points of stickiness were dying out at the same instant that others were coming into more pronounced vividness; and it was only by a process of careful abstraction that *O* arrived at the true temporal course of simple stickiness.

The simplified conditions enabled several *O*s, who hitherto seemed somewhat undecided as to certain features of the perception, to confirm more conclusively the true nature of stickiness. Owing to the fact that only one small point of adherence was perceived under these conditions, stickiness seemed to have lost something of its intrinsic quality according to the earliest reports; but all *O*s soon reported that this loss was illusory, and was occasioned by the very weak intensity and especially by the abbreviation of the temporal course. If the single punctiform contact exhibited a slight increase in clearness and in intensity and then decreased, it furnished a good sticky experience despite the fact that the intensity was low and the temporal course abbreviated. A good stickiness was not realized, however, at every stimulation. In the unsticky instances the *O*s reported a touch in which there was no increase in clearness and intensity before the experience terminated.

(5) We next attempted to give a pull on the skin without setting up the conditions underlying stickiness; that is, we desired to get a method of lifting the skin without permitting the stimulus to break loose or separate from it. Our *O*s had reported that 'pull' is a separate and distinct experience, having no essential connection with stickiness, and that the latter perception is aroused when neither pull nor movement of the skin is perceived. By arranging conditions so that a pull on the skin could be made without allowing the conditions of stickiness, the breakaway, to take place, we hoped to establish these facts more conclusively.

The unmucilaged side of strips of passe-partout was glued to strips of cardboard, which were then cut up into 1.5 cm. squares. Several of these squares were stuck on the unhaired portions of *O*'s wrist and forearm. These 'stickers' were lifted by short pieces of thread, which were attached at the sides of the squares and joined above, care being taken to avoid lifting hard enough to cause them to break loose from the skin. Thus a pull on the skin was occasioned without involving the breakaway phenomenon.

Under these conditions we always got reports of the deeper seated pressure complex. Stickiness was reported in one or two instances only, when a small portion of the 'sticker' broke loose. In such cases *O* always described the experience as the characteristic superficial light pressure phenomenon, which was qualitatively distinct from the pull complex.

During the earlier part of this work several *O*s made occasional reports that they were unable to judge whether the experience, which we have designated as 'pull', was occasioned by a lift upward or by a pressure downward upon the skin.[3] Such reports appeared rather frequently in the case of two *O*s. In the work with the 'stickers' just reported all *O*s reported such things as: "That seemed just like a pressure downward upon the skin," and "That might have been a pull or a pressure downward, I am uncertain which it was." This mistaking a pull for a pressure seemed interesting enough to warrant more investigation; consequently we arranged a series of 40 stimulations, using the passe-partout stickers, in which one half were actuated by lifting the threads and the other half by a gentle pressure downward upon the 'sticker; *E* trained himself so that he was able to give approximately the same amount of pressure downward as was occasioned when the threads were lifted upward.

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[3] Cf. E. B. Titchener, *Textbook of Psychology*, 147, 156.

The results show that pressure is mistaken for pull upward in 30 to 50% of the stimulations, and pulls are confused with pressures in 40 to 55% of the cases when weak stimulations are employed. Moreover, in many instances when *O* judged correctly, he qualified his judgment by saying he was uncertain whether it was a pressure or a pull, or that it might have been either. Besides, in some of his most positive judgments *O* was flatly wrong. These generalizations pertain in particular to the weaker stimulations; such confusions occur much less frequently at higher intensities of stimulation. The two intensities of stimulation used were 5 gr. = .022 gr./mm. and 20 gr. = .088 gr./mm. The following table embodies the result of our work.

	PULL JUDGED AS PRESSURE		PRESSURE JUDGED AS PULL	
O	Weak stim.	Higher stim.	Weak stim.	Higher stim.
M	40%	10%	50%	5%
Mc	50%	15%	30%	5%
S	55%	15%	50%	5%
Z	40%	10%	40%	10%
Zi	50%	10%	45%	10%

The reports indicate that certain sensory criteria aided in distinguishing between the pulls and the pressures. The pulls tended to extend out into the adjoining area; that is, the pull was not confined to the area covered by the 'sticker'; cords or threads of pull extended out into the adjacent parts. The pressures, however, were confined to the area covered by the stimulus, and there was often a sharp delimitation between this area and the adjacent region because of the sharper *pressure gradient* along the edges of the 'sticker.' Pressures are also characterized by a uniform firmness and compactness, while the pulls tend to lack uniformity of stimulation over the stimulated area. These differentiating criteria were much more pronounced at the higher intensities, while at the lower intensities they were scarcely perceptible.

A course of training, which extended over some weeks, was undertaken with one *O*, Mc, in order to determine the influence of training upon this illusion. It appears that, in so far as improvement was possible, it consisted in increased familiarity with the distinguishing criteria mentioned above. With the weaker stimulations Mc reduced the number of confusions to about 25% of the judgments, while with the higher the illusion rarely occurred after the course of a couple of weeks' training.

As a complement to the foregoing experiment with the 'stickers,' we attempted to determine whether stickiness is ruled out by anaesthetizing the skin with ether. When the skin was rendered completely anaesthetic only the deeper seated pull experience was reported; stickiness was not reported by any of the *O*s, though they would certainly have reported it if present because of their previous work. The numbing influence of the ether prevented us from making certain that the pull in this experiment was identical with the experience which we have named pull above; it appears, however, that the pull previously mentioned is a combination of cutaneous and subcutaneous pressure.

B. *Synthesis of Stickiness*

Having carried through the analysis of stickiness and having determined the intrinsic conditions under which the perception is called forth, we next sought a method of synthetizing the perception. Unlike Bentley's *liquidity*[4] and Sullivan's *oiliness*,[5] both of which are compounds of two separate tactual qualities, stickiness involves only a single cutaneous quality, the attributes of which show distinctive modifications. We are therefore unable to present such a clean-cut synthesis as those investigators were able to achieve. Nevertheless, we were able to evoke the characteristic sticky experience by means of a stimulus which was totally devoid of any sticky substance. When a small sharply pointed fish-hook or needle was carefully inserted under the layers of the epidermis of the tip of the forefinger and lifted upward slowly and lightly, and when the lifting was terminated rather suddenly, all *O*s, including J who observed in this phase of the experimentation only, reported stickiness in 50 to 70% of the stimulations. If the needle or fish-hook happened to break through the epidermis during the lifting phase of the stimulation, the stickiness was either not good or was not perceived as such at all, the experience giving rise to a rough and scratching effect. Sometimes a rough stickiness was reported when the stimulus broke through the layers of the epidermis in a gradual fashion. The best sticky experiences were realized when the stimulus did not break through, but when the skin was lifted gently and then allowed to drop to its natural position without removing the needle. This called forth the sustained light pressure complex with the characteristic increase in clearness and in intensity and the rather sudden decrease in intensity as the pressure was decreased.

Our attempt to synthetize the areal form of stimulation was still more successful. Ten fine-pointed needles were driven through a piece of cork at an angle of 45° so that the points extruded about 2 mm. They were irregularly spaced at intervals of about 3 mm. from one another. This cork was applied carefully to the fingertip so that the cluster of needle points caught the outer layers of the skin and lifted it upward lightly and gently; as the tension on the skin increased slightly, the separate needle points began breaking through the skin in succession, thus giving a series of releases. The breaking through the skin did not disturb the *O*s so much with the areal arrangement, probably because the tension of other points, which were still under stimulation, modified the experience of the single point that was just then breaking through the skin, thus altering the rough and scratching effect, which attended the breaking through of the single punctiform stimulus. Here all *O*s, J included, reported stickiness in 80% or more of the stimulations. The experiences were described as belonging for the most part to the dry sticky variety. J reported a sticky like tar or asphalt in 85% of the presentations in a total of 20 stimulations. The following are typical reports.

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[4] M. Bentley, The Synthetic Experiment, this JOURNAL, 9, 1900, 414ff.

[5] *Op. cit.*

"That gave a good dry sticky. There was light pull and some elevation of the skin and then the breakaway. It was the kind of sticky that you get when you put your finger on something that has a little library paste on it that has almost dried" (M). "There was lateral pull followed by stickiness. Most of these sticky stimulations have been dry, but that one seemed gummy" (Mc). "That sticky suggested the feeling you get when you step in tar or asphalt on a hot street and then touch your finger to your shoe sole" (J). "There was a slight lateral thrust and pull and then the breakaway. There were several successive releases which gave a dry stickiness" (S). "That was a strong dry sticky. The series of stickies came with quite noticeable intervals of time between them. It felt like glue that had almost dried, but which adhered tenaciously and broke away in little patches" (Z). "That was a typical dry sticky. It had all the usual characteristics of dry stickiness" (Zi).

Conclusion

(1) Stickiness is a simple tactual perception, which consists of a field of light contact or pressure points, whose attributes show determinate characteristics: the intensity of the light pressures or contacts show a gradual increase and then a rather sudden decrease to a low or zero level of intensity; the duration of every sticky point in the field is noticeably sustained; and the point of maximal clearness shifts constantly and rapidly among the different pressures or contacts constituting the pattern.

(2) The term stickiness, as generally used, includes the deeper seated pull as well as the superficial light contacts which are set up when the sticky stimulus separates from the skin; but on introspective analysis these two experiences are denoted as distinctly separate and independent of one another, although the two are usually aroused conjointly.

(3) Stickiness is aroused by a moving stimulus only. Moveis not an intrinsic conscious moment in the perception, although it is consciously represented in most of the stronger experiences.

(4) The different kinds of stickiness have their basis partly in differences in the attributive combinations making up the pattern, and partly in visual associations. The former factor seems to be the more pronounced basis for these qualitative distinctions of stickiness.

(5) The perception of stickiness shows no essential alteration whether the member stimulated is active or passive.

(6) A sticky stimulus applied to a portion of the skin that has been rendered anaesthetic by ether fails to arouse the perception of stickiness.

(7) A light areal elevation of the skin is mistaken for a light pressure, and conversely a light pressure is perceived as a lift on the skin, as frequently as 30-50 % of the stimulations. At higher intensities of pull or pressure these confusions occur much less frequently.

AN EXPERIMENTAL STUDY OF AUDITORY TIED-IMAGES

By FORREST L. DIMMICK, University of Michigan

It is a common observation that, in listening to a speaker whose enunciation is not distinct, one fills in the confused or inaudible parts of his discourse. To the similar phenomenon in vision Titchener[1] gives the name "Tied-Imagery"; and a recent study by Zigler[2] includes an experimental investigation of the visual tied-image. The problem which we have undertaken is to determine the nature of the supplementation that takes place in the case of an incomplete auditory perception.

The term "tied-image" first appeared in a note by Titchener[3] in 1914. A term which has a similar form in German—*gebunden*—and which designates much the same class of images as does Titchener's "tied-image" is used by Höffding,[4] but is translated into English by M. E. Lowndes as "implicate." In his classification of images Titchener places tied-images in a group by themselves. Other authors make use of examples of tied-images, without specifically naming them, to illustrate the alteration or correction of our experience under the perceptive attitude.[5]

Procedure I.—For simple auditory forms we chose the opening phrases of familiar tunes, *e.g.*, Auld Lang Syne, Annie Laurie, etc. These were played softly on a piano whose strings had been closely damped to prevent any after-ringing of the notes. The *O*s were instructed: "I am going to play a short familiar musical phrase. I want you to listen carefully and to describe your experience from the beginning of the phrase until I say 'Now'." The phrase was not played completely, but was stopped short by several notes. When the time that would have been required to complete the phrase had elapsed, *E* said "Now."

The same phrase was repeated 5 times; a second phrase was then chosen; and the procedure was repeated until all of the phrases had been presented 5 times to every *O*. *O* sat with his back to the piano, and at the preliminary signal closed his eyes until his report had been completed.

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[1] E. B. Titchener, Beginner's Psychology, 1915, 75.

[2] M. J. Zigler, An Experimental Study of Visual Form, this JOURNAL, 1920, 273.

[3] E. B. Titchener, A Demonstration of "Tied-Images," this JOURNAL, XXV, 1914, 300.

[4] H. Höffding, Outlines of Psychology, trans. by Mary E. Lowndes, 1891, 124.

[5] *E g.*, W. B. Pillsbury, Essentials of Psychology, 1920, 170ff.; J. Jastrow, Fact and Fable in Psychology, 1900, 279ff.

The five *Os* who served were: G, R, T, students in elementary psychology; J. F. Finlayson, instructor in psychology, and Dr. C. C. Dimmick.

Results.—In their early reports all *Os* were taken up with the perception of the musical phrase, or with ideational complexes aroused by it. T, *e.g.*, described the perception as a series of notes thus: "I heard a full long tone, then a shorter tone of the same pitch; then a rise of two whole tones or more and a long and a short note played at that pitch; then another smaller rise in pitch by two more notes." F said, when "America" was played: "I had a feeling of patriotism and a realization of the appropriateness of rising." Reports of this sort were given during the first two or three observation-hours.

The next step which all *Os* made was to note the incompleteness of the phrase, as for example: G "6 notes ascending by pairs; 3 more should have gone with them"; F "I was conscious of an annoyance that you stopped short." Immediately with the noting of the incompleteness of the phrases appeared tendencies to fill them in. F "I hear the next 3 notes of the phrase but they are less full, hollow." G "I heard 5 or 6 notes and expected one more; that is, I heard it without its being played." R "I heard the last 2 notes as plainly as the ones you played." T "I thought I heard the high note [which was not played] but not as loudly as the others."

The supplied notes were usually distinguished for the *O* by being less loud and less rich. It soon appeared that the *O* did not, however, distinguish accurately the supplied notes from the preceding played notes. All *Os* reported the number of notes supplied and the pitch of the first one of these. In a large number of cases the number that the *O* was aware of supplying was one less than the number actually omitted. The first supplied note thus seems to have been indistinguishable from the played notes. This conclusion is substantiated in the second procedure. The percent of such confusions was for F 8%, for G 10%, for R 12%, and for T 20%.

Procedure II (*a*).—Our next step was to make the incompleteness of the presentation fall within the phrase, rather than at the end, and to reduce it to a single note. The experimental conditions were the same as before, and the *Os* were instructed as follows: "I shall play a short, familiar musical phrase which will be incomplete. I want you to note whether you supply the omitted notes in imagery and, if you do, to describe them as completely as you can."

Results.—The reports under these conditions substantiate the results under Procedure I. With the exception of a very few cases, most of which were accredited at the time to a lapse of attention, the *O*s always filled in the missing note. Confusions with the actually played notes occurred, but were not so frequent as with Procedure I. For G the notes following the break came in and cut off the supplied note. T compared the supplied note with the succeeding one, and was thus able to mark it off more surely.

Procedure II (*b*).—We further altered our procedure to obtain a systematic attributive description of the image. To the foregoing instructions (Procedure II) we added the direction to the *O* to report only on specified attributes or perceptive aspects. These were required in the following order: pitch and duration, intensity and clearness, "richness" and "tone-color." The judgments were made in terms of relative likeness to the perception. Every musical phrase was presented 5 times to every *O* for reports on each one of the attributive or perceptive aspects.

TABLE I

	Pitch			Intensity				Duration			
O	−	=	+	V−	−	=	+	V−	−	=	+
D	33	59	8	0	12	30	58	0	28	67	5
F	2	96	2	13	37	43	7	0	17	76	7
G	31	62	7	3	17	47	33	0	47	32	21
R	7	29	64	3	16	31	50	10	28	27	35
T	0	100	0	0	16	50	34	3	17	42	38

	Clearness			"Richness"			"Tone-Color"				Confusions
O	bkgr.	part foc.	foc.	V−	−	=	?	tone	noise	=	
D	0	5	95	0	22	78	16	54	8	22	0
F	13	27	60	7	89	4	0	4	2	94	4
G	2	27	71	0	44	56	0	60	12	28	5
R	0	35	65	3	67	30	5	45	29	21	3
T	1	20	79	4	66	30	8	28	39	25	6

Symbols: V− means 'very much less'; − means 'slightly less (lower)'; = means 'same'; + means 'slightly greater (higher)'; *bkgr.* means in the background of attention; *part foc.* means in focus of attention with other processes; *foc.* means dominant in attention; *tone,* means tone predominated; *noise* means noise predominated.

Values are in percentages of 70 cases for every *O*.

Results.—Table I shows the distribution of the reports under the several categories for all the *O*s. In its attributive aspects the supplied note approaches very closely to the presented notes. Its pitch is very close to the pitch of the omitted note, though 2 *Os* tend to hear it often as a bit flat, and one usually sharps it. However, the sharping or flatting is always characterized as "slightly." In intensity, for all *Os* but one, one third or over of the supplied notes are fully as strong as the presented notes, and 80% or more are not more than slightly less intense than the presented notes. There are only a few cases in which the image was "very weak." F is an exception to this rule, in that his intensity-curve moved in the direction of weakness. The duration of the supplied note approximated that of the omitted note, varying "slightly" above and below. Only 2 *Os*, and these in only a few cases, reported a supplementation "very much" shorter than the omitted note. Clearness was generally maximal, *i.e.*, the supplementation was focal in the majority of cases, though sometimes other processes shared the focus.[6]

In its perceptive aspects the image varies more widely from the presented notes. For 3 *Os* (F,R,T) the "richness" of the image is markedly less. For only one *O* (F) is the "tone-color" or "make-up" of the image the same as that of the played notes. For 3 *Os* (D,G,R) the image was just tone, without the characteristic noises of the instrument; for T the noisy character of the notes predominated. Some *Os* noted further characteristics that could not be brought together under these headings. D calls the image "silvery," "shrill," "all tone," "without any thud," and at other times "dull," "lifeless." Other characterizations are "thin," "rough," "ringing," "singing," "blurred," all of which point to textural differences between the struck note and the imaged note. This discrimination rests upon no single attribute, but upon a peculiar collocation of attributes and upon the perceptive setting. The fact that the *Os* were in many cases unable to distinguish the imaged from the perceived notes indicates that they could have been deceived a great share of the time if we had so desired. We were interested, however, in the tied-image under conditions where it is perceived as an image. The cases of confusion give further indication of the sensation-like character of the auditory tied-image.

Conclusions.—In this investigation we have shown, then, (1) that incomplete auditory perceptions are to a great extent filled in by auditory imagery which we may call "tied-imagery"; (2) that auditory tied-images are probably common to most individuals, since 5 *Os* chosen at random experienced them unmistakably; and (3) that the auditory tied-images are, attributively, closely akin to the corresponding sensations and differentiated from them mainly perceptively.

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[6] The effect of training is evident here. D was a trained *O* at the start. G and T were especially apt, and observed for a longer period than did the other 2 *Os*.

These facts agree closely with the corresponding phenomena in vision. The first two are the basis for the wide use in illustrations and advertisements of incomplete figures. The description of the auditory images agrees with Zigler's[7] description of visual tied-images, which he finds attributively closely like sensation and distinguished from sensation largely by their perceptive setting.

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[7] *Op. cit.*, 279.

SOME NEW APPARATUS

By KARL M. DALLENBACH, Cornell University

I. A KINOHAPT

A modification of Benussi's kinohapt[1] is shown in Figg. 1—4. The apparatus, which may be held in any standard, consists essentially of two parts: a mechanical adjuster, shown in Fig. 1 as viewed from above, and in Fig. 2 as viewed from the side; and a magnetically controlled pressure stimulator, shown in Figg. 2 and 3 as viewed from the side.

It is possible by means of the mechanical adjustment to bring the stimulus to any desired point, since the apparatus may be set in all three dimensions of space. Screw A moves the carriage x, and consequently the stimulus, in one horizontal dimension; screw B moves the carriage y, and consequently the stimulus, in the other horizontal direction; and screw C moves the stimulus vertically.

The rods d_1 and d_2 project through the carriage y and serve to keep the stimulator in a rigid perpendicular position. At the lower end of the rod d_1 a steel spring e is fastened by means of the nut f. At the free end of the spring a gutta-percha block g is secured by two screws h_1 and h_2. Through the center of this block a screw i is threaded, the upper end of which is connected to bar j by means of a pin-hinge k_1, and this bar is connected at the other end to the lever L by a second pin-hinge k_2. The lever L extends back to and through rod d_1, in which it is pivoted, and ends in an iron disc m. Directly above the disc is the soft iron core of the magnet N. This disc is covered with a piece of chamois leather so that the noise of the contact of the magnet and arm is eliminated. The short arm is on the magnet side. It is one-half the length of the long arm; consequently the force exerted by the magnet is reduced, but the length of the excursion of the long arm is increased. To enable the apparatus to be compactly assembled, the short lever is displaced downward by two right-angular turns.

The stimulus point is secured in a hole p bored through the far end of the gutta-percha block by screw q. Since the excursion of the stimulus point parallels that of the end of the long lever arm, and depends upon the distance between the end of the short lever arm at m and the magnet, it may be controlled by adjustment of the screw i. If the gutta-percha block is removed from the steel spring e by unloosing the screws h_1 and h_2, screw i may be raised or lowered by revolving the block. As the height of i is varied so also will the distance between arm m and the magnet be proportionally varied. If the screw i projects far above the block, the arm m will be widely separated from the magnet, and the excursion of the stimulus point (when the magnet is excited) will be correspondingly large; if the screw is down, the separation of the arm and magnet and the consequent excursion of the stimulus will be small.

The excitation of the magnet may be controlled by any timing apparatus which will control the making and the breaking of an electrical current. When the current is made,—the wires being connected to the binding posts o_1 and o_2,—the short arm of lever L is drawn up, and the long arm forced down, moving correspondingly the spring e, thus bringing the stimulus, which previously has been adjusted to the skin by the screws A,

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[1] *Cf. Archiv. f. d. ges. Psych.*, 29, 1913, 386; Benussi most frequently used the name 'kinematohapt,' but he at times abbreviated it to 'kinohapt.' We prefer and have therefore adopted the shorter term.

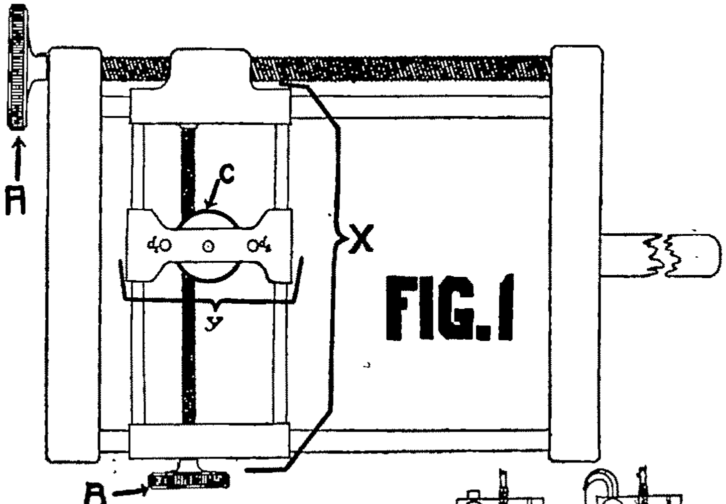

FIG. 1

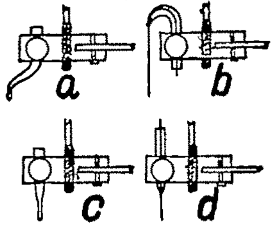

FIG. 4

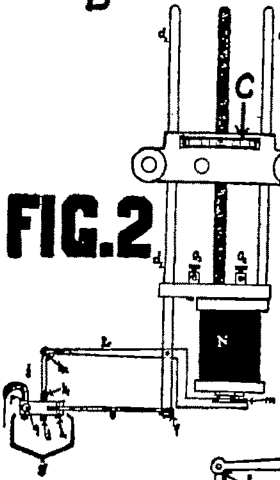

FIG. 2

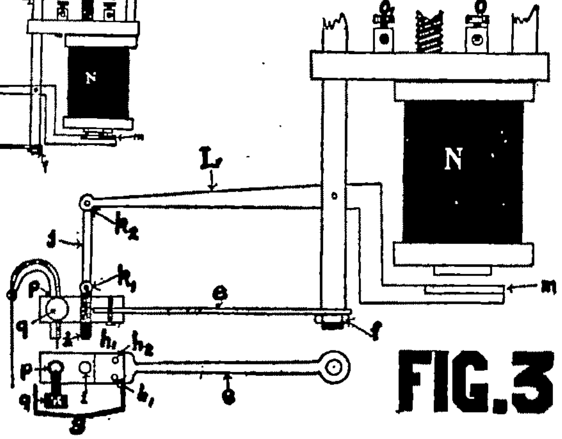

FIG. 3

B, and *C*, into contact with the point desired. As long as the current is made the stimulus will remain in contact. As soon as it is broken the magnet will be released and the lever arm and stimulus will be returned to their original positions by the tension of spring *e*.

The apparatus is adapted for sub-cutaneous as well as for cutaneous work; all that is necessary is a change of stimulus points. If sub-cutaneous stimulation is desired small wood, rubber or metal stimulators may be inserted in the holder at the end of the block; if cutaneous stimulation is preferred a hair aesthesiometer may be substituted.[2]

As many of these units may be used together as needed. The stimuli may be applied for any duration, in any sequence, and as often and at any rate or rhythm desired,—according to the arrangement and variability of the timing apparatus employed.

When isolated spots or widely separated areas are used, straight stimulus points such as are shown in Fig. 4, *c* and *d* are taken; but when neighboring spots or very small spatial intervals are desired, projecting stimulus points, such as are shown in Fig. 4, *a* and *b*, must be chosen. The shanks or butts of these points, 4 *a* for the subcutaneous and 4 *b* for the cutaneous, are so shaped that the stimulus points may, when two or more such units are employed in an investigation, be juxtaposed.

Sharp points may be substituted for the blunt, and the apparatus used in the investigations of the sensation of pain. The apparatus is therefore particularly adapted for punctiform stimulation in which control of the temporal moments and of the gradations of intensity is desired.

The apparatus has been used in a number of studies in this laboratory, the following of which have come to publication: A. K. Whitchurch, The Illusory Perception of Movement on the Skin, this JOURNAL, 32, 1921, 472-489; H. M. Lufkin, Cutaneous Localization and the "Attribute of Order," *ibid.*, 33, 1922, 128-135; W. A. Andrews, Haptical Illusions of Movement, *ibid.*, 33, 1922, 277-285.

II. A THERMAL STIMULATOR

A thermal stimulator, designed for punctiform stimulation, but equally well adapted for moving linear stimulation, is shown in outline in Fig. 5.

The stimulus-point *A*, a copper cylinder of 1 mm. diam. and 8 mm. length, pierces the point of the cone-shaped temperature chamber *B*. It projects 3 mm. beyond the end of the chamber, and (to increase conductivity) approximately 5 mm. within it. The temperature of the point is controlled by the temperature of the water flowing through the chamber. The stream is carried to *B* by tube *C*, and away by tubes D_1 and D_2. Tube *C* extends well within the housing, so that the current of water bathes the upper end of the stimulus-point.

The conducting tubes are shaped to form the frame-work for the support of the handle of the apparatus. The handle *F* moves freely upon the central tube *C*. It is made of gutta-percha, which serves to insulate the incoming water from the body heat of the fingers. The handle is tapered at its lower end in order that its position upon tube *C*, as registered on scale *H*, may be the more readily observed.

The pressure of the stimulus-point upon the skin is controlled, as in an improved Jastrow's aesthesiometer described by C. L. Friedline (this JOURNAL, 29, 1918, 402), by spring *G* which lies between the handle and rod *E*. Rod *E* not only serves as the upper carriage of the spring but also, by connecting the three tubes, serves to strengthen the apparatus. The weight of the stimulator is carried by the spring. Gradually, as the stimulus is applied by lowering the handle *F*, the weight is transferred to the skin.

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[2] For aesthesiometers we used horse hairs which were fastened into small glass tubes by sealing wax and shortened or lengthened according to the intensity of the surface tension desired. *Cf.* Fig. 4, *b* and *d*.

FIG. 5

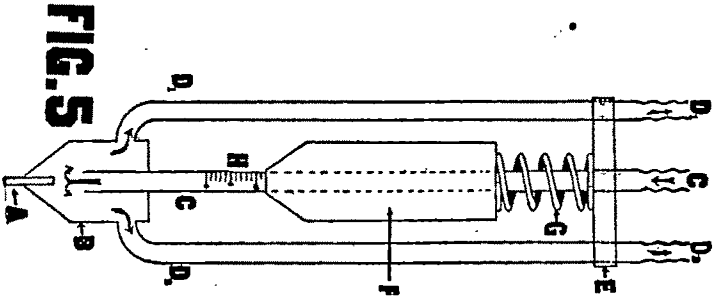


FIG. 6

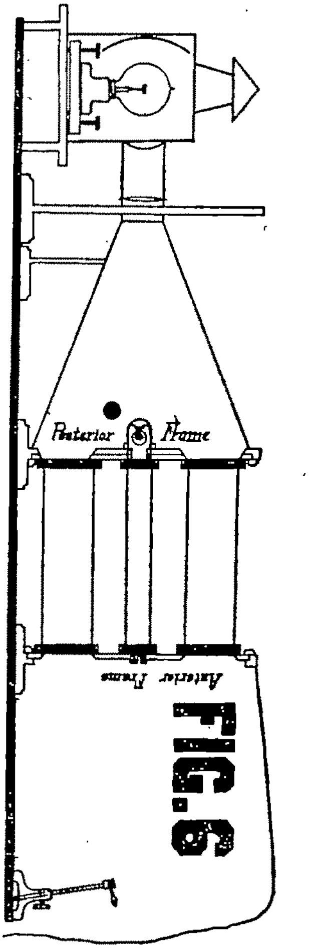
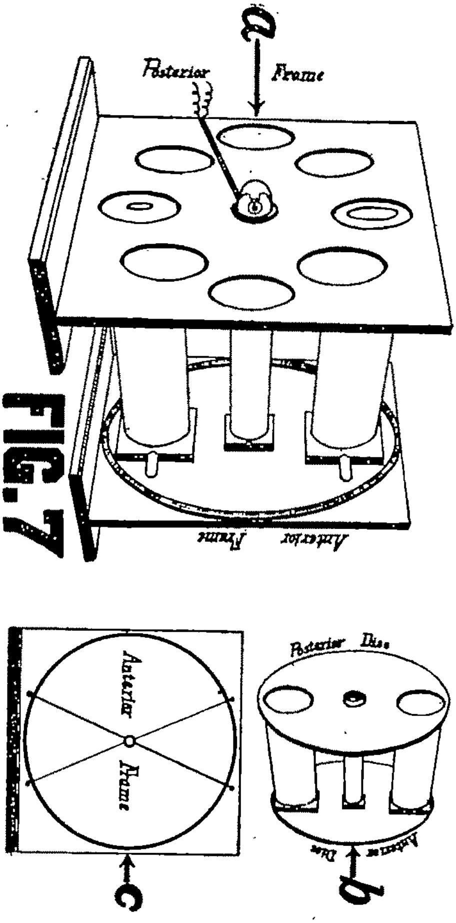


FIG. 7



The amount of pressure upon the skin is, therefore, within the limits of the weight of the apparatus, a function of the amount of the release of the spring,—a function of the distance that handle *F* is lowered on scale *H*. If a pressure greater than that of the weight of the apparatus is desired it may be obtained by placing small circular weights around *C* on rod *E*; if a pressure less than the weight of the instrument is desired it may be obtained and measured, as in the Griesbach aesthesiometer, by controlling the movement of the handle *F* on scale *H*.

The principal advantages of this apparatus are as follows:

(1) In the Willyoung, Kiesow, and von Frey thermaesthesiometers the rubber conduits are attached to the apparatus near the bottom; our use of the metal conduits as a framework for the support of the handle permits the transfer of the rubber tubes to the top where they are out of the way.

(2) The use of the weights and scale *H* do away with the necessity of employing springs of different tension (as in the Friedline instrument) for variable amounts of pressure stimulation.

(3) The stimulus may, if the cylinder is applied vertically and if the surfaces between the handle *F* and tube *C* are kept clean and well oiled, be applied at practically a constant pressure. If used punctiformly the pressure may, by controlling the position that handle *F* is lowered on scale *H*, be kept constant from experiment to experiment; and similarly, it may, if the apparatus be used by the continuous method, be kept constant from moment to moment.

(4) Stimulation by radiation, if not entirely eliminated, is reduced to a minimum; the housing of chamber *B* is small and by means of the projecting point *A* is removed from the skin. The short time that the point rests upon any single part of the skin, with either method of procedure, is not sufficient with this apparatus to stimulate the spots by radiation.

(5) The projecting point also permits of more accurate localization; the point of the stimulus is not obscured by the housing, so that the position of the point at the time of *O*'s report may be exactly recorded.

III. AN APPARATUS FOR THE STUDY OF THE CONDITIONS OF CLEARNESS

An apparatus for the study, and equation of the conditions of clearness, attributive and cognitive, which has gradually been evolved in the Cornell Laboratory[3] is shown in its complete form in Figg. 6 and 7.

The apparatus, as shown in profile in Fig. 6, consists essentially of three parts: (1) a projection lantern with tachistoscopic attachment; (2) a diffusion chamber; and (3) an observational booth.

O sits in dark adaptation in a light-proof booth with his head held firmly in position by a biting board so adjusted that the fixation-point extended will fall midway between his two eyes. The biting board eliminates involuntary head movements and insures a constancy of position from experiment to experiment. The fixation point is 1 m. away from the eyes and 1 mm. in diam.

Two areas on opposite sides of the fixation point are simultaneously illuminated. The shape, size, position, quality and intensity of these areas may be varied concomitantly or independently. The shapes may, of course, be novel or familiar. The fixation point is held by the *O*, who reports after the exposure which of the two areas, differing from one another in one or more of these dimensions, is the more clear.

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[3] *Cf.* L. G. Meads, *Form vs Intensity as a Determinant of Attention*, this JOURNAL, 26, 1915, 150; J. N. Curtis and W. S. Foster, *Size vs Intensity as a Determinant of Attention*, *ibid.*, 28, 1917, 293; A. M. Bowman, *Size vs Intensity as a Determinant of Attention*, *ibid.*, 31, 1920, 87.

Variation of the dimensions mentioned is made possible by what we have chosen to call the diffusion chamber. This chamber, the essential feature of the apparatus, consists, as is shown in outline in Fig. 6 and in detail in the drawings of Fig. 7, of two parallel screens and a rotating drum. The anterior screen, the one facing the *O*, Fig. 7c, is merely a frame which provides a hub and rim for the drum. The opposite screen, Fig. 7a, is solid except for 8 circular windows spaced 45° apart and at a uniform distance from the center. At the center there is a small circular opening which serves a dual purpose; it acts as a boxing for the posterior axle of the drum, and as a window through which the light from a small electric lamp placed in the housing before the hole is transmitted to the fixation point.

The drum, Fig. 7b, is composed of two parallel discs and three connecting tubes. There are three circular windows in each disc; a small one at the centers and two large ones at opposite edges. The holes in the two discs are opposed and connected by the three tubes. The small central tube is extended beyond each disc so that it forms the axle on which the drum is rotated.

At each end of every tube there are two plate or shutter holders. At the anterior ends, the ends near the *O*, ground glass plates are placed in the first holders, and metal shutters, which govern the size, shape, and quality of the exposed areas, are inserted, as desired, in the second holders. At the posterior ends, there is a second set of ground-glass plates which diffuse the light, and a second set of metal shutters which control the amount of light that falls upon these plates. The positions of the plates and shutters are shown in the drawings in Figg. 6 and 7; the ground-glass plates are represented by cross-hatchings and the shutters by straight lines. In Fig. 7a both the anterior and posterior shutters are shown in place; the anterior shutters as seen from the back as they project from the holder, and the posterior shutters as observed through the two windows in the posterior screen.

When the drum is placed in position in the frames the light from the lantern is transmitted through the two large tubes and illuminates the ground-glass plates fronting the *O*. The angular position of these areas to the fixation point is determined by the position of the drum, and this is controlled in relation to the windows in the posterior frame by a scale and marker which appears on the anterior side of the posterior frame and disc.

The illumination is derived from a 100-watt concentrated filament Mazda lamp placed in a projection lantern. The position of the lamp in respect to the lenses, and the focal distance of the lenses, are so adjusted that the disc of light projected upon the face of the diffusion screen is of uniform intensity over its entire surface.

Between the lantern and the funnel, which projects from the lantern to the diffusion chamber, is a gravity tachistoscope—an adaptation of Wundt's apparatus—which controls the duration of the exposure.

All the interior surfaces of the apparatus are enameled white so as to reduce the absorption of light to a minimum.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

XLV. ABSOLUTE JUDGMENTS OF CHARACTER TRAITS IN SELF AND IN OTHERS

By MARGARET F. WASHBURN and VLASTA STEPANOVA

H. L. Hollingworth reports in Ch. VII of his "Vocational Psychology" the results of experiments made on Barnard students, in which members of groups of 25 persons arranged themselves in order of merit with reference to the following traits: neatness, intelligence, humor, conceit, beauty, vulgarity, snobbishness, refinement, sociability. The results related to the following problems: (1) the agreement of self-estimates with estimates by associates; (2) the existence of tendencies toward overestimation of a given trait in oneself; (3) the relation between possession of a trait and ability to judge it in others. In addition to the estimates, the results of certain mental tests were taken into consideration.

The method of the present Study differed in two important respects from Hollingworth's method. In the first place, our observers, instead of comparing the members of a group among each other and arranging them in order of merit, thus using a relative standard, made absolute judgments of the presence or absence of certain traits. In the second place, we used a set of traits in which the factor of complimentariness or uncomplimentariness was reduced to a minimum; Hollingworth's results showed that this factor was of decided influence. We selected our list from the traits used in the Downey Will Profile Test, as follows: careful—careless, ambitious—unambitious, punctual—tardy, industrious—not-industrious, enthusiastic—indifferent, self-confident—self-distrustful, impulsive—deliberate, orderly—disorderly, optimistic—pessimistic, patient—impatient, aggressive—unaggressive, suggestible—not-suggestible, thrifty—extravagant. Every member of a group was asked to judge herself and every other member of the group by checking one member of each pair of traits in the case of each person judged.

There were three groups of *O*s: two of these comprised ten persons each, who had been living in close proximity in a college dormitory and were friends and companions.[1] The experiment was performed on each of these groups in the following way. The members sat with paper and pencil, and each wrote the name of one of the group members: the list of traits was then read by the *E*, and each person noted down that trait of a pair which in her judgment characterized the individual she had chosen to judge. The process was repeated until every person had judged every member of the group, including herself, with reference to every pair of traits. The nine members of the third group did not perform the experiment simultaneously: the *E* went to them individually. They were probably not so intimately acquainted with one another as those in Groups I and II, as they had not actually lived together. This difference in the method used with Group III was not accompanied by any apparent difference in the results: as will be seen, Group I and III gave results more closely parallel than did Groups I and II.

Our results go to show first that, using this method of making absolute rather than comparative judgments, and judging with reference to the list of characteristics given above ,containing no very unfavorable traits, *our Os tended to judge others as they judged themselves.*

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[1] The experiments on this group were performed by Miss Frances Rudy.

The evidence is as follows. In each of the three groups, the number of persons judging themselves to have a positive trait was counted. Each of these persons had judged a certain number of her companions to possess this positive trait. These latter numbers were added, giving the sum of all positive judgments on others made by the persons in that group who judged themselves positively. This sum was then divided by the number of such persons: the quotient represented the average number of times that an individual in the group who judged herself positively judged others positively. The same procedure was then followed in the case of the members of the group who had judged themselves to possess the corresponding negative trait; that is, the average number of times was found that an individual in the group who judged herself negatively judged others positively. Evidently, if this second average was lower than the first, there was a tendency for the members of the group to judge others as they judged themselves.

The figures were as follows.

Carefulness. Group I, average number of positive judgments on others: positive *Os* (self-judged), 8; negative *Os*, 6.3. Group II, positive *Os*, 6.6; negative *Os*, 6.7. Group III, positive *Os*, 8.2; negative *Os*, 6.4.

Ambition. Group I, positive *Os*, 7; negative *Os*, 4.7. Group II, positive *Os*, 6.5; negative *Os*, 5.5. Group III, positive *Os*, 8; negative *Os*, 6.

Punctuality. Group I, positive *Os*, 8.1; negative, 6.6. Group II, positive *Os*, 7; negative, 8. Group III, positive *Os*, 8; negative, 5.1.

Industry. Group I, positive *Os*, 9.1; negative, 7. Group II, positive *Os*, 7; negative, 7. Group III, positive *Os*, 7.7; negative, 6.5.

Enthusiasm. Group I, positive *Os*, 7.2; negative, 5.6. Group II, positive *Os*, 6.8; negative, 4.6. Group III, positive *Os*, 7.6; negative, 8 (only one case).

Self-confidence. Group I, positive *Os*, 6.5; negative, 5.1. Group II, positive *Os*, 6.5; negative, 6.1. Group III, positive *Os*, 4.8; negative, 5.5.

Impulsiveness. Group I, positive *Os*, 6.5; negative, 2.1. Group II, positive *Os*, 3.2; negative, 3.3. Group III, positive *Os*, 3.2; negative, 3.3.

Orderliness. Group I, positive *Os*, 9.5; negative, 5.5. Group II, positive *Os*, 7.7; negative, 6.5. Group III, positive *Os*, 7.5; negative, 4.8.

Optimism. Group I, positive *Os*, 6.7; negative, 7 (two cases). Group II, positive *Os*, 8: negative, 7.2. Group III, positive *Os*, 7.6; negative, no cases.

Patience. Group I, positive *Os*, 7.5; negative, 5. Group II, positive *Os*, 4.3; negative, 4.7. Group III, positive *Os*, 7; negative, 5.4.

Aggressiveness. Group I, positive *Os*, 5 (two cases); negative, 2.4. Group II, positive *Os*, 5.5 (two cases); negative, 4.2. Group III, positive *Os*, 5; negative, 1.6.

Suggestibility. Group I, positive *Os*, 6.1; negative, 5.5. Group II, positive *Os*, 6.3; negative, 4(one case). Group III, positive *Os*, 5.3; negative, 5 (two cases).

Thrift. Group I, positive *Os*, 9.5; negative, 4.7. Group II, positive *Os*, 7; negative, 6.8. Group III, positive *Os*, 7.3; negative, 5.6.

It will be noted that the tendency to judge others as we judge ourselves is less marked in Group II than in the other two groups. In Group II, in 5 out of the 13 traits, there is no indication of such a tendency. Group III fails to show it in the case of two traits; and Group I in the case of one trait.

There is probably no significance in the relative strength of the tendency in different traits as indicated by the figures: the number of cases is too small. The existence of a general tendency however seems apparent, and there seems to be also a tendency to avoid judging others as impulsive or aggressive, whether one judges oneself so or not.

Our data relate also to a second problem, namely: Is a person who judges herself to have a certain positive trait judged to possess it by the majority of her group? This is one of Hollingworth's problems: do we tend to overestimate or underestimate ourselves in relation to certain traits? Our method involves a comparision of ourselves not with other individuals, but with a certain mental standard, necessarily vague.

To answer this second question, we proceeded with the data as follows. Each person in a given group who had judged herself to possess a certain positive trait had been judged by a certain number of her companions to possess this trait. These numbers were added, giving the sum of all positive judgments made by others on the members of that group who had judged themselves positively. This sum was divided by the number of such members, and the quotient represented the average number of persons who had judged positively an individual in the group who had judged herself positively. The same procedure was followed in the case of those members of the group who had judged themselves negatively; that is, the average number of persons was found who had judged positively an individual in the group who had judged herself negatively. When these averages were higher than 5 (since the size of the groups was 10, 10, and 9 respectively), a majority judgment of possession of the positive trait was indicated. The figures follow.

Carefulness. Group I, average number of positive judgments received from others: positive *Os* (self-judged), 6.3; negative *Os*, 5.6. Group II, positive *Os*, 8.6; negative, 5.4. Group III, positive *Os*, 7.7 ; negative, 6.8.

Ambition. Group I, positive *Os*, 6.8; negative, 4. Group II, positive *Os*, 7.4; negative, 5. Group III, positive *Os*, 7.7; negative, 7.

Punctuality. Group I, positive *Os*, 7.7; negative, 7.6. Group II, positive *Os*, 8.8; negative, 3.6. Group III, positive *Os*, 6.6; negative, 5.8.

Industry. Group I, positive *Os*. 8.3; negative, 7.5. Group II, positive *Os*, 8; negative, 5.5. Group III, positive *Os*, 7.8; negative, 6.

Enthusiasm. Group I, positive *Os*, 6.4; negative, 6.4. Group II, positive *Os*, 8; negative, 3.3. Group III, positive *Os*, 7.7; negative, 7.

Self-confidence. Group I, positive *Os*, 7.5; negative, 4.8. Group II, positive *Os*, 9; negative, 5.4. Group III, positive *Os*, 5.6; negative, 4.

Impulsiveness. Group I, positive *Os*, 4; negative, 3.6. Group II, positive *Os*, 8; negative, 2.1. Group III, positive *Os*, 4; negative, 3.3.

Orderliness. Group I, positive *Os*, 7.2; negative, 6.8. Group II, positive *Os*, 7.2; negative, 6.1. Group III, positive *Os*, 7.2; negative, 5.

Optimism. Group I, positive *Os*, 6.5; negative, 8. Group II, positive *Os*, 8; negative, 7.4. Group III, positive *Os*, 6.9; negative, no cases.

Patience. Group I, positive *Os*, 7.5; negative, 6. Group II, positive *Os*, 8.3; negative, 3. Group III, positive *Os*, 6.7; negative, 5.6.

Aggressiveness. Group I, positive *Os*, 3; negative, 2.8. Group II, positive *Os*, 8.5; negative, 3.6. Group III, positive *Os*, 4.5; negative, 3.3.

Suggestibility. Group I, positive *Os*, 6,1; negative, 5.2. Group II, positive *Os*, 8.5; negative, 3. Group III, positive *Os*, 5.7; negative, 3.5.

Thrift. Group I, positive *Os*, 6.5; negative, 7. Group II, positive *Os*, 7; negative, 4.8. Group III, positive *Os*, 7.5; negative, 5.3.

It will be seen that our *Os* in all three groups underestimated their possession of the following traits, in the judgment of their companions: carefulness, industry, orderliness. Optimism would probably show the same tendency, but for the circumstances that no member of this group judged herself to be pessimistic. The members of Groups I and III underestimated their punctuality, their enthusiasm, their patience and their thrift. On the other hand there was a tendency in Groups I and III for the members to overestimate their own impulsiveness and aggressiveness.

Group II differed from the other two groups in the relatively high degree of unanimity among its *Os*. In Groups I and III there was not a single person as to whose lack of any positive trait her companions were unanimous. In Group II their were 15 such cases of unanimous negative judgments. Nobody thought *O* II of Group II thrifty nor *O* III impulsive or aggressive, nor *O* IV self-confident, impulsive, or aggressive, nor *O* IV patient, nor *O* VIII enthusiastic, impulsive, or suggestible, nor *O* IX careful, punctual, patient, or thrifty, nor *O* X self-confident. The case was analogous as regards unanimity of positive judgments. In Group I there were 6 such cases: everybody thought *Os* A and F punctual, *Os* A and H industrious, *O* A suggestible and thrifty. In Group III there were no cases of unanimously positive judgments. In Group II, on the other hand, there were no fewer than 27 cases in which all the judges agreed in ascribing a positive characteristic to an individual. *O* I was agreed to be ambitious, self-confident, optimistic, and thrifty; *O* II to be ambitious and enthusiastic; *O* III to be careful, punctual, orderly, and thrifty; *O* IV to be patient, suggestible, and thrifty; *O* V to be punctual and industrious; *O* VI to be careful, punctual, industrious, and orderly; *O* VII to be ambitious, self-confident, enthusiastic, and orderly; *O* IX to be enthusiastic, and impulsive; *O* X to be ambitious. This unanimity in Group II may have been due to either or both of the following reasons: its members were persons of more marked traits than those of Group I; they were accurate judges of one another's character.

Two important influences suggest themselves as affecting the relation between judgments of self and judgments of others on oneself when made thus 'absolutely'. The first relates to whether the trait judged is one that is habitually repressed in society. People do not give free social expression, as a rule, to their pessimism, their impatience, their impulsiveness, and their aggressiveness. Hence, as our results show, when judging themselves absolutely they regard themselves as more pessimistic, more impatient, more impulsive and more aggressive than their companions think them. Secondly, traits like carefulness, punctuality, industry, and orderliness, refer to an ideal standard, which will be higher in proportion as a person's performance judged by others is more excellent. In other words, a really careful, industrious, orderly, and punctual person is not satisfied with his own performance. Hence our results show a tendency to underestimate one's own possession of these traits when one's own judgments are compared with those of one's companions on oneself.

XLVI. A STUDY OF REVIVED EMOTIONS

By MARGARET F. WASHBURN, RUTH FIELD and ELAINE D. WOLF

The object of this Study was to gather data concerning the speed and intensity with which anger, fear, and joy can be revived under instructions to revive them, and the relation of these data to the remoteness in time of the original occurrence of the emotion. So far as we know, this is the first attempt to investigate such a problem. We also collected data regarding the bodily expressions of revived emotion.

Our *Os*, on whom the experiment was performed individually, were 90 young women college students. The following instructions were given them: "I want you to recall some occasion when you were very angry. Let the anger develop again as far as it will, so that as far as possible you relive the incident and its accompanying anger. Do not speak, but give a signal when you feel the anger as fully as you can, in this revived form."

A stop-watch was started as soon as the *O* understood the request and began to fulfill it, and was stopped when she gave the signal. The time was recorded, together with observations on the *O*'s attitude and facial expression during the process of recall. The *O* was then asked the following questions, and her answers recorded.

"1. Did you feel, in recalling the anger, any sensations from your own body, such as those of quick breathing, flushing, stiffening of the muscles, and so forth? Recall the anger again, if necessary, to describe these sensations.

2. Is the recalled anger as intense as the original anger, somewhat less intense, very much less intense, or do you only remember that you were angry, without being angry again?

3. How long ago did the incident occur?

4. Indicate which of the following causes occasioned the anger: humiliation, injustice, cruelty, being interfered with. Was the injury to yourself or to another?" (The answers to this question are not considered in the present report).

After the *O* had answered these questions a similar procedure was followed for fear. The instructions were: "I want you to recall, silently, an occasion on which you were very much frightened. Recall it fully, and re-live, as far as possible, the fear, giving a signal when you have succeeded." The time was recorded as before, the facial expression and bodily attitude noted, and the answers to the following questions.

"1. Did you feel, in recalling the fear, bodily sensations? Recall it again if necessary to describe those sensations.

2. Is the recalled fear as intense as the original fear, somewhat less intense, very much less intense, or do you only remember that you were frightened?

3. How long ago did the incident occur?"

Thirdly, a similar procedure was followed for joy. The instructions were: "Now recall, silently, an occasion when you felt great joy. Recall it fully and re-live, so far as possible, the joy, giving a signal when you have succeeded."

The time of recall was recorded, the facial expression and bodily attitude noted, and the answers of the *O* to the following questions.

"1. Did you feel actual bodily sensations in recalling the joy? Recall it again if necessary to describe those sensations.

2. Is the recalled joy as intense as the original joy, somewhat less intense, very much less intense, or do you only remember that you felt joy?

3. How long ago did the incident occur?"

In the case of the next *O* the order of recall was: anger, joy, fear; for the next, fear, joy, anger; for the next, fear, anger, joy; for the next, joy, anger, fear; for the next, joy, fear, anger. This variation of order was repeated with succeeding *O*s, to eliminate the influence of fatigue.

Results. Our data, setting aside for the present the observations on bodily manifestations, relate to three variables: the intensity of the revived emotion, the speed of the revival, and remoteness of the original occurrence of the emotion. As an indication of the intensity of the revival, we had the statements of the *O*s, using four grades, ranging between "as intense as the original emotion" to merely remembering that one had felt the emotion. As a measure of speed of revival, we had the stop-watch record of the time between giving the instruction to recall and *O*'s signal that the recall was complete. Obviously there was no way of insuring that different *O*s would give the signal at the same stage of completeness of the recall-process. We therefore made no statistical use of the actual times, but as will be seen made inferences only from the relative speed with which a given *O* recalled the three emotions. Similarly, in considering the third variable, the length of time since the original emotion was experienced, we took account not of the absolute length of the interval, *e.g.*, two months, a week, five years, but merely of the relative length, noting which of the three emotions, in the case of a given *O*, dated from the most remote period and which from the most recent.

It is evident that our three variables are interrelated in such a way as to complicate the statement of results. For example, we cannot simply average the times of recall for each of the three emotions (even if we could rely on the accuracy of these times), and state that the emotion having the smallest average time of recall is revived more readily than the others; for the original occurrences of this emotion might have tended to be of more recent date, and we should expect this fact to have an effect on the speed of revival. Similarly, if we simply counted the number of cases in which a given emotion was recalled more intensely than the other three, and took these sums as indicating the relative intensity with which these emotions can be revived, we should again be neglecting the important factor of the recency of the original experience.

It is essential to begin with, therefore, that we should consider the relative recency of the original occurrence of the emotions.

In the case of anger, the original occurrence was the most remote of the three in 25 instances; it occupied a middle position in 27 instances; and was the most recent of the three in 38 instances.

In the case of fear, the original occurrence was the most remote of the three in 54 instances; it occupied a middle position in 20 instances; and was the most recent in 16.

In the case of joy, the original occurrence was the most remote in 14 instances; occupied a middle position in 47 instances; and was the most recent in 29.

It may be assumed that the remoteness of time from which an emotional experience is recalled depends upon, first, the relative frequency with which this type of emotion occurs; and secondly, the persistence of its memory. Our results show that fear dates from further back than either anger or joy. This may be explained both by the comparative rarity of the experience of fear, and by its greater frequency in childhood. Anger tends to be of more recent date than joy; whether because it is more frequent, or because the memory of earlier angers falls off more readily than that of earlier joys, does not appear.

These figures regarding the time-position of the original occurrence of the emotion may now form the basis of our other data.

Of the cases of anger recalled from most recent time, 55% were very intense revivals; of the cases of fear recalled from most recent time, 50% were very intense revivals; of the cases of joy recalled from most recent time, 62% were very intense revivals. The average of these three percentages is 55.6%.

Of the cases of anger recalled from middle position, 34% were very intense revivals; of the cases of fear recalled from middle position, 28% were very intense revivals; of the cases of joy recalled from middle position, 61% were very intense revivals. The average of these three percentages is 41%.

Of the cases of anger recalled from the most remote position, 16% were very intense revivals; of the cases of fear recalled from most remote position, 30% were very intense revivals; of the cases of joy recalled from most remote position, 45% were very intense revivals. The average of these three percentages is 30%.

It thus appears that (1), as might be expected, *revivals are less intense the more remote the time position of the original emotion.* This falling-off is not at all rapid, however, as indicated by the series: 55.6, 41, 30. (2) The falling-off is decidedly more marked for anger than for either of the other two (series: 55, 34, 16). *Revived anger falls away in intensity with time more rapidly than either joy or fear.* Joy falls away least rapidly.

(3) *Joy is more intensely revived than either fear or anger.* The sum of the percentages of most intense revivals is for joy, 168; for fear, 108; for anger, 104.

Passing to a consideration of the speed with which the revivals were made, we have the following data.

Of the cases where anger was recalled from the most recent time position, 26% had the longest time of recall of the three emotions, 22% the shortest.

Of the cases where fear was recalled from the most recent time-position, 37% had the longest time of recall and 31% the shortest.

Of the cases where joy was recalled from the most recent time-position, 36% had the longest time of recall and 37% the shortest.

Of the cases where anger was recalled from the middle time-position, 59% had the longest time of recall and 11% the shortest.

Of the cases where fear was recalled from the middle time position, 28% had the longest time of recall and 21% the shortest.

Of the cases where joy was recalled from the middle time-position, 10% had the longest time of recall and 55% the shortest.

Of the cases where anger was recalled from the most remote time-position, 41% had the longest time of recall and 25% the shortest.

Of the cases where fear was recalled from the most remote time- position, 42% had the longest time of recall and 29% the shortest.

Of the cases where joy was recalled from the most remote time-position, 24% had the longest time of recall and 63% the shortest.

From these figures the following conclusions may be drawn. (1) There *is no relation between speed of recall of an emotion and the length of time that has elapsed since the original occurrence of the emotion.* That is to say, the time of recall does not increase as the distance of the original experience increases. In the case of joy, the reverse relation almost seems to be true.

(2) *Joy is more quickly recalled than either fear or anger.* The sum of the percentages of quickest recalls is for joy, 155; for fear, 81, for anger 58. The sum of the percentages of slowest recalls is for joy, 70; for fear, 107; for anger, 126. It thus would appear that *anger is most slowly recalled.*

Finally, what is the relation between the speed with which an emotion is revived and the intensity with which it is revived?

There were in all 35 cases where anger was recalled in the most intense degree. Of these 28% were also the emotions most quickly recalled, and 28% the emotions most slowly recalled.

There were in all 29 cases where fear was recalled in the most intense degree. Of these 32% were the emotions most quickly recalled, and 43% the emotions most slowly recalled.

There were in all 53 cases where joy was revived in the most intense degree. Of these 53% were the emotions most quickly recalled, and 21% the emotions most slowly recalled.

Hence it appears (1) that *in the case of anger there is no relation between the speed with which the emotion is revived and its intensity when revived;* (2) that *in the case of fear there is perhaps some tendency for the more intense revivals to take longer;* (3) that *in the case of joy there is rather a marked tendency for the more intense revivals to be made quickly.* These conclusions would be in harmony with the often verified fact that *unpleasant experiences are recalled more slowly than pleasant experiences.*

Bodily Manifestations. Under this head, it will be recalled, we have the observations made by the *E* in the appearance and behavior of the *O* and the introspective reports of the *O* on her organic and kinaesthetic sensations. Obviously not much reliance can be placed on either of these sources of information. The chief source of error in the case of both is that it is impossible clearly to separate those bodily manifestations that are due to the effort of attention in recalling the emotion and those that are due to the emotion itself. Although the observations of the *E* and the introspections of the *Os* have been carefully classified, they seem to justify no general conclusion regarding the special bodily reactions belonging to different emotions. For example, 'general muscular contraction' was reported introspectively in the case of joy by 17 *Os*; 'general bodily relaxation' by 26. General muscular contraction was reported in the case of fear by 36 *Os* and in that of anger by 43 *Os*; no one reported relaxation in the case of fear or anger. 'Quickened breathing' was reported with about equal frequency for all three emotions. 'Quickened heart beat' was reported most frequently (28 times) for joy; 10 times for fear, and only twice for anger: revived anger would seem to lack this characteristic.

Perhaps the most trustworthy datum of this part of our investigation concerns the *number* of bodily manifestations reported in the case of the three emotions. Since the processes of attention and effort to recall are probably not essentially different for the different emotions, any difference in the number of bodily manifestations reported is likely to be due to some character of the emotions themselves. The number of instances where a bodily manifestation was introspectively reported by an *O* was for joy, 332; for anger, 314; for fear, 282. The number of instances where a bodily manifestation was observed by the *E* was for joy, 269; for anger, 243; for fear, 220. *More bodily manifestations thus appear in the case of joy.* It will be remembered that joy was more intensely recalled than either of the other emotions. The intensity of recall for fear and anger was about equal; it is *probable*, then, that the greater number of bodily manifestations reported and observed for anger as compared with fear is due to there really being a *greater amount of observable bodily reaction in anger than in fear.*

Finally, when the results involving each of the six different time-orders of recall were compared, allowance being made for the factor of the recency of the original experience, no effects of fatigue appeared. That is, an emotion was not revived any more or less intensely or quickly for being the last or the first of the three emotions in the series as presented by the *E*.

XLVII. THE CORRELATION OF A TEST OF CONTROL OF VISUAL IMAGERY WITH ESTIMATED GEOMETRICAL ABILITY

By MARGARET F. WASHBURN, ELSIE HATT and ELIZABETH B. HOLT

In this JOURNAL, vol. 25, pp. 293-295, there was described a test devised by the senior author of the present Study, of the power to control visual imagery. We have recently given this test to two groups of Vassar College freshmen, and obtained rank-difference correlations with their instructor's estimates of their ability in solid geometry. It would seem probable that such ability might involve as one of its factors the power to control, to move about, as it were, mental images of geometrical forms.

As modified for a group test, the procedure of the test for control of visual imagery was as follows. The experiment was performed at the beginning of an ordinary class hour, in the class room. A mimeographed sheet of problems was laid face downward before each student. The following directions were read aloud: "You see on the blackboard a square divided into sixteen small squares, which are numbered 1 to 16. In the upper left-hand corner square, whose number would be 1, you see a triangle.

"You will be required to imagine that you have on paper before you a square like this, divided into sixteen small squares, and you are to imagine the triangle moved about from one to another of the small squares, according to directions which have been given you. When you have imagined the triangle moved about according to the directions, you are to write down on your paper of directions the number of the square in which the triangle would be at the end of the series of movements. For each new problem the triangle starts out on the upper left-hand corner.

"For example. Suppose your directions read: "2 to right,"—you would imagine the triangle moved to this square (pointing); "1 obliquely down to the left,"—you would imagine it moved to this square; "2 down,"—you would imagine it moved to this square; "1 obliquely up to left,"—you would imagine it moved to this square; "3 to right,"—you would imagine it moved to this square; "2 up,"—you would imagine it moved to this square; "2 obliquely down to left,"—you would imagine it moved to this square; "1 up,"—you would imagine it moved to this square; "1 to left,"—you would imagine it moved to this square; "2 down,"—you would imagine it moved to this square. This being the end of the series of movements directed, you would glance at the large square and write after the directions the number of the square on which the triangle would now be, namely, 13.

"Do not use your pen or pencil at all, not even in gesture, while you are imagining the movements. There are ten problems. Do not look at the square on the board until each problem is completed and you need to know the number of the square in which the triangle is imagined to be. Finish each problem before going on to the next. Write your name on the back of your paper."

After a pause: "When I say, 'Ready!' turn over your papers and begin. Work as rapidly as you can. As soon as you have finished the whole paper, look at the clock and write down the time. On this clock the disk shows the minutes and the hand the seconds. When you have finished, raise your hand and we will take your paper. Have you each a pencil or pen?"

The mimeographed sheet given each student ran as follows:

1. 1 obliquely down to right, 2 to right, 2 down, 1 to left, 3 up, 1 obliquely down to left, 2 down, 2 obliquely up to right, 1 to right.

2. 2 down, 2 obliquely up to right, 3 down, 1 to right, 3 obliquely up to left, 2 obliquely down to right, 1 to left, 1 obliquely up to right, 2 to left, 1 down.

3. 1 down, 2 to right, 1 obliquely down to left, 1 down, 2 obliquely up to right, 1 obliquely up to left, 2 down, 2 to left, 1 obliquely down to right, 2 obliquely up to right.

4. 1 to right, 1 obliquely down to left, 2 down, 2 obliquely up to right, 1 to right, 1 up, 1 obliquely down to left, 1 down, 2 to right, 2 up.

5. 1 obliquely down to right, 2 to right, 1 obliquely down to left, 2 obliquely up to left, 2 down, 1 obliquely down to right, 2 obliquely up to right, 2 to left, 2 obliquely down to right, 2 to left.

6. 2 down, 1 obliquely up to right, 2 to right, 1 up, 2 to left, 1 obliquely down to left, 2 down, 2 to right, 1 obliquely up to right, 2 up.

7. 3 to right, 2 obliquely down to left, 1 to right, 1 down, 1 obliquely up to right, 3 to left, 2 obliquely up to right, 1 to left, 1 to left, 2 down.

8. 2 to right, 1 obliquely down to right, 2 obliquely down to left, 2 up, 1 to left, 2 obliquely down to right, 1 to right, 2 to left, 1 obliquely up to right, 2 up.

9. 2 to right, 1 obliquely down to left, 1 obliquely down to left, 1 down, 2 to right, 1 up, 1 to right, 2 obliquely up to left, 1 to left, 1 down.

10. 2 obliquely down to right, 1 to right, 2 up, 1 obliquely down to left, 2 to left, 2 down, 2 obliquely up to right, 1 to left, 2 down, 1 to left.

Results. The scores made were as follows.

Section I (27 persons). Average time, 14 min. 48 sec.; longest time, 20 min. 20 sec.; shortest time, 8 min. 3 sec. Average number of problems correctly solved, 6.7. Highest number, 10; lowest number, 4.

Average index of efficiency (number of problems correctly solved, divided by total time in sec.), .0079; highest index, .0160; lowest, .0024.

Section II (21 persons). Average time, 13 min. 54 sec.; longest time, 18 min. 15 sec.; shortest time, 8 min. 45 sec. Average number of problems correctly solved, 6.7. Highest number, 10; lowest number, 1. Average index of efficiency, .0087; highest index, .0164; lowest, .0009.

The instructor, Assistant Professor Gertrude Smith, kindly undertook to rank the students in each section according to her judgment of their natural ability for geometrical thinking. We wish to express our gratitude for her co-operation in this and other ways.

The rank-difference coefficients found were as follows.

Section I. Speed and teacher's ranking, +.46, PE .1087; accuracy and teacher's ranking, +.16, PE .1348; speed and accuracy, +.23, PE .1306; efficiency and teacher's ranking, +.50, PE .1037.

Section II. Speed and teacher's ranking, +.36, PE .1362; accuracy and teacher's ranking, +.60, PE .1002; speed and accuracy, +.68, PE .0811; efficiency and teacher's ranking, +.53, PE .1122.

It will be seen that *good correlation is obtained in both sections between efficiency in this test and the teacher's estimate of natural ability in geometry.*

The two sections differed in the relation shown between speed of performance and accuracy. In Section I there were five persons who were very quick but inaccurate. In Section II the quick workers were also accurate. Where, as in Section I, speed and accuracy did not correlate, the instructor's estimate followed speed rather than accuracy.

XLVIII. AFFECTIVE SENSITIVENESS IN POETS AND IN SCIENTIFIC STUDENTS

By M. F. WASHBURN, ELSIE HATT and ELIZABETH B. HOLT

In this JOURNAL, vol. 24, pp. 583-585, the senior author of the present Study suggested as a measure of affective sensitiveness, thati s, the tendency to be strongly affected both in the direction of pleasantness and in that of unpleasantness, the ratio of the sum of the number of judgments of extreme pleasantness and extreme unpleasantness to the number of judgments of indifference, when the method was used of having an *O* express her judgment of pleasantness or unpleasantness by using the numbers 1 to 7, 1 meaning extreme unpleasantness, 7 extreme pleasantness, and 4 indifference.

The object of the present Study was to compare affective sensitiveness in two groups of *Os*, which we shall call the group of poets and the group of scientists. The poets were undergraduate students recommended by the English Department of Vassar as having distinct poetic gift. There were 33 of them: needless to say, not all budding Shelleys, but recognized by their instructors as having some measure of real gift for poetic expression. The scientists were merely students who were majoring in science, and who therefore might be supposed to have a natural bent in that direction. There were 34 of them.

Two kinds of material were presented for affective judgment: 50 nonsense-syllables, consisting of an initial vowel and a final consonant; and 50 color-combinations, two pieces of Bradley colored paper 3 cm. square pasted directly next to each other on a white card 8 by 3.5 cm. It was attempted to include in the series of syllables and of color-combinations both agreeable and disagreeable material. For the syllables, we were guided in choice by the results of the Study on The Affective Value of Articulate Sounds (this JOURNAL, 23, 579-584). The series of syllables was first presented to an *O*, who was instructed to attend to the sound of each and judge its pleasantness or unpleasantness, using the numbers 1 to 7.

The syllables were always given in the same order, and the *E* kept the pitch and loudness of her voice as constant as possible. When the syllables had all been presented, the color-combinations were shown and judged in the same manner.

Results. The total number of 1 judgments, 7 judgments, and 4 judgments was counted for each of the two groups. The results were as follows.

Sounds. Scientists. 1 judgments, 139; 7 judgments, 28; 4 judgments, 364; index of affective sensitiveness for the group (139+28, divided by 364), .45.

Poets. 1 judgments, 189; 7 judgments, 60; 4 judgments, 361; index of affective sensitiveness, .68.

Colors. Scientists. 1 judgments, 237; 7 judgments, 88; 4 judgments, 262; index of affective sensitiveness, 1.24.

Poets. 1 judgments, 278; 7 judgments, 133; 4 judgments, 182; index of affective sensitiveness, 2.25.

Both for articulate sounds and for color-combinations the group of poets shows distinctly higher affective sensitiveness than the group of scientific students. This result is what we should expect, and indicates that this measurement of affective sensitiveness might really be of value in investigating an individual's bent.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF YALE UNIVERSITY

Communicated by JOHN E. ANDERSON

II. THE EFFECTS OF PRACTICE WITHOUT KNOWLEDGE OF RESULTS

By LLEWELLYN T. SPENCER

The distinction has been drawn between habits which are built up with constant evidence of the success or failure of a given movement, and habits whose development takes place without such guiding data. In an article published several years ago, Judd exemplified the latter type by an experimental study.[1] "The striking fact which appears in the results.... is that practice brings little if any change."[2] He found that maladaptative movements failed to be eliminated and remarked that in the course of his practice period "nothing appeared to lead to any modification or specialization of the general ability."[3]

An examination of his tables, however, leads to some uncertainty as to the real significance of his data. In computing scores when a subject was instructed to place a dot behind a screen upon what he considered the extension of a given line, Judd measured the errors as *plus* and *minus* as they were above or below the true extension of the line, and found the average error for the day and line by summing algebraically. This manifestly has an obscuring effect upon the averages especially when their numerical size is to be compared. He recognized this, and admits that the averages constitute an indication of the predominant direction rather than of the amount of error. The mean variations which he gives enable one to gauge the effect of this method of computation somewhat, but the final result is not clear and cannot be expressed numerically.

Furthermore, if his results for the first day and for the tenth (*i.e.*, last) day be averaged without regard to sign, merely as given in Table I of his paper, the average error on the first day was, in cm., 1.44, and for the tenth day, 1.34; which shows a slight improvement (7%) which appears greater when it is considered that the average error for the whole ten days was 1.30 cm., from which we may infer that the results for the last day were by no means the best. This means that somewhere along the performance-curve an improvement was scored of at least 11%.

Because of these two difficulties in supporting Judd's conclusions by his tables, the present Study was undertaken in order to discover what would be the effect of ignorance of results during practice if the errors were treated purely as errors of amount without regard to direction.

The experiment as reported by Judd was repeated as nearly as possible. Sheets of paper were prepared with lines drawn from the center of the paper to the right-hand edge at 9 different angles to the horizontal. These angles were: +60°, +45°, +30°, +15°, 0°, −15°, −30°, −45°, and −60°. Guides were fixed in the table so that a sheet bearing one of these lines might be inserted accurately and later replaced by another. A high screen was then set up at the right-hand edge of the paper so that the subject, seated before the line, could not see beyond the screen. Beyond the screen sheets were used upon which the lines had been extended previously. Upon each of these sheets the subject placed a dot where he thought the extension of the line visible to him would fall. Ten trials were made with a given line and then the papers were replaced with sheets drawn to a different angle. Thus each day each subject was given 10 trials with each of 9 lines, making 90 trials for each subject.

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[1] *Psych. Rev. Monog. Suppl.*, 7, 1905, 185 ff.
[2] *Op. cit.*, 187.
[3] *Op. cit.*, 188.

Measurements were then made of the perpendicular distance from the dots made by the subject to the extended line. These errors were then averaged without regard to sign. Three subjects, A, B and C, made the 90 trials for a practice-period of 10 days. As it was evident that in allowing the subject not only to choose his vertical location but also his horizontal location there were two variables in operation which would make the results less regular, a fourth subject, D, was used for whom the conditions were modified to eliminate the horizontal variation. A ruler 1.25 in. wide was placed beyond the screen and fitted snugly up against it. The subject was instructed to place his pencil-point against the ruler and to slide it up until he considered that it stood upon the extension of the visible line.

Table I was constructed to correspond to Table I in Judd's article. In it are shown the average errors without regard to sign with the mean variation for each line for the first day, for the tenth day, and for the 10 days as a whole. The mean variations of the averages for the whole 10 days represent the mean variations of the daily averages from the total average and not the variations of the actual errors as do the mean variations in the first two columns. They therefore give a better picture of the course of the averages through the 10 days than the crude mean variations would give. The last column of the table gives the order in which the different angles were given. The averages of the averages, given at the bottom of the columns, summarize the general tendency of the results. The data are from the results of Subject A.

TABLE I.
Subject A

	1st day		10th day		10 days		Angle of line
	Av.	M.V.	Av.	M.V.	Av.	M.V.	
1	1.60	.40	1.64	.32	2.17	.54	−60°
2	2.26	.38	1.57	.35	1.50	.45	−15°
3	1.58	.32	1.41	.29	1.58	.49	−45°
4	1.91	.41	1.30	.46	1.13	.48	+15°
5	.78	.58	.41	.19	.74	.37	+60°
6	1.63	.54	.96	.34	.98	.46	0°
7	1.16	.45	1.02	.32	.87	.25	+30°
8	1.64	.51	1.41	.29	1.40	.44	−30°
9	.54	.30	.57	.40	.51	.17	+45°
Av	1.46	.39	1.14	.36	1.21	.26	

All quantities are in cm.
Errors are added without regard to direction, *i.e.*, arithmetically.
Each line was tried 10 times each day.

The concern of the study was not to question the greater improvement of the informed learner, which Judd pointed out, but to bring out the facts as to whether or not the subject ignorant of his success or failure improved at all. The table as given both by Judd and in Table I of the present article does not represent the improvement or non-improvement of the subject very clearly. This question may be more obviously answered when results are given by days over the whole practice period. Table II gives the average error for the 9 lines for each subject for each of the 10 days. The mean variations show no significant result of practice and are omitted from the table in the interest of clearness.

TABLE II.

Average error each day for all subjects.*
Italicized figures were obtained by adding algebraically, after Judd.
Otherwise quantities and computation as for Table I.

Day	1st	2nd	3rd	4th	5th	6th	7th	8th	9th	10th
A	1.46	2.07	1.38	1.02	1.28	.89	1.03	.97	.81	1.14
A	*1.27*	*1.49*	*1.14*	*.56*	*1.01*	*.34*	*1.01*	*.79*	*.91*	*1.12*
B	.68	.87	.62	.78	.48	.56	.69	.60	.90	.48
C	.68	.74	.49	.58	.56	1.26	1.04	1.35	1.54	1.38
D	1.56	1.57	1.54	1.52	1.36	1.48	1.46	1.16	1.15	1.24

In order that the degree to which the algebraic method of addition employed by Judd obscures the significance of the results may be clearly shown. the scores of Subject A have been treated as *plus* and *minus* quantities and their sums taken algebraically. These algebraic results are also given in Table II in *italics* under the averages obtained by the arithmetic method from the same results. It is readily seen that the algebraic method results in different values, which quite definitely alter the course of the general tendency especially during the last few days.

TABLE III.

Measures of Table II smoothed to give smoothed curves as in Plate I.
(For method of smoothing, see text).

Day	1st	2nd	3rd	4th	5th	6th	7th	8th	9th	10th
A	1.66	1.64	1.49	1.23	1.06	1.07	.96	.94	.97	1.03
A	*1.34*	*1.30*	*1.06*	*.90*	*.64*	*.79*	*.71*	*.90*	*.94*	*1.05*
B	.74	.72	.76	.63	.61	.58	.62	.73	.66	.62
C	.70	.64	.60	.54	.80	.95	1.22	1.31	1.42	1.43
D	1.56	1.55	1.54	1.47	1.45	1.43	1.37	1.26	1.18	1.21

Table III gives the values for the 4 subjects which result from smoothing the measures to give a more regular picture of the tendency present. The smoothing was obtained by using for each average the average of the crude average and the two immediately contiguous averages. Thus 149 in the Table under A, 3rd day, represents the average of the 2nd, 3rd and 4th day from Table II. The other values were obtained in the same way. The first and last values were obtained by averaging the contiguous value with twice the extreme value. The measures so obtained have been plotted in curves which are reproduced in the annexed plate. The presence of improvement in curves A, B, and D is very clearly evident. The graphic method makes very clear the difference in results due to the method of treatment. The curve which starts at 1.66 cm. represents the results of Subject A figured arithmetically. The curve starting at 1.34 cm. is that of the algebraically treated results for the same subject. While the presence of improvement is noted in the first half of the latter curve it is obscured in the last half. It is evident that the study of the improvement factor in the results can be adequately made only when the results are treated arithmetically.

The curve for Subject C is strikingly different from the other curves. The improvement evident in the others is shown through the fourth day, after which the curve reverses its direction and rises to a point denoting over twice as great an error as the initial score. If the other curves represent a true tendency in the practice-effects, there must be some exceptional reason for the type of curve produced by Subject C. Introspec-

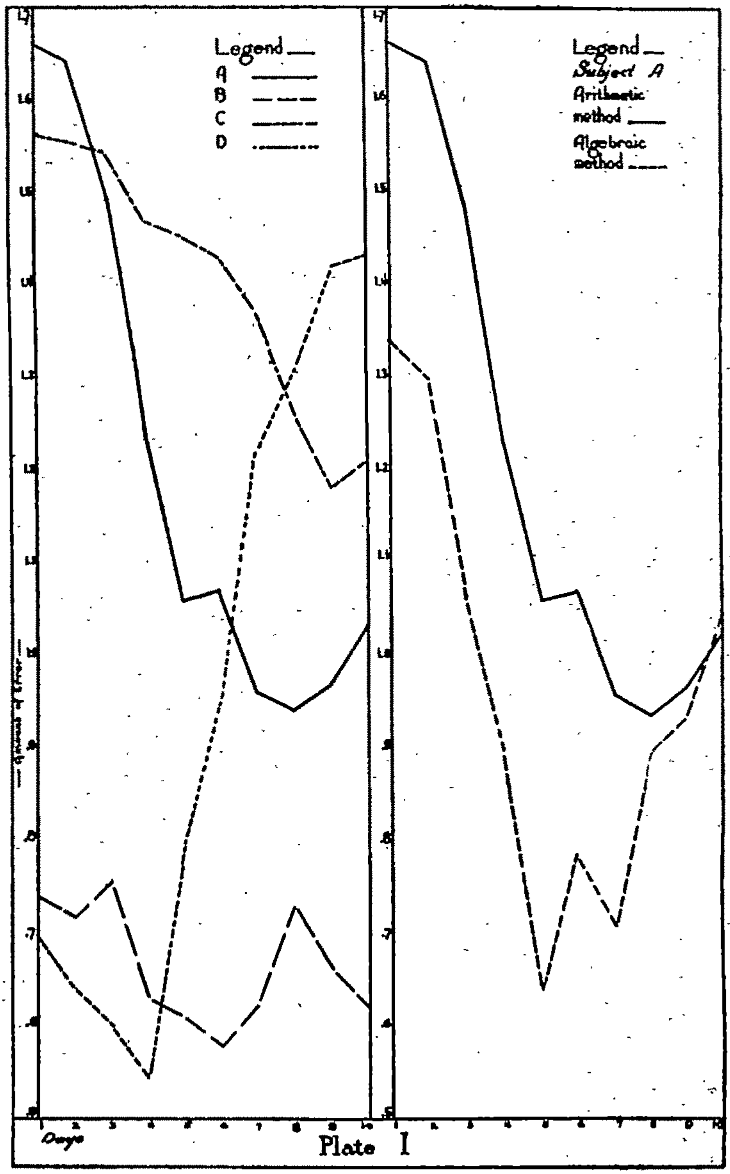
Legend
A
B
C
D
Legend
Subject A
Arithmetic method
Algebraic method
1.7
1.6
1.5
1.4
1.3
1.2
1.1
1.0
.9
.8
.7
.6
.5
Amount of Error
Days
1
2
3
4
5
6
7
8
9
10

Plate I

tion was requested of the subject without, of course, mentioning to him the presence of anything exceptional in his performance. The subject reported that he had merely tried to estimate the projection of the line as accurately as possible during the first few days, but that he had then felt for some unknown reason that he was underestimating the distance of the line. Thereafter each estimate was carefully compensated for this supposed underestimation, with the result that the *plus* errors increased both in number and amount so that the averages soon indicated a loss rather than a gain in accuracy. If this introspective evidence may be taken as indicating the true cause of the sudden reversal in direction, the conclusion reported by Judd that maladaptative movements in practice without knowledge of results are not corrected and eliminated gains additional support.

The interesting fact is that in the other curves in general and in C's curve for the first 4 or 5 days such maladaptative movements did not occur and that improvement actually was made notwithstanding the lack of knowledge as to the success of the movements. Just how the subjects were able to improve with no indication of success or failure is beyond the province of the present Study to determine. It may be that the improvement shown represents merely the improvement of adjustment to the general conditions of the procedure, etc., and that a continuance of the experiment would show a plateau of no improvement once the situation was thoroughly familiar. This however cannot be inferred from the present results.

Summary. An approximate repetition of Judd's experiment on this question under a more direct method of treatment of the results shows that there *is* improvement in the function studied by him in three of the cases examined.

One subject attempted to adjust himself to a supposed knowledge of his results after the first few days. His reactions show a sudden change of tendency, and he became less accurate than at the start.

Conclusion. The slight improvement indicated by Judd's Table I represents an actual improvement in the habit practised which Judd's method of averaging to some extent disguised.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CLARK UNIVERSITY

Communicated by EDWIN G. BORING

XXVII. A PRELIMINARY STUDY OF THE PSYCHOLOGY OF MATHEMATICAL ADJUSTMENT

By MILES A. TINKER

The Problem

In the scientific treatment of quantitative data it is often desirable to adjust a set of observed quantities to some best fitting mathematical function. The psychologist has frequent occasion to fit distribution curves and psychometric functions to his data and the mathematical procedures for certain classes of the cases that arise are already well worked out. When elaborate procedures exist, however, there is often a tendency toward over-refinement of mathematical treatment: an elaborate and exact method is applied with great pains to data of low observational precision so that an artificial accuracy is introduced into the statement of results. When the data are secured under inconstant conditions, or when they are few in number, or when their nature is such that there is little assurance that the mathematical assumptions involved hold with rigor in the particular case, then some rougher method of adjustment is in order. The simplest form of adjustment consists in drawing 'by eye' the line or smooth curve that appears best to fit the data. In the case of the line an equation can easily be stated. In the case of a curve, although no equation can ordinarily be written, interpolations can be made, exceeding the linear interpolations in accuracy.

Our original problem was suggested by the desirability of determining the degree of accuracy with which an *O* could 'by eye' draw the best fitting line for a number of given points. Such a statement of the problem is however not explicit.

One has in the first place to define the term 'best fitting.' Adjustment by the method of least squares is usually supposed to give the 'best fitting' line, but the method of least squares is entirely arbitrary, and depends for its validity upon convention and certain mathematical considerations which render the method easy of application. Moreover, as one discovers in studying a correlation table, the meaning of this form of adjustment is equivocal. What is, one may well ask, the 'best fitting' line for the scatter of points in a correlation table: the one regression, or the other, or the line of relation lying between the two regressions and from which the regression toward the mean occurs? An *O* can draw what he regards as the 'best fitting' line for a scatter of dots, but it is impossible to test his success unless there is agreement as to what constitutes the 'best fit' or there is information as to the meaning of this term in the *O*'s mind as he sought to make the adjustment. The general problem therefore becomes two-fold: we have in the first place to determine what in a given case a scientist means by the 'best fitting' hypothesis, since it is to give meaning to data that he makes an adjustment at all; and then we can proceed to determine how well this kind of an adjustment can be made 'by eye.'

Even with the problem thus defined we can probably hope for no general solution, for the accuracy of the *O*'s performance will presumably depend upon the nature of the curve to which adjustment is to be made, and, in the case of a given function like the straight line, upon the form of

Lº 7-1

the constellation of points to be adjusted. Accuracy will presumably also vary with his degree of practice both general and specific, and with other unanalysable individual differences.

Such is the general problem. In this paper we have sought to do no more than to formulate the problem and to work out a single simple case as a type. For this purpose we selected a simple constellation of seven points for adjustment to a straight line.

An Experimental Linear Adjustment

For the experimental linear adjustment we selected the constellation of seven points given below. The points were represented by dots drawn in ink on a square of blue-lined coordinate paper 8 in. wide. The coordinates, x and y, of the points in tenths of an inch are:

$x=$	10,	20,	30,	40,	50,	60,	70;
$y=$	22,	22,	32,	42,	32,	42,	52

The O was asked to make an experimental adjustment by placing a transparent celluloid stencil, on which had been drawn a fine black line, over the group of dots, and moving the stencil until the lines occupied the position which in his judgment "best represented the dots." After the position had been determined to the satisfaction of the O, the E recorded the line on the paper by marking with a pencil through three holes punched in the stencil along the line.

In certain preliminary experiments we used five *O*s to make numerous adjustments for this group of points. They were given the constellation as defined by the coordinates above and also with the paper turned through 90°. In the first position the dots all lay on the heavy-faced ordinates and between the heavy faced abscissae; on turning through 90° this relationship was reversed. After making the adjustment O was asked to make comments upon his method of adjustment and upon any 'difficulties or doubts' that had presented themselves in making the determination.

In general we found great diversity of procedure and of result among the *O*s, so that we were forced to conclude at once that no great reliability could be placed upon observational adjustments by *O*s who were unaccustomed to the process or whose skill was unknown.

Accordingly we decided to limit our work to three *O*s only, to give them more specific directions, and to practise them in the experimental placement of the line. We selected B (Dr. E. G. Boring), who was accustomed casually to making such adjustments and to thinking in terms of them, T (the author), whose experience in these matters was progressing rapidly as work upon the problem continued, and M (Mr. M. K. Macdonald), who is a laboratory man with some mathematical knowledge but little statistical experience and who was entirely without knowledge of the problem, as B and T were not. These *O*s were required to make a single adjustment a day for 10 consecutive days of this same constellation of points, presented always with the same base line toward the O. The directions were as follows:

"Suppose the dots on the sheet of paper represent numerically observations that should theoretically fall along a straight line. You are to adjust the straight line on the celluloid stencil so that it will in your judgment have the position of the straight line which best represents these points. Make three dots along this line (through holes in celluloid) and draw a line through these points.

"Give a short description of just what you have been trying to do and how you did it."

TABLE

Average measures of deviation between the presented points, the least square adjustment, and the observed adjustment. Observers *T*, *B*, and *M*. The M.V.s show the variability of the *O* for 10 adjustments on successive days. The deviation, d, is measured in mm.

Nature of deviation	*O*	Sum of squared deviations		Sum of absolute deviations		Algebraic sum of deviations	
		Σd^2	M.V.	Σd_{abs}	M.V.	Σd_{alg}	M.V.
Points from least square line		941.57		68.71		0	
Points from observed line	*T*	948.56	6.99	69.74	.38	3.26	1.32
	B	948.68	16.13	69.46	.48	—.62	2.40
	M	1006.06	80.98	69.50	4.07	—6.58	7.54
Observed line from least square line	*T*	6.12	3.17	5.11	1.79		
	B	12.85	8.84	7.15	2.86		
	M	52.86	54.39	13.72	5.21		

The results of these 10 adjustments are given in the Table. The deviations of the presented points from the observed line were measured in mm. by dividers and the sum of the squares of these deviations, the sum of the absolute values of the deviations (without regard to sign), and the algebraic sum of the deviations (regarding the sign) were computed for each day for each *O*. The Table gives the average for the 10 trials for each of these sums, and the M.V. for the 10 trials.

The linear adjustment by the method of least squares was computed. Its equation in tenths of inches is $y = .4643\,x + 16.2851$. The Table gives the sums of the deviations of the points from this line of least squares, and also of the observed line from the line of least squares. The values of the algebraic sum of the deviations between the two lines are omitted for the reason that they do not show well the degree of approximation of the two lines.

As we have noted above the 'best fitting' line is usually supposed to be the line of least squares. In accordance with this principle the fit of a line to a scatter of points is measured by the sum of the squares of the deviations of the points from the line. The first column of the Table shows these values. The least possible sum of the squared deviations is 941.57; *T* and *B* approximate this value to the closeness of 948.56 and 948.68, whereas *M* deviates by over 60 units. The same individual differences are shown by the sums of the squared deviations between the observed line and the line of least squares: *T* is most accurate and *M* least accurate in the approximations of this theoretical line. *M*'s inaccuracy is probably related to his variability of performance. The column of M.V.s shows that the more variable *O* is the less accurate and that *M* was the most variable; and *M*'s protocols show that he did not have a fixed mode of adjustment.

It might be supposed now that we had answered our original problem, the problem of the degree of accuracy with which adjustment can be made observationally. A good *O* (*B* or *T*) with a fixed method and knowledge of the problem can reduce the sum of the squared deviations to within 1% of their least value (to about 948 when the least value is about 941).

Immediately, however, the problem arises whether the line of least squares is the 'best line' and whether the sums of the squares of the deviations are the best measure of goodness of fit.

If one enquires now concerning the nature of the 'best' hypothesis for the linear representation of a given set of points, one might be justified in contending that the best line is the line which most nearly fulfills the conditions which the scientist has in mind when he seeks to let a non-linear set of observed points be represented by a line. To argue to representativeness on purely mathematical grounds is to make the meaning of the line clear but not necessarily to give it the meaning which the scientist has in mind. The answer thus lies in the mind of the scientist, and we sought from the protocols of our *O*s to discover what they, as scientists, meant by a line which 'best represents' a constellation of points.

The protocols showed a similarity between *B* and *T* in their methods of adjustment. *B* initiated his adjustment empathically, balancing the dots on two sides of the line and regarding them as deviating from the line in a direction perpendicular to the line. After the completion of the initial empathic adjustment, he checked or corrected his setting by noting whether the sum of the distances of the dots above the line appeared to equal the sum of the distances of the dots below, taking all these distances now vertically to the base line and therefore diagonally to the observed line. *T*'s method was practically identical with *B*'s; he sought to make the dots balance about the line at first and then to check the result by equating the sum of the distances above to the sum of the distances below. Both *B* and *T* sometimes accomplished this final equation by pairing off superior and inferior dots against each other. *T* held to his method more rigorously than did *B*, who sometimes sought to make the entire adjustment empathically. This difference may account for the fact that *T* is less variable than *B* by all criteria computed (see the M.V.s of the Table).

M did not in his 10 trials achieve any fixed method. He began by selecting the left hand dot as a pivot, passing the line through it and then swinging it until a balance of dots on either side had been obtained. He secured this balance by summing up the vertical distances above and below the line. Later he abandoned the pivot and sought to obtain the balance with the line free within the field, but in the very last trials he returned again to the use of the pivotal dot. This change in procedure seems to account for the fact that he is the most variable *O* (see Table).

It will be seen that all three *O*s begin by obtaining a balance or empathic adjustment and that this initial adjustment seems to them approximate only. They then proceed to a finer adjustment, and quite independently of one another seek to equate the sum of the vertical distances above the line to the sum of the vertical distances below. Their selection of the vertical distances instead of the horizontal or the perpendicular deviations may be due to the fact that the dots to be adjusted lay on the heavy-faced ordinates of the cross-section paper.

No *O*, it is to be noted, in making the adjustments sought to think in terms of squared deviations, although all three were accustomed to use the method of least squares and two were aware that the usual form of adjustment would require them so to think.

If now, following the hint contained in the protocols, one seeks to substitute for the line of least squared distances some other line which is determined without regard to the squares of the deviations, one then discovers quickly enough the reason for the popularity of the hypothesis of least squares. What might this other line be? It might be the line for which the sum of the unsquared deviations is least. But this line is indeterminate, for the least sum is zero when the positive and negative deviations are equal and any line of constant slope moved through the constellation passes through a position where this sum is zero. It is for this reason that the text-books tell us that the method of least squares is of advantage because in squaring the deviations it makes them all of like sign. But can we not simply arbitrarily dispense with the signs and sum up the deviations in their absolute values? Such a line is also not necessarily determinate; when the number of points is even and half lie on either side of the line, then all positions fulfilling this condition give for a constant slope the same sum of the absolute deviations. It is possible, however, to combine the two criteria. By applying simultaneously the condition that the algebraic sum of the deviations shall be equal to zero and the sum of the absolute deviations a minimum, and then effecting the solution by assuming enough values of the slope to indicate the slope that will give the required minimum (interpolating parabolically to find the minimum), we obtained, as a line satisfying these two conditions: $y=.4631\ x+16.3331$. This line differs so little from the line obtained by the method of least squares that we deemed separate mathematical treatment unnecessary, but it is interesting to note that it can be obtained in cases where it is desired.

The foregoing discussion makes it evident that the *O*s did not tell the whole tale in their protocols. To equate the sum of the positive to the sum of the negative deviations is not to determine the line: only if the slope be given or a single pivot be fixed upon will this process determine the line. It would seem that *B* and *T* tended rather to fix the slope on general empathic grounds and to shift the line up or down in equating positive and negative deviations, whereas *M*, at least when he was using the 'pivot,' probably selected the pivot empathically and then adjusted the summed deviations by rotation.

Although the protocols are not fully descriptive of the process, there is no doubt that the *O*s failed to attempt to apply a method of least squares in their observational adjustments, although they used in part a method of the equation of the sums of the positive and negative deviations. It was of interest therefore to discover whether some measure of deviation more in accord with the *O*'s avowed intent in adjustment would give results different from those from the sums of the squared deviations. We have therefore presented in the Table as measures of deviation the sums of the absolute deviations and the algebraic sums of the deviations for the three *O*s for the case of the deviations of the points from the observed line.

The Table shows that, although individual differences in variability are shown to be the same by any criterion, the degrees of approximation of the dots to the observed line vary considerably with the criterion. If we take the sum of the absolute deviations as a measure, then we find that all *O*s do about equally well, and that if small differences are taken into account the rank order of accuracy is *B, M, T;* whereas when squared deviations are considered *M* deviates widely from the other two *O*s and the rank order of accuracy is *T, B, M*. If we take the *O*s' own criterion as given in their protocols, *viz.*, that the algebraic sum of the deviations should be zero, we find that the line of least squares fulfills the conditions perfectly (necessarily), and that there are individual differences this time in the rank order *B, T, M*.

In the face of such difference it is impossible to contend that the measure of deviation makes no difference in the conclusions. The problem seems to call for a further analysis, logical and perhaps introspective, of the meaning and scientific use of adjustment, with an adaptation both of the form of adjustment and of the measure of its degree to the scientific problem involved, rather than a submission to principles of mathematical simplicity which have little correspondence with the psychology of scientific thinking.

Conclusions

Under favorable conditions of practice and constancy of attitude, observers can make a linear adjustment of a small number of points with an inaccuracy that is only about 1% greater than the least inaccuracy that can occur as measured by the method of least squares.

Observers do not, however, work by least squares, nor think in terms of squared distances. The psychology of adjustment is still obscure in that it is in part empathic, but it includes as an important factor the equation of the sums of positive and negative unsquared deviations. It is therefore not apparent that the method of least squares has as much scientific meaning as might some other hypothesis.

The applications of different criteria of the goodness of fit of an adjustment give different results. It appears therefore that goodness of fit depends upon the scientific meaning to be attached to the deviations of the points from their adjustment, and that no 'best' line can be defined until a criterion of deviation from it is thus established.

XXVIII. Attention as a Condition of Affection

By Max Meenes

In the psychological laboratory it is ordinarily assumed that attention is a condition of affection: if the affective value of a stimulus is to be determined the stimulus must be attended to.[1] On the other hand the psychoanalysts assert that the affective tone of consciousness may be determined by ideas obscurely or unconsciously present. There would, of course, be no excuse for confusing *a priori* the "unconscious" of the psychoanalyst with the "unclear" of the experimentalist; but there exists nevertheless an obvious problem for the experimentalist in the determination of the affective value of the stimulus which is unattended to. Yokoyama[2] recently touched upon this problem in his study of the simultaneous effect of color and form upon attention, but he was unable to solve it because his series did not include a complete simultaneous variation of the two variables. The present study, suggested by Yokoyama's problem, seeks to compare, by the method of paired comparisons, the affective values of colors attended to with the affective values of the same colors when attended from.

Stimuli. We used six Milton-Bradley papers: "red" (R), "orange" (O), "yellow" (Y), "green" (G), "blue" (B), and "red violet" (P). Every stimulus consisted of a pair of these colors, arranged in immediate left-right juxtaposition, with each member of the pair a square 5 cm. on the side. In making a comparison one such paired stimulus was presented above to be compared with a second paired stimulus presented below. The distance between these upper and lower paired stimuli was 8 cm. *O* was 1.5 m. from the stimuli, which were observed under the artificial daylight of the C2-Mazda lamp. The stimuli were presented through windows in a gray cardboard screen and exposed for 2 sec.[3]

The series included every possible pair of the six colors including every color paired with itself. There are 15 heterogeneous pairs and 6 homogeneous pairs (color with itself), making 21 paired combinations in all. To have reversed every pair in order to give both space orders would have ultimately required 10,332 judgments from each *O* to complete the experiment. It would not have been possible to obtain so many judgments in the time available; hence we reduced the labor by presenting the pairs without reversal, — a reduction from 10,332 to 1260 judgments, for the work increases approximately with the square of the number of stimuli. We took account of space error, however, by making up the pairs in such a manner that every color came as often at the left as at the right. The list of stimuli prepared in this manner is as follows:

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[1] The relationship is more often implicit in the experiments by the method of impression than explicit in systematic exposition; nevertheless *cf.* E. B. Titchener, *Beginner's Psychology*, 1915, 79f.

[2] Yokoyama, M., Affective tendency as conditioned by color and form, this Journal, 1921, 32, 81-107, esp. 97-100.

[3] The apparatus and conditions were similar to Yokoyama's: *op. cit.*, 83ff.

1. RR
2. OO
3. YY
4. GG
5. BB
6. PP
7. OR
8. YO
9. GY
10. BG
11. PB
12. RY
13. OG
14. YB
15. GP
16. GR
17. BO
18. PY
19. RB
20. OP
21. PR

Procedure. Every one of these 21 stimuli was presented with every other one by the usual method of paired comparisons in both vertical space orders, *i.e.*, although the right-left space order within the paired stimuli was taken account of only indirectly in the manner described, the up-down space order was taken strict account of by actual reversal under observation. This comparison required 420 judgments.

Such a series of 420 judgments was taken (1) with attention upon the right members of each paired stimulus, (2) with attention upon the left members of each paired stimulus, and (3) with attention equally upon both members of each stimulus, thus making the entire experiment consist of 1260 judgments for each *O*.

The instructions were as follows: "After the preparatory signal you will be shown two pairs of colors, one pair above the other. Sometimes you will be instructed to pay attention only to the right color of each pair, sometimes only to the left color of each pair, and sometimes to both colors in each pair. Give yourself up to the colors that you are told to attend to and report as quickly as possible which of the stimuli attended to is the more pleasant, whether the 'top' or 'bottom'. Do not form any associations with these colors if you can help it; simply live them and state immediately the more pleasant. Whenever you fail to live up to these instructions, report 'failure'. If the stimuli attended to are equally pleasant report 'same'."

The series were balanced against temporal effects by requiring successively 210 judgments each for (1) attention to the right, (2) attention to both, (3) attention to left, (4) attention to left, (5) attention to both, and (6) attention to right.

"Failures," which were not often reported after practice, were repeated when reported.

Observers. The *Os* were Dr. Marjory Bates (*Ba*), then a graduate student in psychology; Dr. M. Yokoyama (*Y*), assistant in psychology; and Mr. F. L. Bixby (*Bi*), an advanced undergraduate.

TABLE I

Observer *Ba*

A. Preferences for paired colors where one color of the pair is attended to and one attended from. Method of paired comparisons; maximal number of preferences 40. Colors attended to in caps (B, R, G, *etc.*); colors attended from in lower case (y, r, g, *etc.*). Each cell gives the preferential score for a given color attended to (column) when combined with a given color attended from (row). Columns and rows are arranged so that average scores are in rank order.

COLORS ATTENDED TO

Colors attended from

	B	R	G	Y	O	P	Av.	m.v.
y	39	36.5	24.5	10.5	13.5	20	24	9.3
r	35.5	20	19.5	24	17	9.5	20.92	5.9
g	36.5	30.5	17	16.5	18	4	20.42	8.7
b	32	32	18.5	17	17	5.5	20.33	5.5
o	37.5	33.5	23	10.5	10.5	6	20.17	11.1
p	30.5	16.5	19.5	24	8.5	8	17.83	6.8
Av.	35.17	28.17	20.33	17.08	14.08	8.83		
m.v.	2.6	6.7	2.3	4.6	3.2	4.0		

m.v. of the Av.s = 1.23
Av. of the m.v.s = 8.3

m.v. of the Av.s = 7.37
Av. of the m.v.s = 3.9

B. Preferences for paired colors where both colors of the pair are equally attended to. Method of paired comparisons; maximal number of preferences 40 (doubled in order to be comparable with part A). Each cell gives the preferential score for the pair indicated by the column and row in which it lies. Columns and rows are arranged so that average scores are in rank order.

COLORS

Colors

	Y	B	R	O	G	P	Av.	m.v.
Y	18	34	37	17	25	30	26.83	7
B	34	34	23	27	27	12	26.16	5.9
R	37	23	23	27	13	1	20.67	9.1
O	17	27	27	11	21	8	18.5	6.5
G	25	27	13	21	15	9	18.33	6
P	30	12	1	8	9	8	11.33	6.4

m. v. of the Av.s = 4.09
Av. of the m. v.s = 6.8

Table II

Observer *Y*

See legends of Table I

A. One color of pair attended to and the other attended from.

COLORS ATTENDED TO

Colors attended from	G	O	B	Y	R	P	Av.	m.v.
y	30	24.5	29.5	26	13	12.5	22.58	6.6
g	36	25.5	23	19.5	12	9	20.83	7.3
o	33	30	28	15.5	10.5	5	20.33	10.
b	19	28	28	26	4.5	8	18.92	10.7
p	17.5	19.5	17.5	22	9	9	15.75	6.3
r	25	22	17	18	12	6	16.75	5.2
Av.	26.75	24.92	23.92	21.17	10.18	8.25		
m.v.	5	3.4	4.7	3.5	2.3	2		

m. v. of the Av.s = 2.07
Av. of the m.v.s = 7.7

m. v. of the Av.s = 6.65
Av.s of the m.v.s = 3.5

B. Both colors of pair attended to.

COLORS

Colors	G	Y	B	O	R	P	Av.	m.v.
G	37.5	35.5	29	32.5	20	14	28.08	7.4
Y	35.5	24.5	35	23	12	14.5	24.08	7.6
B	29	35	27	30.5	13.5	6.5	23.58	9.2
O	32.5	23	30.5	28.5	3.5	8.5	21.08	10
R	20	12	13.5	3.5	16.5	1.5	11.17	5.9
P	14	14.5	6.5	8.5	1.5	6.5	8.58	6.5

m.v. of the Av.s = 6.38
Av. of the m.v.s = 7.8

Table III

Observer *Bi*

See legends of Table I

A. One color of pair attended to and the other attended from.

COLORS ATTENDED TO

Colors attended from	R	B	G	P	O	Y	Av.	m.v.
y	34.5	34	26.5	18	13	1.5	21.25	10.4
r	32	31.5	21.5	17.5	16	3.5	20.33	8
g	32.5	32.5	21	17.5	12.5	4	20	8.7
p	31.5	32	22	17.5	13.5	3.5	20	8.5
b	36.5	32	19	15	14	2	19.75	10.4
o	31.5	33	22.5	19	10	2	19.67	9.3
Av.	33.08	32.5	22.08	17.41	13.16	2.75		
m.v.	1.6	1	1.6	0.8	1.3	0.9		

m.v. of the Av.s = 0.43
Av. of the m.v.s = 9.7

m.v. of the Av.s = 8.89
Av. of the m.v.s = 1.1

B. Both colors of pair attended to.

	COLORS							
	R	B	G	P	O	Y	Av.	m.v.
Colors R	28	37.5	34.5	25	25.5	24	29.08	4.6
B	37.5	26	32.5	25.5	22	22.5	27.67	5
G	34.5	32.5	13.5	18	22	14	22.41	5
P	25	25.5	18	15	13.5	9	17.67	5.2
O	25.5	22	22	13.5	8.5	3.5	15.83	7.3
Y	24	22.5	14	9	3.5	0	12.17	8.0

m.v. of the Av.s = 5.58
Av. of the m.v.s = 5.2

TABLE IV

M.v.s of the averages of preferential scores in their absolute amounts (see Tables I-III) and as percentages of the dispersion of a maximally determined series. The m.v. of a series under the method of paired comparisons where preferential determination was invariable would be 10.5.

		ONLY ONE COLOR OF PAIR ATTENDED TO		BOTH COLORS OF PAIR ATTENDED TO
	Obs.	For color attended to	For color attended from	
M.v.	*Ba*	7.37	1.23	4.09
	Y	6.65	2.07	6.38
	Bi	8.89	0.43	5.58
M.v. as % of 10.5	*Ba*	.70	.12	.39
	Y	.63	.20	.61
	Bi	.85	.04	.53

Results. The results are tabulated in Tables I-III.

Part A of each of these tables gives the number of preferences for each pair of colors. The column indicates the color of the pair attended to and the row the color attended from. Thus the rows give the preferential functions for the colors attended to when the color attended from is constant; and the columns give the preferential functions for the colors attended from when the color attended to is constant. The highest possible score would be 40, since each paired stimulus was presented 40 times with attention on one member of the pair and 40 times with attention on the other member. Neither space-error (right-left, up-down) appears in the table, since all the results have been combined.

Part B of each table gives similar results for the series when both colors of the pair were equally attended to. Actually the table should be only a half table, but it has been filled in completely and the scores have been doubled in order to make the scores comparable in magnitude and the table comparable in form to Part A.

It is apparent from the tables that the colors attended to are the predominating factors in the determination of the preferences. The evidence for this conclusion is as follows:

(1) The preferential orders of the colors are much more widely dispersed for the colors attended to than for the colors attended from. The m.v.s of Tables I-III are brought together in Table IV to show this relationship. If we were dealing with a univocal determiner in either set of colors the score for the most pleasant pair would be 40: the score for the

next most pleasant pair would be 38, since this pair would be judged more pleasant than any other pair except twice when compared with the most pleasant pair; and so on. The m.v. of such an absolutely determined-series would be 10.5, and the greater the m.v. of one of these series the greater would be the dependence of the preferences which it expresses upon the differences which the series represents. Table IV shows that the dispersion for the color attended to is from 3 to 20 times greater than for the color attended from. The color attended to shows 63 to 85% of the maximal dispersion possible; the color attended from only 4 to 20%.

(2) The same relationship is shown in a different manner by the averages of the m.v.s in part A of Tables I-III. The av. m.v. at the right of the table is the average dispersion when the color attended to is the variable, and it is consistently larger than the av. m.v. at the bottom of the table which is the average dispersion when the color attended from is the only variable. In the one case the dispersions are from 2 to 9 times the dispersions in the other.

(3) It would seem that a further test of the relative importance of the color attended to and the color attended from might be made by comparing the preferential function for each with the preferential function for the colors when both are attended to (Part B of the tables). The method is not satisfactory on statistical grounds, however, for the reason that the series involved is in some cases of low precision, as indicated by the m.v.s of Table IV. One gets high coefficients of correlation (.70 to .98) between the functions for the colors attended to and the functions where both colors were attended to, but one may also by chance get high correlation between the functions for the colors attended from and the functions for both colors since the former are all of low precision.

The further question arises whether the colors attended from, although they have no great effect upon the preferential judgments of the paired stimuli, have any demonstrably consistent effect at all. It is not to be expected that the preferential functions of these colors (columns of Part A, Tables I-III) should be indeterminate. Even if the colors had no effect in themselves, chance differences would presumably appear in the columns, making rank orders statable. The problem is simply whether the rank orders of the colors attended from are significant.

One turns for the solution to a comparison of the preferential functions for the colors attended from and the corresponding functions for the colors attended to. If, given everything else constant, the colors attended from do cause the paired stimuli to fall in a preferential function of the same order as the colors attended to, then it would seem that color is still an effective, if slight, determinant under inattention.

A positive conclusion on this point can not, however, be reached from our results. For *Y* the correlation between the series of averages for the colors attended to and the colors attended from (Part A of the table) is .81; agreement is high. For *Ba* there is almost no agreement; the correlation is .18. *Bi*, the least skilled *O*, brings Y from one end of the series to the other, thus giving a negative correlation, —.53. In the face of such unaccountable variability it is tempting to ascribe the differences to "chance" and to say that the colors attended from have no demonstrable effect at all. The conservative conclusion is, however, that their effect, if it be that they have a consistent effect, is not the same from *O* to *O* nor in general of the same nature as the effect of the colors attended to.

Conclusion. In paired comparisons of colors affective judgments are conditioned predominantly upon the colors attended to. Colors attended from, but presented with the colors attended to, have at most but slight effect upon the judgments.

BOOK REVIEWS

Hugo Münsterberg, His Life and Work. By MARGARET MÜNSTERBERG. D. Appleton and Co., New York, 1922, pp. x, 449.

The shape in which Miss Münsterberg has set forth the life of her father is quite different from the usual form of biographies. From the nature of the case it could hardly have been a judicial survey of Münsterberg's life interwoven with a critical estimate of his works. In lieu of the latter the author has worked out an appendix of 147 pages, made up of abstracts of her father's writings. A classification of these under the headings psychological, philosophical, educational, biological, ethical, social, political, medical, legal, aesthetic, economic, and belletristic, gives some notion of the extent of Münsterberg's activities as well as of his unwearied industry and facility of expression.

The book is also not a biography in the form of "Life and Letters;" for, regrettably, but few of Münsterberg's letters are included in the volume which runs true to its title "Life and Works" and chiefly and especially works that appeal to the contemporary public. As much space is given to the Congress of *Gelehrte* at the St. Louis fair, in great part organized by Münsterberg, as to the first twenty-nine years of his life; and the account of the investigation of the criminal Orchard is noticed as much at length as the transactions of the Freiburg laboratory,—psychologically the most fruitful period of her father's life.

Perhaps Miss Münsterberg felt that this division of labor was justified in the case of one so much in the public eye as was her father, and further that the scanty treatment of his professional work in the first part of her book is amply compensated by the abstracts of the appendix. But such a plan mars the integrity of the work.

Assuredly there is enough material in an account of Münsterberg's exoteric activities, from the time he set foot in America, to fill a fair-sized book. With his name is associated the establishment of Emerson Hall and the Germanic Museum at Cambridge, the planning and execution of the congress of "World's Scholars" at St. Louis, and the founding of the American Institute in Berlin. In addition he treated psychopathic sufferers, investigated trance mediums, advised business houses, and lectured extensively outside of the University. He was a steady attendant upon congresses of Psychology and Philosophy and yet found time to dispense a liberal hospitality which in its far flung scope included such diverse personalities as Charles Eliot Norton, Prince Henry of Prussia, and the actress Nazimova. And all this on top of his routine of academic work, and along with a steady stream of books, essays and magazine articles! The man's industry was tremendous and his facility of expression prodigious, especially when one considers that the greater part of his publications was in a language which he practically learned after his advent in the United States.

Münsterberg's power of expressing himself copiously, fluently, and directly, whether in English or German, was of course a gift which required practice for its development; no more in writing than in piano-playing does one acquire facility without practice. For Münsterberg the practice began in his boyhood with the writing of verses and plays. Later he published a volume of poems. And the pen of a ready writer was no negligible quantity in shaping his career. It certainly was one of the factors in influencing James to recommend him to Harvard. James' aversion to the cumbrous and involved style of German systematic philosophy amounted to repulsion, and his delight in finding a German who could write psychology clearly and directly was pronounced. "You handle things with such a broad, light touch in writing about them," he tells Münsterberg in one of the letters which are the high lights in the 'Life and Works," "that it is a constant pleasure to read what you say."

What James had read was the *Willenshandlung* and two or three numbers of the *Beiträge*. Curiously enough, but little notice is given to the *Beiträge* by Miss Münsterberg, though they belong to the period of her father's life when his professional work was at its best. They were also decisive factors in bringing him to Harvard. "I must say," James wrote him, "that you seem to me to be doing more to open out vistas in Psychology than anyone today."

Of course James was no experimentalist; in fact he detested experimental work; and it was at this period of his life he had declared that the psychological outcome of Fechner's work was "just nothing." In England Robertson and Alexander also gave the *Beiträge* hearty welcome. On the other hand, the work was sharply criticised by the experimentalists Martius, Meumann and Titchener. G. E. Mueller ran through the *Beiträge* with a fine-toothed comb dipped in vitriol. In a fine letter of consolation to Münsterberg, James called this critique "brutal." Stumpf in Berlin was perplexed to find his own work confirmed by Münsterberg by methods which he (Stumpf) thought invalid. The task which Münsterberg had set himself in publishing the *Beiträge* was beyond the power of anyone to carry out; it was to perform all the experimentation himself. The outcome was a series of studies top-heavy with hypothesis. Rich in suggestion and fertile in experimental expedients, Münsterberg somehow gives the impression that experiments must need bear out his hypothesis. However, the work made enough of an impression on James to bring about a call to Harvard where the early volumes of the Harvard Psychological Studies give more evidence of Münsterberg's fertility in ideas.

Münsterberg's *magnum opus*, the *Grundzüge der Psychologie*, was dedicated to James, though the latter gave warning that the work might find in him "one of its worst enemies." This, of course, with James would imply no change in personal regard. Whether, however, the entente of the earlier days was kept up, we do not learn from the "Life and Works." James' letters, as time went on, seem to have become fewer and shorter. He could hardly have regarded with approval Münsterberg's absorption in applied psychology. In any case, contrasting if not opposing temperamental tendencies had become pronounced, Münsterberg enjoying the throngs of the market-place, while James detested crowds—psychological congresses included. Somewhere he writes of his dislike of "big standup receptions where people scream and breathe in each other's faces."

Of the scholarly *Grundzüge* only the general part, the Principles of Psychology, was published. Some fourteen years later Münsterberg brought out his "Psychology General and Applied," shaped on the principles laid down in the *Grundzüge*. The work was written as a text-book, and was hardly a worthy successor of the *Grundzüge*. As a text-book, it introduced novel sections on Social and Applied Psychology, but it was too shot through with epistemological puzzles to take the place of the common form of manual.

In these later days Münsterberg had pretty well given himself over to applied psychology, and from his pen flowed a steady stream of 'applications'. "Psychotechnik," "Psychotherapy," "Psychology and the Teacher," "Business Psychology," and so on. While all these publications resulted in making Münsterberg the best known psychologist in America, they were as a whole in no wise helpful to his reputation as a scientific psychologist or to the science of psychology. Despite much sound advice and scholarly admonition, they formed the gate for the flood of pseudo-psychologising and commercialized psychologues now inundating this country.

In the later years of his career, Münsterberg was deeply interested in bringing about an entente between Germany and the United States. To this he sometimes added England, though in this respect his activities do not seem to have gone much beyond a pious wish. His anxiety to maintain the entente between his adopted and native countries during the Great War, and the coldness with which some of his colleagues met him in those days, contributed in all likelihood to his tragic death. That his preference was for this country his refusal of the call to Königsberg in 1905 attests: the prospect of a call to Oxford (which, *pace* Miss Münsterberg, is far from being a "call") was probably no great temptation. But that twenty-two years of residence over here had wrought no fundamental change in the Germanism of his nature is curiously shown in a eulogy to the Emperor William II., who had been selected along with Wagner, Schopenhauer and Nietzsche as representative writers for a volume of Kuno Francke's "German Classics."

Miss Münsterberg has had a very difficult task to perform, and she seems to have chosen to guard against filial fondness by making her account as *objectiv* as possible; but the absence of letters deprives the biography of that which counts most in biography—the disclosing of personality. From the "coign of vantage" of the reviewer, I would counsel her, in case of future editions, to scan her proofs more narrowly, to verify all quotations, especially from Shakespeare, and above all, to follow d'Alembert's advice to Voltaire as regards economy in the use of adjectives.

Stanford University FRANK ANGELL

Brain Abscess: Its Surgical Pathology and Operative Technic. By WELLS P. EAGLETON, M.D., Newark, N. J. (President American Otological Society, 1921.) The Macmillan Co., 1922.

As stated in the preface, this monograph is the result of extensive experience in cranial surgery. It is original in conception and aims to induce the surgeon to think in terms of pathology and to plan his operation so that it may be adapted to the pathological condition found in any particular case, and at the same time to utilize surgically the active protective forces of the brain.

Discarding the older ideas of the aural and nasal origins of the infection, the author bases his classification upon the mode of entry into the brain of the different pathological processes causing brain abscess. By utilizing this classification the surgeon is assisted in determining not only the existence of an abscess, but also the presence or absence of a capsule, which is of great importance.

Part I. deals with the general aspects of intracranial surgery, Part II. with the special consideration of the surgical pathology and operative technique of brain abscess, while Part III. is devoted to its surgical diagnosis. The text is divided into a description of the microscopical lesions of brain abscess, followed by the physiological and mechanical conditions which must be dealt with surgically, and concludes with a description of the technique best adapted to meet them. By this arrangement the reader is enabled to peruse a general treatise on the pathology and surgery of brain abscess without the tiresome details of operative technique, which follow in their proper place, in small type, at the end of each section.

Although the book is primarily a record of personal experience, over seven hundred references are given from recent literature. It also includes a compilation and analysis of the 125 post-mortem records of cerebellar abscess which had been reported up to 1919; and 141 recorded cases of frontal lobe abscess, in addition to 31 personal case reports, with the diagnostic and surgical lessons learned from each. It is an encyclopedia of information on the surgical pathology and operative treatment of brain abscess.

The illustrations, with two exceptions, are from original drawings and photographs.

Chapters I. and II. contain brief directions regarding the method of approaching the case, the proper time for operation, and describe the general surgical technique for intracranial operations. Their perusal will give valuable assistance to any surgeon who may be called upon to make an intradural exploration.

Chapters III. and IV. deal with the pathology of brain abscess, call attention to the importance, from a surgical standpoint, of the presence of a capsule, giving points as to how it may be diagnosed and utilized. Stress is also laid on the value of the protective processes of the brain and the importance of conserving them.

In chapter V. the author gives his views on metastatic abscess, which it will profit the reader to consult in the original.

Chapter VI. deals with cerebellar abscess. It aims to help the surgeon to localize the abscess from a study of the mode of entry.

In chapter VII. abscess of the frontal lobe is considered and, as stated in an introductory note, it is largely the result of a study of the cases reported in the literature. It emphasizes the importance of the protective character of the nasal and accessory sinus membranes, and the danger associated with their injury during operation.

Chapter VIII. is concerned with hernia cerebri, and Chapter IX., on the protective mechanism of the brain, explains the action of the delicate processes within the dura which must be preserved and encouraged if a case of brain abscess is to recover following an operation. It is based on the experimental work of Flexner and others, and is the first attempt to correlate this work and make it applicable to brain surgery.

In Part III., chapters X., XI. and XII., the diagnosis of brain abscess is very exhaustively treated. The symptomatology peculiar to brain abscess is divided into (1) evidence of cerebral suppuration, (2) symptoms of cerebral compression, (3) localizing symptoms. The symptoms are explained on the basis of the physiology of the nervous system by giving, as far as possible, the anatomical and physiological reasons for the different symptoms as they develop. The diagnostic value of "little things" is emphasized, *e.g.*, the "initial vague chill" in the diagnosis of a capsule; the "disproportion" of "pathological sequences" in diagnosis, and the possible cause of subnormal temperature, are considered. The literature is filled with records of cases where a diagnosis was not made "because of the absence of the, so-called, symptoms of brain abscess." Review of these cases will show that what in reality prevented the surgeon from making a diagnosis was not the absence of the symptoms of cerebral suppuration—for they were present—but the absence of those symptoms which point to an increase of the intracranial contents, the signs of cerebral compression; and few cases of brain abscess do present evidences of cerebral compression in the early stages, while in a number of cases of frontal lobe abscess compression never occurs at all. Headache, according to the author, is always present, and no other single symptom is of such uniform occurrence or of greater assistance to a positive diagnosis. Early diagnosis is of the utmost importance if operative interference is to be successful.

The presence or absence of papilloedema and the value of "transient symptoms" such as aphasia and hemianopsia for colors are discussed. The frequency and importance of this type of hemianopsia as an early diagnostic sign of adjacent temporo-sphenoidal lobe abscess have not been previously recorded.

In chapter XIII. the diagnosis of frontal lobe abscess is dealt with, and in chapter XIV. certain complications that may arise are considered.

While this book covers a restricted field, the subject is treated exhaustively, and it is not too much to say that it should be in the hands of every cranial surgeon. It will also be of the greatest service to the otologist and rhinologist, and, because of its extensive discussion of the presence or absence of papilloedema, it will appeal to the ophthalmologist. Even to the general surgeon, who any day may be called upon to perform an intradural operation, the book will be a valuable aquisition, and because of the wealth of diagnostic details it may be recommended to the neurologist and the general practitioner. In fact, as the fruits of an extensive experience, and careful and discriminating observation, it may be heartily recommended to all medical men.

The paper and printing are excellent and the general arrangement of the material leaves nothing to be desired. Author and publisher alike are to be congratulated on the production of this important contribution to brain surgery.

Cornell University SUTHERLAND SIMPSON

Instinct and the Unconscious. By W. H. R. RIVERS, Cambridge University Press, 1920. pp. viii, 252.

This volume by the late Dr. Rivers is, its subtitle tells us, "A Contribution to a Biological Theory of the Psychoneuroses." The book has two parts. "The first gives the substance of lectures delivered in the Psychological Laboratory at Cambridge in the summer of 1919, and repeated in the spring of the present year (1920) at the Phipps Clinic of the Johns Hopkins Medical School, Baltimore, under the direction of Professor Adolf Meyer. The second part consists of appendices in which are republished occasional papers written as the result of clinical experiences gained during the war." "The general aim of the book," as the Preface goes on to state, "is to put into a biological setting the system of psychotherapy which came to be generally adopted in Great Britain in the treatment of the psycho-neuroses of war."

One may note at the outset that this system is essentially a modified Freudianism in which the instinct of sex was found to play a negligible part: the general conception of instinctive nature upon the basis of which these pathological psychoses are explained and treated being that current in British writings since McDougall's comprehensive treatment of the subject in his "Social Psychology." These views are supplemented and interpreted in their biological aspects by reference to the work of Henry Head and his collaborators in neurology. Thus the mechanisms of diseased conditions of mind are, in general, reduced to suppressed instincts. "The aim of this book," we are told, "is to consider these mechanisms in their relation to the more normal processes of the animal organism, and especially to the mechanism by which certain parts of experience become so separated from the rest that they are no longer capable of recall to consciousness by the ordinary processes of memory. Psycho-neurosis depends essentially upon the abnormal activity of processes which do not ordinarily enter into consciousness, and the special aim of this book is to consider the general biological function of the process by which experience passes into the region of the unconscious. I shall attempt to show that the main function of psychoneurosis is the solution of a conflict between opposed and incompatible principles of mental activity. Instinctive processes and tendencies, and experiences associated therewith, pass into the unconscious whenever the incompatibility passes certain limits" (pp. 5, 6).

The author supports his theory of the nature of these conflicts and their mechanisms by reference to Head's distinction of *protopathic* and *epicritic* sensibility, and the general function of the neo-pallium in providing for mass-action of the all-or-none type accompanied by a strong affective tone. The indiscriminative character of protopathic sensibility, together with the emotionally toned responses controlled by the *optic thalamus*, furnish a biological setting for the most original of the instinctive tendencies. *Sup-*

pression is defined biologically as the natural and 'unwitting' process whereby outgrown and no longer serviceable instinctive tendencies become unconscious. In contrast, the term *repression* is used to note a 'witting' endeavor to accomplish a similar end. Suppression, however, is biological and accounts for complete submergence, especially of those impulses of the all-or-none type which are accompanied by mass-action and strong affective tone. But not all of protopathic sensibility is thus suppressed, part of it being retained and fused into the epicritic sensibility which development has superimposed.

Analogous to phylogenetic development in which higher levels are constantly being achieved through the combined processes of suppression and fusion, we have ontogenetic processes of a similar character accounting for the suppression and fusion of infantile experiences and impulses. Hence in childhood experiences tend to become unwittingly suppressed whenever they involve certain incompatible instinctive impulses; quite as the no longer serviceable biological tendencies of the all-or-none type of mass-reaction are suppressed in the development of critically controlled responses. The precise nature of these suppressions can be studied by reference to the manifestations of instinct, which the author proceeds to classify under the three general heads of (1) self preservation, (2) regeneration, and (3) social cohesion. The first of these exists in two forms: one the appetitive and the other the danger-instincts; the second has likewise two forms: the sexual appetitive and the parental tendencies of solicitude. The third, however, has four aspects, *suggestion*, *sympathy*, *imitation* and *intuition*.

Of the first group the danger-instincts play a large part in the war-neuroses, and may be described under five heads: (1) the reaction of flight with the emotion of fear; flight tending towards an 'ungraduated' total response; (2) the reaction of aggression with the emotion of anger; (3) the reaction of manipulative activity characterized by an absence of affect, as indicated by the marked coolness and lack of fear attending a situation of danger when it is met by any consistent habituated response; (4) the reaction of immobility with complete suppression of action, fear, and pain; completeness being absolutely essential to serviceableness; and (5) the reaction of collapse with the emotion of terror; a combination of both paralysis and tremor arising from an obstruction to any normal instinctive mode of response, and attended by an excess of affect.

The suppressions involved in these various types of response to danger while originally complete, are later modified by partial integration arising under special conditions and needs of the organism; as when an animal first flees until it has removed itself from the source of danger, and only then resorts to the reaction of immobility. "In such a case it is necessary to inhibit a mode of reaction which only a moment earlier has been in full activity. The mechanism of suppression is thus one which must have come into being at a very early stage of animal existence" (p. 68).

The question of the independent existence of suppressed experience in the unconscious is held in suspended judgment. The independent activities of dissociation, as indicated for instance in the *fugue*, are attributed to alternate states of consciousness, without admitting a necessary existential continuity such as Morton Prince has assumed for 'co-consciousness'.

The gregarious instinct, with its affect of *sympathy*, its unwitting *mimesis*, or imitation of the movements of others, and its *intuitions* of another's thoughts, is explained by *suggestion*, to which the author attaches a very special importance. Suggestion is a mechanism of instinct which demonstrates that instinct is not altogether of the all-or-none type; for it is a form of reaction occurring under the conditions of collective or social life whereby graduation and discrimination of responses can be obtained *unwittingly*. Suggestion is thus made to account for the marvels of instinctive resourcefulness found in insects, and though it too, like the mass-response, tends to be replaced in man by intelligence, it is still functional under special conditions, such as those of hypnosis, sleep, and psycho-neurosis.

With this system of interpretation the author proceeds to deal with various types of psycho-neurosis, which he defines as "attempts, successful and unsuccessful, to restore a balance between instinctive and controlling forces" (p. 119); failure being marked both by an increase in suppressed tendencies and by a weakened control. Special attention is given, first, to anxiety or repression-neuroses, in which the witting attempts at repression are found to be the chief hindrance to recovery because they are unavailing where the impulses are deeply instinctive and subject only to an unwitting suppression. Secondly, hysteria, or substitution-neuroses are considered, and a theory is advanced to explain the characteristic paralyses, contractures and anaesthesias of war victims in terms of the crude protection afforded by partial manifestations of instinctive immobility.

In general, the author finds anxiety-neuroses characteristic of officers, persons of higher intelligence who make a witting though ill-advised attempt to repress their emotions in the presence of danger, whereas hysteria is more frequently found among private soldiers whose training has enhanced their suggestibility. Escape through *dissociation* is the third chief mode of dealing with conflict, and it is found to characterize *phobias*, *paranoia* with its elaborately rationalized delusions, and *dementia praecox* with its proneness to escape through day-dreams eventuating in hallucinations. The disintegration psychoses such as *mania* with its exaltation and excitement are likewise traced to "different degrees of expression of modes of thought and behavior which, owing to their incompatibility with social traditions and ethical standards, are in health subdued and suppressed" (p. 146).

The last two chapters are given over to a discussion of *regression*, showing how disease of the mind tends to retrace the steps the organism has passed through in the course of its development; and to *sublimation* as a means whereby the energy of conflict can be deflected into channels beneficial to the individual and productive of works of genius.

The appendices, six in number, and occupying nearly two-fifths of the entire volume, serve primarily as a documentation of the ideas and theories set forth in the main body of the book. The whole work might have benefitted if the material had been brought together into a single compact whole; for the initial essay furnishes an anti-climax to the appendices which, obviously, were written from a less comprehensive and a less systematic point of view.

As it stands the book is more 'suggestive' than definitive in its statements, and the mixture of clinical materials with systematic conceptions of interpretation is not always as convincing as one could wish. In particular, the biological theory upon which the author bases his interpretation is open to the serious objection that it is throughout telelogical. The ingenious employment of Head's theories of sensibility and response is therefore perverted by the constant employment of the criterion of *serviceableness* as an explanation of the fundamental operations of suppression. As an illustration of this non-scientific criterion, the author thinks he must meet or set aside a real difficulty in his attribution of the hysterias of warfare to a re-excitation of the all-or-none type of instinctive immobility, because immobility on the battle field would not appear to be a particularly efficacious means of protection.

"To this objection," he tells us, "there are two answers." One is that in the vast majority of cases these protective reactions do not occur on the field of battle, but it may be a few days, it may be months, after the shock, wound, or other event of which they are the sequel. When they occur in this manner they fulfil in great perfection the protective purpose which is the special function of the danger-instincts" (p. 128). Why the 'great perfection' of this fulfillment, after the event has passed and gone, is not altogether obvious to the reviewer. The second answer to the difficulty, applying to those cases where the disability follows immediately, is given in terms of the suppression of fear and pain attending immobility, which makes the individual incapable of realizing the danger to which his throwback exposes him. But the chief point is that the author should find this to be "one of the problems which will have to be settled by any scheme of explanation which can be regarded as satisfactory" (p. 128).

Had the author seen fit to follow Head a little more closely, and interpret these regressions as *compulsory* integrations determined by conditions of situation and affect, instead of insisting that serviceableness must always control and be ever apparent if the individual is at all conscious, we should have had a more scientific statement of the facts of the case, and at the same time one in which the lines of future experimental investigation would have been more clearly laid down.

Cornell University R. M. OGDEN

Manuel de psychologie. By E. B. TITCHENER. Authorized translation by H. Lesage. Paris, 1922. Pp. 571. Price 35 fr. net.

It is curious that, despite the many contributions to experimental psychology by Frenchmen like Binet, Henri, Bourdon, and Piéron, there should not be in their language an introductory text in experimental psychology. The nearest approach to a book of this kind is Wundt's *Eléments de psychologie physiologique*, a translation made in 1885 of the second edition of the *Grundzüge*, which, even if it could ever have served as a textbook, has for many years been hopelessly out of date. It is to remedy this omission that Professor Lesage has now translated Titchener's *Text-Book of Psychology*, which, he says "by virtue of its clearness, its conciseness, its fullness, and its scrupulous distinction of fact and hypothesis, seems to me to be a model of its kind."

Aside from this compliment to the book, and in addition to its value for French students of psychology, the French edition is a contribution to the terminology of psychology at large. If we may trust the opinion of the translator, the psychological vocabulary is not only better established but it is also richer in English than in French. A satisfactory translation, therefore, of a book from the one language to the other must tend to bring the two vocabularies to the same level, and psychology in general gains therefrom. In the present instance there is even a greater gain because Klemm's earlier translation of the *Text-Book* into German as *Lehrbuch der Psychologie* makes possible for the first time in the history of experimental psychology a cross-comparison in three languages of the terms employed in one and the same book.

Professor Lesage's translation is entirely satisfactory. He brought to his task both a full realization of its difficulties on the side of terminology, and a feeling for style and *l'expression heureuse*. He has sought the assistance of Bourdon and Titchener in certain technical and idiomatic passages; wherever he is in doubt or dissatisfied with a term he has placed the English equivalent within parentheses; and where he has been forced to make a neologism he has based it on the English word. He has added no new references to the experimental literature, but Titchener himself has in a new preface indicated some of the more important lines of development during the last decade. The Subject-Index has, unfortunately, not been translated. The only error we have thus far discovered is the omission of a sentence on p. 393, which destroys the sense of the passage.

H. P. W.

General Psychology in Terms of Behavior. By STEVENSON SMITH and EDWIN R. GUTHRIE. New York: Appleton, 1921. Pp. xii, 270.

The teacher of psychology to-day is not (as a rule) content to teach psychology as psychology. Owing to his own enthusiasm and to popular demand, he sometimes endeavors to present his subject so that it will be all-explanatory and all-inclusive; it appears necessary to him that it should be easily applicable to the social sciences. When a psychologist, therefore, states in positive terms *why* individuals and groups conduct themselves in certain ways, he has prepared fertile ground for the educator, the economist and the sociologist. He has vindicated himself and his discipline; he has secured for himself academic and popular approval.

A psychologist, then, bent upon such a purpose must have a text-book which will assist him in carrying out this point of view. Such a text, it seems to the reviewer, is *General Psychology in Terms of Behavior.* The purpose of this work, as stated in the preface, "is to describe man's original nature, the way in which this nature is altered by use, and the common modes of individual and social behavior that result." The facts are selected and presented so that the student may have a "systematic explanation of his own conduct and that of his fellows."

Psychology is the science of behavior, we are told, that "takes the common sense view that any animal is a physical object in a world of physical objects. It assumes that all these objects act upon each other in the ways described by physics, chemistry and physiology. It considers man's behavior as a physical event that can be analyzed into bodily movements." If this study of the bodily movements is to constitute a science, we might ask why it is necessary to consider that all responses have a certain utility, which may or may not be apparent? "Although we may usually expect to find some use served by every response, there are many cases of inappropriate reactions. For example, the instinct that leads a dog to run barking beside the front wheels of an automobile seems to serve no useful purpose either to dog or driver. If we remember that the dog is the descendant of the wolf and that the wolf in killing his prey must depend upon the pack for assistance, it is clear that there was a time when this reaction was of service." This attempt to explain all behavior cannot be undertaken, in the opinion of the writer, at the present time. Surely, if "psychology is the science of behavior" the study of behavior must follow the methods and the exactitude employed in the other physical sciences to which it claims relationship.

Again, it is stated that "a complete understanding of behavior always involves an analysis in terms of stimulus-response mechanisms." Later, however, in treating two opposed reaction tendencies, the author tells us that "introspection usually detects either a bodily change or a change of external stimulation as an antecedent of voluntary movement." Why is it necessary, in this case, to resort to introspection?

The usual topics considered by the behaviorist are treated in five chapters entitled: The Elements of Behavior, Instinct, Learning, Coenotropes (or Common Habits) and Perception. In the sixth and seventh chapters, the topics of Human Motives and Social Psychology are taken up, and an attempt is made to describe phenomena of this variety in terms of the stimulus-response mechanisms.

In the Appendix of this text, we are informed that "a description of mind in terms of stimulation, neural action, and responses is by no means the only one in use. There is another language, which the individual may employ in describing the world as his own experience, and which includes terms that are useless in a description of behavior. This is the language of consciousness, and its importance is established by its popularity." The student has already been introduced to an objective psychology which has been quite adequate, in the opinion of the authors, to explain all behavior.

To such a student, then, psychology must be a study of behavior, and it would seem that the introduction of short paragraphs which deal with the topics of Consciousness, Sensation, Emotion and Affection, Images, Association of Ideas, Imagination, Attention, Perception and the Unconscious (they are given seventeen pages) must result in nothing but actual confusion for the student. It might also be noticed that popularity alone does not account for the importance and value of the "language of consciousness." Scientific theories have persisted not on account of their popularity, but in spite of prevailing modes of thought.

In an elementary text, of course, many theories may be presented without stating the evidence upon which these theories are based. To this one would not object; but when theories are presented for which there is no scientific evidence, objection is perfectly valid. The reviewer objects therefore to many of the theories in explanation of individual and social conduct presented in this work on the grounds that they are mere matters of opinion. Theories of this sort cannot constitute a *science* of behavior which will give the student, or anyone else, "a systematic explanation of his own conduct and of his fellows."

Cornell University SETH WAKEMAN

The Psychology of Learning. By W. H. PYLE. Baltimore, Warwick & York, Inc. 1921. pp. 308.

In the preface to this little book the author says that "he has tried to state every thing that is known about learning." How nearly this programme, rather ambitious in itself, finds fulfillment will depend to a considerable degree on the definition given to learning. Learning turns out to be a purely practical affair; it is what we learn in the way of habits formed and knowledge acquired, *i.e.*, organised experience. The essential thing in the one is *movement*, in the other, *idea*. We are told also that "learning consists, psychologically, *in building up and establishing preferential routes in the nervous system.*" This hypothetical neural basis is stated or implied throughout. The initial connections result from trial and error and from simultaneous experience, respectively, in the two forms of learning. If we go to the chapter on ideational learning and inquire more closely for a theory of learning, we read: "By ideational learning we mean acquiring knowledge." In knowledge "the important thing is *sequence of ideas.*" We seem to be running in a circle. The definitions, however, may help. "The meaning given to each of these terms is a simple common-sense one." ......"Ideas are the conscious processes representative of objects of revival or secondary experience." Knowledge is "the name for connected ideas:" at least one term of the connected pair must be an idea. Ideas get connected if the "processes take place together in the brain or come in close succession." Hence the "*inner world of ideas*" is orderly because "the outer world is lawful, orderly, systematic." But there is more in the process: for "things which we have experienced together may have for us no useful connection." "We therefore *reorganise* the world by thinking over our experience in the relations that are important to us." The reader halts at the 'we' and the 'relations'; for these are surely new factors. We learn nothing about the 'we'; but we are told that the causal relation is the most important one, since "causal sequences exist in nature." We had supposed that 'cause and effect' was a man-made notion. Three pages farther on we find that "the most important thing about an idea is its meaning;" that "the meaning of an idea is another idea closely associated with it: one that represents some characteristic, some relation, some use." We may pass by the implied infinite regression and ask anew, What is knowledge? We find now that "most of our knowledge is knowledge of meanings; that our progress in knowledge consists in learning new meanings." We get nothing more definite. All this may be a theory of learning, but it can scarcely be said to exhaust all that is extant on the subject.

We might in a similar way go into an analysis of other chapters, and in every case we should find that the book before us is not exhaustive. In fact the card-sorting experiments made by the author form the backbone and skeleton of the work. This statement is not intended as a negative criticism except in the light of the opening sentence of the preface. In every one of the fourteen chapters some topic receives consideration in a rather simple, elementary way that reflects, what the author so often professes, a common-sense point of view.

A fairly complete list of references follows every chapter, and there is a general bibliography at the end of the book.

The Child and His School. By Gertrude Hartman. New York, E. P. Dutton & Co. 1922. pp. xiii, 248.

This book grew out of the effort on the part of the author to compile a bibliography which should serve the needs of those teachers who are interested, experimentally, in curriculum-making, and out of the conclusion that such a list of titles stripped bare of any unifying principle is next to worthless. In its outward aspect it is very largely a compendium of quotations, for the most part from the writings of John Dewey, of whom the author is an enthusiastic follower.

The system makes a progressively developing social order the object or aim of education, and the assumptions of a dynamic behaviorism the means for its realisation. But there is more here than Woodworth, to whom the author refers her readers, would admit; consciousness appears in the part ("a superior device for securing adaptation") to which the functionalists assign it, and agreeableness and disagreeableness appear as the agents which select from among the several adaptations. Hence we find that "social considerations determine the end toward which adaptations are to be directed and supply us with the subject content of education; psychological processes dictate the method by which our aim may be accomplished." This emphasis upon the consciously adapted activity as means robs the subject matter, as such, of any claim to an independent existence or to a consideration in its own right.

The entire school-programme is worked over, with general indications of the way in which the several subject-matters which grew out of social demands may serve to solve the problems of education instead of themselves being the problems. Finally, the changes demanded by such a system in the work of the teacher, in the measures of success, and in school-equipment receive discussion. On the whole the book forms a fairly complete working-out of Dewey's philosophy of education, and will for that reason be of interest to educators. In so far as such efforts tend to free us from our bondage to subject-matter and to emphasise attitude and process, we may wish them all success; as a practical solution of a practical problem, trial alone can decide the merits of this particular essay. The book carries, beside sectional references, a bibliography of over a thousand entries, many of which are duplicates due to their topical arrangement.

Nutrition and Growth in Children. By William R. P. Emerson, M.D. New York & London, D. Appleton and Co. 1922. pp. xxix, 342.

The book deals, primarily, with the problems of the undernourished child and with the nutritional class as an agency for their solution. Except for the technical terms, the layman can understand the book and may get helpful hints from it; especially from the charts and photographs. But the book is in no way intended for the layman; many other sources are available which give him what he needs to know in more convenient form. The book is for the clinician, since malnutrition is a disease that demands the services of the expert as much as does any other ailment.

The author's main thesis is that malnutrition results, invariably, from the various combinations of part or all of a limited number of factors all of which lie within our power to control. These factors are home control, food habits, health habits, physical defects, especially obstructions to breathing, and overfatigue. Contrary to some writers, the author does not place diet at the top of the list; "it is not the first or chief cause of malnutrition, but actually fourth in the list and shares even this place with faulty food habits." The cooperation of the home, the school, the medical profession and the child's own interest is necessary for their control. The clinician diagnoses, arouses interest and prescribes; the burden of reform, aside from physical defects, falls on the home with the cooperation of the school.

The fundamental measure of nutrition is the relation of weight to actual height quite independently of any norms of height, though these are not wholly disregarded. Such norms as we have for height and especially for weight are so low as to be unreliable, since they include the undernourished who make up a considerable percentage of the population. Accordingly the author presents a new set of norms. The mental life of the child may be symptomatic; hence we infer that mental retardation or even reversion parallels the physical state. A somewhat modified form of the Stanford Revision of the Binet-Simon test series, given without control, enables the mother in the home to measure the intellectual status of the child. We infer also that in the interpretation of the IQ we must take into account the state of nutrition of the subject. This very brief chapter reveals the usual weakness of the man of medicine when he writes on subjects that touch psychology.

The book is primarily a book of professional propaganda, and as such should render service in a field where much remains to be done.

Psychanalysis in the Class Room. By GEORGE H. GREEN. New York and London, G. P. Putnam's Sons. 1922. pp. 272.

With a greater or less show of reason the teacher evidences a distrust of psychology, which distrust will break down under the forces of the practical results to follow from the application of the principles of psychanalysis. "The teacher who is able to make himself acquainted with them will discover that they help him to explain much in the mental life of children which previously puzzled him, and which appeared incapable of explanation.... It enables the teacher to understand himself and to realise that a great many difficulties that occur in his work are the result of tendencies in himself."

It is important, if psychanalysis is to furnish this insight into the mind of the child and thus to render service in the resolution of the difficulties that beset education, that the teacher should know where to look for the disturbing factors, what to look for, and what to do when she finds them. Those traits, activities and what-not which interfere with the progress of the child, not alone in his school-work but also in his more complete development, reveal themselves in day-dreams, play, dreams, word-associations, interests, slips, accidents, omissions, and in the phenomena of introversion, extraversion and identification. The factors which actually make for interference are unfulfilled wishes, impulses, interests, and modes of response actuated by the unconscious activity of instincts that find less than their normal expression in the environment in which the individual leads his daily life; in a word, instinctive wishes. The most common bases of these wishes are the instincts of domination, homage, subjection, self-assertion, possession, sex, and the gregarious instinct; self-assertion, domination and possession would seem to be the most important. The remedy lies in the exposure of the submerged wish, especially when it grows out of some early experience of an unpleasant sort, and sometimes in the fulfillment of the wish; but the more usual and wholesome remedy lies in sublimation. "The eradication of the instinctive wishes would mean the eradication of the whole of the pupil's motives; not motives for daydreaming only, but for doing anything whatsoever....In the psychanalytic view the chief aim of education is to discover the nature of the deep instinctive motives of children and to train them from egoistic levels of expression to altruistic levels."

The book reveals many features of the Freudian system, with its emphasis on the unconscious aspect of mind with interests that differ from those of the conscious mind; but the author lays greater stress on the other instincts, for which he accepts the terminology of MacDougall, and less on sex. To those who accept the doctrine of complexes in all its latitude, the book will come as a welcome extension of it, specifically, to the problems of education; to those who do not believe that every dream, every act of play, every word-association, every slip, every interest in Cinderella, Charlie Chaplin and fairy stories, every accident, every act of kindness or tap on the table means an attempt to escape from the world that is into an ideal world in which all the instinctive wishes find fulfilment, the book will appeal less strongly. There is little that is new in the way of fundamental concepts.

The Education of Behavior. By I. B. SAXBY. London, University of London Press, Ltd. 1921. pp. vii, 248.

The author accepts the definition of education offered by Herbert Spencer, which in general terms is "preparation for adult life; and this in turn may be described as preparation for efficient citizenship." This attitude, together with his acceptance of the instincts and emotions of MacDougall, the sentiments of Shand, and the complexes of Freud, gives the key to the book; "it is an attempt to bring our present knowledge of psychology to bear on the problems of behavior which have to be faced by those who are in charge of boys and girls during their adolescence." Of the two factors, heredity and environment, which determine behavior, heredity supplies the 'vital energy,' but environment supplies the moulds that shape the patterns; "the natural endowments of the child present as it were the sum total of his possibilities, but it depends on environment how they develop."

The urges or sources of energy for action spring from the inherited tendencies toward self-preservation, race-preservation or a combination of these. More specifically, the inborn tendencies to action may be classed under reflexes, impulses (the usual instincts), sentiments and complexes. The sentiments which arise as the impulses organise about some object of thought are "really *centers of potential activity* of which we become aware through our likes and dislikes." These, with the complexes which are unconscious sentiments, form the "mental systems which make for consistency in *acquired* forms of behavior." They are on that account basic for habits, emotions, and sympathy as well as for all that goes to make up character. Thus far we have a group of systems played upon by environment in the form of percepts, or as perceived, and the whole seems consistent although purely conceptual. But when we come to consider character, other forces demand admittance. We now have in addition intelligence, dispositions, a general attitude towards life, and will power. Intelligence we are told, we know about already; disposition becomes a function of impulses; attitude under its two aspects, temperament and perseveration, becomes a matter of physiology; and "will power, *i.e.*, the energy that makes it possible for us to overcome resistance within ourselves, is in some way derived from our self-regarding sentiment." We must not, for all this, view these systems as blind mechanisms to be played upon indifferently by environment. Not only do the impulses have ends, which need imply nothing more than teleology, but the 'self' steps in to change things in all sorts of ways; it prevents overflows of energy by diverting it to other forms of activity, it desires to avoid or wants to overcome, and the needs of the 'self' determine the strength and permanence of sentiments. This view is yet in harmony with the major thesis; for "in any particular case the final idea of the 'self' is the combined product of the innate possibilities of the individual and of the pressure which has been exerted by his environment."

When one approaches a problem of utility, of ends to be attained, every means to that end become legitimate. The author in this book assumes the concepts of MacDougall, of Shand and of Freud, and by modifying here and there, as in the case of emotion and the complexes, works the lot into a unified whole. It is a pretty display of logic as applied to words but, even if it should 'work,' that would prove nothing of the truth or falsity of the concepts. The place of such conceptual constructions is in the world of values.

How to Measure in Education. By WILLIAM A. MCCALL, New York, The Macmillan Co. 1922. pp. xii, 416.

There is no need to attempt even a summary of the contents of these more than four hundred pages of topical material. A great deal is here, but there is much that we miss. As we read we wonder why the book need be more than one-half its present bulk; that is, as we read with the impress of the title and of Chap. I on us. We find the answer in the preface; the author means to tell not only the How but also the Why. The wisdom of the mixture is not apparent. He who needs the Why is scarcely ready for the How, and he who wants the How gets lost in the Why, so that if he is at all serious he will turn to other sources where the material is ready to hand.

The book strikes the reader as extremely elementary; it is as if the teachers had become the victims of their own 'soft pedagogy'. It is difficult enough, perhaps, for the very beginner; but with a little pruning here and there some of the more involved and even of the more fundamental concepts could have found a place. But what strikes one most is the divergence between what is, at least tacitly, promised in the theses of Chap. I and what one actually finds in the rest of the book. The ideals of Chap. I cannot now and in all probability never will be realised. For example, it may be true that we have no element showing a qualitative dimension that does not at the same time show a quantitative one; but this does not mean that the qualitative may not vary independently of the quantitative aspect. It is less harm, however, to promise more for the future than we can give at the present time than it is to give less than we can. If we are going to measure, there is just one set of tools at our disposal,—the mathematical. When the author comes to the matter of weighting, he tells us that weight is a function of variability; yet he assigns weights to his own scores on the basis of some vague notion of their importance and "because it was a saving of labor." Such procedure can never lead to exact results, nor can the plea of simplicity justify it.

On the other hand, the book gives information about how to use a great many of the measures which educators have used; and where they have used two types of measure for the same end, it gives the reasons in non-mathematical terms for their choice, together with the advantages and disadvantages of each type. Another good feature is the numerical arrangement of points on any topic according to their order of development.

Cornell University L. B. HOISINGTON

PSYCHOLOGICAL PERIODICALS

Zeits. f. Psychologie, Bd. lxxxviii., Heft 3 bis 5. G. RIES. 'Untersuchungen über die Sicherheit der Aussage.' [Reports experiments on subjective assurance; the materials were for the most part visual objects, meaningless and meaningful, of graded complexity, tachistoscopically exposed; single series were taken with words brought in from a distance (Wiegand) and with odors. The experiments fell into three groups: in the first, the observers stated what they had cognised at a single exposure; in the second, they judged whether the given stimulus was identical with (or similar to) a previous stimulus; in the third, they were asked to identify odors. The results of the first group showed that subjective assurance is conditioned on exclusiveness of reproduction (less upon the other Müllerian factors of promptness and insistence), on fulness of ideation, on meaning and associative connection; more generally, upon attention and readiness of residua. The second group furnished nothing new. The third group brought out the fact that the coexistence of sensation with idea of name is not always adequate to assurance; an express comparison might be made of sensation with memory image.] Anhang: F. SCHUMANN. 'Das Erkennungsurteil.' [The cognition of a visually presented word depends upon the cooperation of residua, the feeling of familiarity (not invariable), the arousal of the auditory-motor word-image, and the conviction that this image corresponds with the visual presentation. Experiments with odors prove that this conviction, in turn, depends upon the coincidence of the residua aroused by stimulus and name. It may consist, experientially, in increased insistence of quality, feeling of familiarity, satisfied expectation, the formation of a more unitary complex; or it may contain an as yet unidentified component.] T. SCHJELDERUP-EBBE. 'Beiträge zur Sozialpsychologie des Haushuhns.' [Every bird has its established place in a social hierarchy, determined ordinarily by combat, and carrying with it the privilege of harrying (pecking) subordinates.] Anhang: D. KATZ. 'Tierpsychologie und Soziologie des Menschen.' [The preceding article gives various suggestions toward an experimental sociology. There is, *e.g.*, in man also, a congenital tendency to form ordered groups: cf. schoolboys, sportsmen, intellectual cliques.] V. HAECKER und T. ZIEHEN. 'Ueber die Erblichkeit der musikalischen Begabung: nebst allgemeinen methodologischen Bemerkungen über die psychische Vererbung,' i. [First part of an investigation based on questionary returns (595 replies, relating to at least 5000 persons). Discusses terminology; begins the discussion of results. Thus, discordant marriages (plus-minus, minus-plus) show a far higher percentage of plus and double-plus than of minus and double-minus descendants. Gives the evidence for and against the occurrence of the various biological types of inheritance.] Literaturbericht. E. R. JAENSCH. 'Zur Richtigstellung und Ergänzung.' [Explains the intercurrence of phenomena of macropsia in the work of P. Busse, vol. 84.] Gesellschaft für experimentelle Psychologie.

Bd. lxxxviii., Heft 6. J. PLASSMANN. 'Psychologische Erfahrungen mit einem Zeitsignal.' [A study of the reception of wireless time-signals from the Eiffel Tower. As against the average of all signals, the three longs are taken too early, the long-short, and especially the long-long-short, are taken too late. In the former case, only the personal equation is involved; in the latter, the observer is prejudiced; he looks for a final long. Experiments in the laboratory are needed.—In the records, the figures 0, 1, 2 are overdetermined; the figures 3, 4, 5, 6, 7, 8, 9 are underdetermined.] K. SAPPER. 'Physikalische Betrachtungen zum Problem der Wirkung psychischer Faktoren auf materielle Systeme.' [Physics can raise no objection to an operation of a psychical or vitalistic factor upon material particles such that the direction (but not the velocity) of their movement is changed. Since, however, this operation has strict limitations, and since every mechanical force may be analysed into an accelerating and a directive component, it is better to regard the psychical factor as operating directly on the directive component of the force and thus only indirectly on the material movement.] J. HIRSCH. 'Ueber traditionellen Speisenabschau: ein Beitrag zur genetischen Gefühlspsychologie.' [Traditional aversion from certain forms of food cannot be explained teleologically; the line of development runs rather from prohibition through avoidance to aversion. The prohibition may arise through conflict of cults (pork among the Jews, horse-meat among the modern Germans); usually, however, it goes back to primitive magic (totem-animals, soul-animals, protective animals). A similar magical origin accounts for our aversion from the flesh of animals that have died a natural death, from blood, from food that contains hairs, from the remains of food left by another on his plate, etc., etc. The reasons for such aversion offered by civilised man are merely rationalisations; they may serve to maintain or intensify the tradition, but they could never have originated it. The passage from avoidance to aversion is accounted for, psychologically, by the close relation of reverence to aversion.] Literaturbericht.

Bd. lxxxix., Heft 1 bis 3. W. PETERS. 'Das Intelligenzproblem und die Intelligenzforschung: eine Auseinandersetzung mit William Stern.' [Critical review of Stern's *Die Intelligenz der Kinder und Jugendlichen und Methoden ihrer Untersuchung*, 1920. (1) In place of a theory of intelligence Stern offers a definition, which theoretically is altogether uncertain and practically is useless to the psychologist. (2) In his account of the Binet method Stern gives no general survey of corrections, changes, extensions, and makes no attempt to supplement the determination of status by a determination of cause. (3) Experiment and observation can be applied alike both to reactive and to spontaneous behavior, since there is no essential difference between these forms of intellectual expression. (4) Stern neglects statistical methods. In particular, there is no evidence whatever for the constancy of the IQ in the case of intellectually backward children; theory and fact both speak for a gradual decrease. (5) Both the teleological nature of normality and the doctrine of qualitative difference in abnormality are assumptions only.] H. HENNING. 'Assoziationsgesetz und Geruchsgedächtnis.' [Discusses, chiefly with reference to smell, the differences in association and memory between the qualities of the higher and those of the lower senses. Thus (1) palpable olfactory images and abstract olfactory surrogate ideas are not associatively equivalent; the latter stand generally on a par with meaningless syllables, words, figures; the former are far superior. (2) An odor presented as object-odor in the context of a total situation has an immense associative power; an odor presented as datum-odor, say with a meaningless syllable, has less associative power than another syllable. (3) The primary meaning of an odor depends upon its object-character within an unitary complex (total situation); its concrete actuality; and congruence of the factors in the situation (habituation).] A. RIEKEL. 'Psychologische Versuche an Hühnern: Ueber den Vergleichsvorgang und damit zusammenhängende Fragen, besonders auch über das Farbensehen der Hühner.' [Experiments, with simultaneous and successive presentation of stimuli, prove that the process of comparison depends upon 'transitional' experiences; the result is confirmed by experiments on young children, before the age of naming, and itself confirms Jaensch's work on eidetics, while it makes against Köhler's theory of structural function.—The birds are sensitive to blue, but far less sensitive than is man.] E. R. JAENSCH. 'Ueber den Aufbau der Wahrnehmungswelt und ihre Struktur im Jugendalter.' v. E. R. Jaensch (mit einigen Versuchen von H. Freiling). 'Ueber Raumverlagerung und die Beziehung von Raumwahrnehmung und Handeln.' [Transpositions of various kinds, arising spontaneously in eidetic images of bidimensional objects, are conditioned upon successive antagonistic attentional impulses; the phenomena are therefore akin to that of the horopter deviation (transposition in the third dimension). Incidentally, Mach's problem of symmetry is thus brought nearer solution.—These transpositions are valuable in the forestages of thinking: *e.g.*, in recognition, in the thought-experiment (in Mach's sense).—Since eidetics are able to produce transpositions, not only in their images but sometimes also in the actual objects of perception, in relation to a predetermined end, there is no reason to adopt Köhler's 'insight' as an explanatory concept in chimpanzee psychology; optical-dynamic processes are sufficient.] Literaturbericht.

Bd. lxxxix., Heft 4 bis 6. F. HILLEBRAND. 'Zur Theorie der stroboskopischen Bewegungen,' i. [The Wertheimer phenomenon cannot be accounted for by retinal processes alone; it requires the cooperation of that factor, known as the vehicle of 'absolute' localisation, which was introduced to explain why visual objects remain at rest, under voluntary movement of regard, although the images on the retina have shifted. This factor depends on attention; the retinal space-values are revalued (compensated) by transference of the point of attention (*Aufmerksamkeitsort*); and the effect of attention is complete distinctness of the terminal point of the intended movement. Stroboscopic movement is the expression of a passage through all stages between null and total revaluation; it is the correlate of the gradual process of revaluation.—This instalment discusses the circumstances which influence the mode of stroboscopic movement, and stroboscopic movement under fixed regard.] V. HAECKER und T. ZIEHEN. 'Ueber die Erblichkeit der musikalischen Begabung,' ii. [Examines the distribution of the descendants of discordant marriages among the various biological types. Concludes that males are in general somewhat more receptive than females to transmitted musical endowment, and that the mother is in general somewhat more effective than the father for such transmission. Begins the discussion of the result of positively concordant marriages.] J. LINDWORSKY. 'Umrissskizze zu einer theoretischen Psychologie.' [(1) There are two kinds of psychical processes. The one (successive contrast, spontaneous reproduction, the arousal of sense-feeling) shows indifference of content, is physiologically conditioned, and is palpable; the other (the apprehension of relation, conation) is not indifferent as regards content, is psychologically conditioned, and is impalpable. The second class brings psychology into relation with the mental sciences. (2) The general law of the conscious life is that all conscious phenomena take place in an experiencing subject. There are also special laws: the law of correlation (every psychophysical process has its correlated psychical); the law of continuity; the law of apprehension of relation; the law of conation (reciprocal relation of conation and value); the law of attentional effect (the will can, within limits, enhance the intensity of ideas).] Literaturbericht. G. A. ROEMER. 'Berichtigung.' [Reply to Stern's review of Rorschach's *Psychodiagnostik*.]

Bd. xc., Heft 1 und 2. F. HILLEBRAND. 'Zur Theorie der stroboskopischen Bewegungen,' ii. [Discusses, under various heads, the dependence of stroboscopic movement on change of magnitude of excitation; brings direct proof of the shift of the field of vision in stroboscopic movements; summarises the theory. Two appendices deal (1) with preceding theories (Wertheimer, Linke, Koffka) and (2) with the author's experimental technique.] E. RUBIN. 'Zur Psychophysik der Geradheit.' [(1) There are likenesses and differences between physical and psychological straightness. Thus, one can no more see two straight lines connecting the same two points than one can construct such lines geometrically; lines to be seen apart must diverge 30″, but lines to be seen both as straight must not diverge by more than 22″. On the other hand, two lines may appear of the same length, and yet one may look straight and the other just noticeably curved. (2) Arrangements of discrete points etc. may give the appearance of straightness; here, however, the relation of grouping (*Zusammengehörigkeit*), which has no physical parallel, plays a large part. Various illustrative figures are given. (3) Attitudes have a direct influence upon the limen of curvature as determined by minimal changes; in the method of constant stimuli these attitudes are replaced by tendencies to bend the line to right or left and by direction of attention. The limens yielded by the second method are the smaller.] T. SCHJELDERUP-EBBE. 'Soziale Verhältnisse bei Vögeln.' [Observations on a large number of species confirm the writer's previous report: always a social order of rank is formed, whether between birds of the same species or in mixed groups.] Literaturbericht. G. E. MUELLER. 'Nachruf.' [Notice of the work of W. Baade.] Dr. HAUSCHILD. 'Notiz.' [Exposition of handwriting, drawings, etc., of psychopathological subjects.]

Bd. xc., Heft 3 bis 6. H. ROTHSCHILD. 'Untersuchungen über die sogenannten Zöllnerschen anorthoskopischen Zerrbilder.' [If visual figures are moved horizontally behind a vertical slit, at high speeds they look narrower and at low speeds broader than they appear under ordinary conditions (Zöllner). The writer finds that this effect cannot depend upon eye-movement; experiments with unequivocal and with equivocal figures prove alike that the chief factors are difference between figure and ground, and contour of figure. The observation that a figure, though narrowed, may still look wider than the slit through which it is seen is not explained.] J. BAPPERT. 'Neue Untersuchungen zum Problem des Verhältnisses von Akkommodation und Konvergenz zur Wahrnehmung der Tiefe.' [Experiments with edges, rods and luminous points show that neither state nor change of state of accommodation and convergence, nor yet the impulse to such change, is a direct factor in the perception of depth; accommodation and convergence are, however, directly concerned with apparent magnitude. Even when a change of accommodation and convergence is perceived, the perception is often wholly indeterminate as regards distance; and its voluntary initiation does not give it determinateness.] V. HAECKER und T. ZIEHEN. 'Ueber die Erblichkeit der musikalischen Begabung,' iii. [Concludes the exposition of facts concerning descendants (negatively concordant marriages) and ascendants; there is general agreement between the two sets of results. Proceeds to discuss components, development, and correlations of musical endowment.—The whole material suggests a modified *Pisum*-type; evidence of extra-Mendelian modes is disappearingly small. It is surprising that the negatively concordant marriages yield relatively numerous plus and even double-plus descendants; the possibilities of explanation are set forth at length. The curve of development shows two peaks. In the male sex there is correlation of musical with artistic (graphic) and still more definitely with poetic endowment; the case for mathematics is not made out.] M. FLOORS. 'Ueber den Einfluss der Grössenvariierung bei Gedächtnisleistungen.' [Experiments undertaken to determine the influence of variation of the size of figures upon their recognition and upon their capacity to reproduce, and to be reproduced by, associated syllables. The best results are obtained when the figures impressed and tested remain of the same size. If the figures tested are of different size from those impressed, the best results are obtained when the figures impressed have themselves been of various sizes. If the test is made by way of the associated syllables, the best results are obtained when the corresponding figures have remained of constant size during impression. With adults, large figures are more favorable for impression than small (confirmation of P. Meyer's thesis).] R. HENNIG. 'Neue Untersuchungen zu einem Fall von abnormem Datengedächtnis.' [Cf. vols. 10 and 49. Instances of retentiveness; sympathetic and unsympathetic dates; mnemonic devices.] Literaturbericht. G. E. MUELLER. 'Gesellschaft für experimentelle Psychologie.'

Archives de psychologie, tome xviii., nos. 1, 2. E. CLAPARÈDE. 'Théodore Flournoy, sa vie et son oeuvre, 1854—1920: avec un portrait.' T. FLOURNOY. 'L'idée centrale de la critique de la raison pure.' [From notes written in 1884 or 1885 for a lecture-course. The heart of the Critique is the transcendental analytics, whose two books give a rational justification of the application to scientific objects of our a priori notions, and the development of the general principles grounded in these notions.] M. J. LARGUIER DES BANCELS. 'L'abîme de Pascal.' [The story of Pascal's 'precipice' need not be doubted. Janet has described a similar case; E. Jones has analysed a closely related phobia; and Pascal's niece gives details of his early childhood which may easily be brought into connection with his affliction.] J. PIAGET. 'Une forme verbale de la comparaison chez l'enfant: un cas de transition entre le jugement prédicatif et le jugement de relation.' [Two forms of the test "A is fairer than B but darker than C: who is darker—B or C?" were given to two groups (37, 20) of primary schoolboys aged 7 to 14. It appears that the earliest judgments are predicative (judgments of classes). Between these and judgments of relation proper lie intermediate forms, in which relation is introduced within the classes (prerelation), while the classes themselves multiply and interfere. The writer finds that the form of judgment correlates with (acts and reacts upon) the field of attention.] Recueil de faits: documents et discussions. C. WERNER. 'Réunions des philosophes de la suisse romande: xv. Lausanne, 20 juin 1920; xvi. Lausanne, 12 juin 1921.' E. CLAPARÈDE. 'Ire et IIme conférences internationales d'orientation professionnelle: I. Genève, 27-28 septembre 1920; II. Barcelone, 28-30 septembre 1921.' Bibliographie. Necrologie. [A. Leclère, W. Wundt, etc.] Notes diverses.

Psychological Review. Vol. xxviii., no. 3. 'In Memory of Wilhelm Wundt, by his American students.' [Brief notes by seventeen students, with portrait.] J. A. MELROSE. 'The Structure of Animal Learning.' [The lowest animal learning begins with organic fixation, the defining and fixing of inherited behavior by practice. This is followed by organic spatial accommodation, by learning through the repetition of direct sense-stimuli and the assimilation of the result; and this, in turn, by organic choice or adaptation to functional meaning, which is learning through the repetition and shortening of serial reaction. Higher animal learning begins, in animals endowed with smell and sight, by way of organic association, the fore-stage of association by contiguity; odor becomes connected with food, and so a cross-connection is made and a new term of adaptive value is added. This is followed, in the case of animals with sight, by use of the organic concept; an animal in a new situation may respond in a way that shows the influence of past experience with like concretes and yet that is not wholly determined by such experience. Finally, the highest animals below man possess the organic judgment; their behavior shows association of complexes, of relations; there is close accommodation to the general problems of the environment. Throughout the entire field of learning we thus find three constants: repetition of stimuli and response, synthetic assimilation of effects, and the projection of the integrated result of these two in behavior.] F. T. RUSSELL. 'A Poet's Portrayal of Emotion.' [Discusses and illustrates Browning's treatment of disgust, envy, fear, anger, grief, joy, pity, shame and adoration. The poet shows how the ephemeral and discordant emotions of psychology may be transformed into sentiments, convictions and intelligent behavior worthy of the dignity of human life.]

Vol. xxviii., no. 4. H. B. ENGLISH. 'Dynamic Psychology and the Problem of Motivation.' [Pleads for a theory of motivation broader than hedonism or the instinct-theory. The dynamics of human behavior reside not only in the innate mechanisms but also in any relatively permanent psychophysical disposition (habits).] H. C. WARREN. 'Psychology and the Central Nervous System.' [Psychology deals with stimulation of organisms by their environment, central nervous activity resulting therefrom, and responsive activity of whatever sort consequent on this central activity. Since the essential feature of the stimulus-response process is constituted by the operations in the central part of the nervous arc, behavior and introspection are coordinate methods of psychological enquiry. To these the neurological method must be added. The double-aspect theory provides the best regulative principle for psychology.] A. F. BRONNER. 'Apperceptive Abilities.' [Apperception, as "the 'sizing-up of a situation,' the interpretation of all the aspects, each in the light of the other," is a particularised ability, among both the mentally normal and the defective, that has prognostic value and important social implications. The Pictorial Completion test measures little else than apperception.] A. B. WOLFE. 'The Motivation of Radicalism.' [The individual who suffers from obstructed interests has three ways in which to readjust himself to his environment: repression, substitution and transference, and reinforcement. The first has no connection with radicalism. The second, commonly called sublimation, may lead either to a symbolical conservatism or to a symbolical radicalism; this radicalism is usually derived from egotistic interest, and is likely to be superficial, emotional, ineffective. Reinforcement alone gives drive and consistency to radical movements; it may make radicalism, as the result of balked social sympathy, a powerful social force.] G. M. STRATTON. 'The Control of Another Person by Obscure Signs.' [Tests of the muscle-reading sort, with and without contact (slack watch-chain from hand to hand), made on a professional exhibitor. When touch was excluded visual aid was received from obscure and fleeting signs given unintentionally by the 'guide;' when all such visual cues were also excluded, the successes dropped to the number indicated by pure chance.]

Vol. xxviii., no. 5. W. McDOUGALL. 'Belief as a Derived Emotion.' [Belief is neither a primary nor a compound emotion, but one of the 'derived' emotions which may accompany and qualify the operation of any strong desire; it thus belongs to the group of which confidence, hope, anxiety, despondency, despair and regret are the principal members. Indeed, what confidence is on the place of practical bodily striving, that is belief on the purely intellectual plane. In its fullest sense, belief is always preceded by doubt, deliberation, and choice of means.] J. R. KANTOR. 'How do we Acquire our Basic Reactions?' [Maintains, against any doctrine of innate powers, that "each stage of our development represents the acquisition of response systems, by means of the modification of previously acquired reaction systems and through the instrumentality of newer forms of stimulation, that is to say, through contact with new types of objects and conditions." The contact (stimulation) may be physical, personal, cultural; its process may be casual or deliberate. The author supports his view by an analysis of various reaction types: adaptive, compensatory, manipulative, expressive, approbative, accommodative, etc.] M. W. CALKINS. 'Fact and Inference in Raymond Wheeler's Doctrine of Will and Self-Activity.' [The records bear unequivocal testimony to the occurrence of a consciousness of self which their writers do not and could not analyse into sensation-complexes and imagery. The self in volition is thus both kinaesthetically and 'actively' conscious.] E. L. THORNDIKE. 'The Correlation between Interests and Abilities in College Courses.' [Further inferences from Bridges' data (July, 1920) corroborate the idea of a very close relation between the order of an individual's interest and the order of his abilities.] C. L. Hull. 'A Device for Determining Coefficients of Partial Correlation.' [Consists of scaled sum-and-difference chart of Kelley's *A* and *B* functions, and transparent stencil.]

Vol. xxviii., no. 6. J. R. Kantor. 'Association as a Fundamental Process of Objective Psychology.' [The Associationists magnified a single kind of associational process, namely, connections between implicit responses. To an objective and naturalistic psychology associational processes, as (1) the processes of organising connections between stimuli and acts and (2) the operation of these reactions when formed, appear as a fundamental basis of all behavior. Such specific reactions to stimuli as learning, thinking, and the manipulations of skill are all operations of connections between stimuli and responses; and psychological capacities of various sorts may be looked upon as the associations of many responses and stimuli of some given type, all of which in their operation give the individual a series of general competencies serviceable in his various adjustments. 'Laws' thus make way for innumerable contributing conditions. We may, however, distinguish modes (*e.g.*, primary and secondary, temporary and inseparable, original and formed, logical and conventional, direct and indirect) and levels (reflex, unintentional, intentional, critical) of associative formation. Finally, since association consists of the organisations of stimuli and responses, it cannot have a neural basis.] J. E. Boodin. 'Sensation, Imagination and Consciousness.' [The centres of the central nervous system form a hierarchy of energy-patterns, not storehouses or factories of sensory or imaginal content; the content is due to lines of motion connecting these energy-patterns with the sense-organs. The processes of suppression, selection and integration at the various neural levels are due, not to consciousness, but to the characteristic energy-patterns of the levels. Consciousness need not be limited to a particular level, though we have evidence of it only at the level of meaning-patterns; it contributes, in connection with energy-patterns and their excitement, the bare fact of awareness; it is not an explanatory principle. Individual behavior may be explained in terms of neural patterns and their stimulation; the concept of mind, as a system of intersubjective meanings or valuations and of controls as resulting therefrom, becomes necessary only when we take account of social relations.] H. C. Warren. 'Some Unusual Visual After-Effects.' [Reports delayed after-sensations; a prolonged after-sensation of glare; and observations of voluntary and involuntary visualisation. The experiences are brought into relation with those of Urbantschitsch and Jaensch.]

Vol. xxix., no. 1. W. McDougall. 'Prolegomena to Psychology.' [The aim of psychology is to render our knowledge of human nature more exact and systematic, that we may control ourselves more wisely and influence our fellows more effectively. Psychology has three direct sources of information: introspection, the noticing of one's own experiences; observation of the conditions or occasions of experiences; observation of the expressions of experiences. These three means of approach have been used with varying emphasis, with varying motivation, with various supporting hypotheses; thus the historical school-systems have arisen. Today the student is confronted by a number of mechanistic readings of psychology (behaviorism, sensationism, associationism, presentationism). But the fundamental principles of these psychologies are, at the best, matters of overhasty dogmatism. We cannot avoid the concept of 'mind,' as that which expresses itself in the experience and behavior of the individual organism; and we must begin our psychology by gathering all possible facts of human experience and behavior and by inferring from them the nature and structure of the mind. Psychology thus becomes the science, empirical and positive, of the human mind. At the outset we note (1) that experience is always of something and of some one, so that whenever we refer to a fact of experience we imply some one thinking of something, and (2) that it is necessary to distinguish clearly facts of mental activity and facts of mental structure.] E. C. Tolman. 'A New Formula for Behaviorism.' [Advocates a non-physiological behaviorism, to be worked out in terms of stimulating agency, behavior-cue, behavior-object and behavior-act. The theoretical advantage is that all the various methods of actual psychology are thus brought under a single rubric; the practical advantage, that the treatment of motive, determining tendency, purpose, etc. becomes easier and more adequate.] A. A. Roback. 'Intelligence and Behavior.' [A survey of recent definitions shows that behaviorism has not furthered our knowledge as regards the problem of intelligence. This remains primarily what it always was: the problem of understanding or comprehension. The intelligent man is he whose higher mental operations are as a rule carried out on some more or less consistent plan.] G. S. Gates. 'The Meaning of the Term "Practice", [Practice as defined may be taken to mean mere repetition, repetition with gain, a certain type of consciousness, phenomena or processes occurring in the individual when an act is repeated. It is best to adopt the widest meaning, of mere repetition of any process or exercise of any function; further enquiry may then proceed from this basis. Customary practice-phrases must necessarily be altered.] H. R. Crosland. 'Conscious Analysis in Learning,' [Critique of Perrin. Conscious processes are criteria for the presence of certain cortical integrations and the workings of integrated mechanisms, which frequently can be known in no other fashion.]

Vol. xxix., no. 2. M. F. Washburn. 'Introspection as an Objective Method.' [The first doctrine of a radical behaviorism, that consciousness does not exist, is traversed by the existence of sensory qualities. The second doctrine, that psychology should abandon introspection, is less easily met. But introspection may be redefined, from the behavioristic point of view, as symptomatic language behavior. So considered, it (1) yields results as trustworthy as those accepted in other descriptive sciences, which themselves, indeed, often rely upon introspective evidence. It is most vulnerable in cases where the reactions reported by language alone have no immediate external stimulus. Moreover, it (2) has furnished data which the behaviorist cannot afford to disregard; which, in fact, he takes for granted. Witness Sherrington's appeal to observations on flicker; Head's appeal to the Weber compass experiment; and, most striking instance of all, the behaviorists' own theory of learning: it is to introspection that we owe our knowledge of kinaesthesis, or (in behavioristic phrase) it is the evidence of language reactions associated with the contractions of the muscles of various parts of the body which is most important for the theory that habit and association rest on a kinaesthetic or proprioceptive basis.] J. A. Melrose. 'The Crux of the Psychological Problem.' [The central problem of psychology is the problem of conscious 'thinking' reflection. Over against it, the older psychology has the theoretical, behaviorism the practical advantage. The former, nevertheless, has failed to produce a generally accepted theory, and the latter may be improved out of its doctrinaire intellectualism. Let us admit, as behaviorists, that the sign-system structure of the reaction is more important to the organism's expression and more accessible to science than any muscular reaction; that distinctively human behavior is the adaptive use of a vocal sign-system known as language. Then the problem stands clear before us.] F. L. Allport. 'A Physiological-Genetic Theory of Feeling and Emotion.' [The craniosacral division of the autonomic, supplemented at times by the cerebrospinal system, produces the visceral responses represented in consciousness as pleasantness; the sympathetic division, those represented as unpleasantness. The differentiae of emotions within a single affective class are to be sought in the sensations arising from stimulation of the proprioceptors in the muscles, tendons, joints of the somatic part of the organism.] E. C. Tolman. 'Concerning the Sensation Quality: a Behavioristic Account.' [The world of stimuli presents for every organism a particular system of likenesses and differences. These may be registered, behavioristically, in term-characters. Never, not even in the case of our fellow-man, can the immediately experienced qualia be known.] J. L. Mursell. 'The Stimulus-Response Relation.' [Both behavior psychology and self psychology are working hypotheses, whose representatives have no necessary quarrel. In the stimulus-response relation, taken broadly, we have an adequate basis for an objective psychology: against this position current criticism (Warren, Calkins) is ineffective.]

Vol. xxix., no. 3. J. L. Mursell. 'The Ontogenetic Significance of Instinct, Habit, and Intelligence.' [Instincts constitute the basic tendencies or impulses of the individual, and can be modified but not eliminated by habit. Habit is essentially a matter of function. Its value is that it adapts original tendencies to a complex environment, and makes action more 'intelligent,' *i.e.*, more efficient and successful. This is the normal ontogenetic sequence.] C. Ladd-Franklin. 'Practical Logic and Color Theories.' [Certain recent theories of color present instructive instances of sins against fundamental methodological principles. Troland's theories are taken in evidence, and the author's theory is defended against Troland's criticisms.] L. T. Troland. 'The Significance of Psychical Monism for Psychological Theory.' [Exposition, with bibliography. The world of external reality is a complex system of psychical entities generally similar in character to the internal system which comprises any thinking individual. The system of physics is an unfinished product of the efforts of such an individual to formulate the abstract relations existing between the components of this external system. The physical scheme is thus formally identical with the external psychical scheme, but its terms or qualities are different.] R. H. Wheeler and T. D. Cutsforth. 'Synaesthesia, a Form of Perception.' [In the case examined, that of a blind subject with stereotyped synaesthetic imagery, the secondary process is essential to perception; the primary process by itself is only an indescribable something. Sensation develops to perception by way of an attention-shift from either process to the other; the simplest describable mental process is thus a dual phenomenon.] T. R. Garth. 'A Comparison of Mental Abilities of Mixed and Full-Blood Indians on a Basis of Education.' [Groups of mixed (165) and full-blood (215) Indians, of the same school-training as indicated by grade attainment, were given the same tests (of higher mental processes) under controlled conditions. The mixed excelled the full-bloods by about 11%; the best scores were about evenly divided. The difference may, after all, be nurtural and not racial, since pure-blood thinking must meet interferences from which mixed-blood thinking is relatively free.] Discussion. V. S. Yarros. 'Induction and Radical "Psychology".' [Not all radicals are made such by thwarted ambition: the idealism of youth and the clear-sightedness of adult thought play their part, as well as bias and personal grievance. An inductive study is needed.]

Vol. xxix., no. 4. S. I. Franz. 'Psychology and Psychiatry.' [Theoretically, the gap between psychology and psychiatry is bridged by abnormal psychology and psychopathology; but as intermediate technologies with special groups of workers these disciplines do not yet exist. Since in both psychology and psychiatry a knowledge of abnormalities and of pathological conditions is of value, investigation in the middle field should be urged; the oftener the extremes are brought into contact, the better also will be the mutual understanding of their representatives.] B. H. Bode. 'What is Psychology?' [The psychology of mental states is moribund; mechanistic behaviorism lets the distinctively psychological evaporate. The only hope lies in a psychology of purposive behavior, whose chief concern is with the unique and irreducible nature of the stimulus.] W. B. PILLSBURY. 'Suggestions for a Compromise of Existing Controversies in Psychology.' [The outstanding differences in the psychology of today are really the age-old differences of animism and materialism. They are not harmonisable, and not amenable to investigation, because they are rooted in something like instinct. At best there may (as in the days of Wundt and James) be a specious truce, which is inevitably to be broken.] J. R. KANTOR. 'An Analysis of Psychological Language Data.' [Objective psychology has to set linguistic reactions in their proper perspective in relation to the other coordinating functioning responses to stimuli. Language responses are inherently indirect or referential adaptations to stimuli, involving an adjustment stimulus (acted on or reacted to) and an auxiliary stimulus (person using language). Language and thought are not identical; they constitute different types of adaptive response and they operate differently; thus, language is always indirect, thought always direct adaptation. Language reactions may be divided modally into morphological and functional; the latter are either expressive or communicative. As communicative, language reaction systems may be primarily transmissive (*e.g.*, vocal speech) or primarily receptive (*e.g.*, reading and understanding). Words, to be psychological data, must be either stimuli or responses.] L. M. TERMAN. 'A New Approach to the Study of Genius.' [Impressionistic and anecdotal methods (Lombroso) gave way to inductive and statistical study of biographical material (Galton), replaced in its turn by investigation of the living (Cattell). The next step is the study of genius in the making, of gifted children. The writer reviews the work done and doing at Stanford University, and gives a few tentative conclusions: in the schools of larger cities, 1 pupil in 250 has an IQ above 140; Jewish cases are in excess; some cases are of very inferior social status, etc.] R. O. FILTER. 'A Practical Definition of Character.' [Intellectual and aesthetic values have been ruled out of the concept of character, and moral values are now following them. The emphasis is on force rather than on direction of activity, on quality of behavior as strong, persistent, ready, rapid, etc., rather than on its value as good, right, wise, etc.] Discussion. F. A. PERRIN. 'Conscious Analysis, Introspection, and Behaviorism.' [Reply to Crosland.] E. B. TITCHENER. 'The Term "Practice."' [Reply to Gates.]

Vol. xxix., no. 5. A. P. WEISS. 'Behavior and the Central Nervous System.' [Behaviorism studies the origin and development of those bodily movements (reactions) of the organism which establish its social status. Warren's limitation of the behaviorist to a study of the responsive phase of the sensory-cerebro-motor system is inacceptable, since behaviorism takes account of the stimulating conditions from the standpoint of the social categories into which they may be classified, and of responses from the standpoint of their significance as stimuli (signals) in establishing the social status of the organism. In general, there is no bridge between the two doctrines; the gap will probably grow wider.] Z. Y. KUO. 'How are our Instincts Acquired?' [Bodily movements (units of reaction), chaotic at birth, are presently integrated, simultaneously and temporally. Behavior-sets, developed through the establishment of conditioned responses, serve to explain purposive behavior in purely objective and mechanistic terms. They help us to classify temporal integrations according to the end-results accomplished; and in classifying these integrations we classify the instincts.] J. PETERSON. 'Intelligence and Learning.' [The trend of psychology toward intelligence-tests has brought theoretical problems along with practical results. We assume, *e.g.*, but we do not know, that there is practically perfect correlation between degrees of ability to learn the habits and ideas on which the tests are based and degrees of intelligence. Another problem is the general nature of mental growth. It is, again, a grave error to identify the IQ with intelligence; it is gravely confusing to identify outright learning ability with intelligence. No doubt, the two are related; but we need a more functional analysis of both. The older passive associationism, against which the pseudo-psychology of the subconscious marks an unreasoned revolt, must be set in its proper place by emphasis on the inner urge of the metabolic changes as important factors in instincts. Only a dynamic psychology can tell us how errors become eliminated in trial-and-error learning, while certain acts finally survive in habits.] J. A. MELROSE. 'The Organismal Point of View in the Study of Motor and Mental Learning.' [The approach must be made by way of the total organism and the organism's most characteristic behavior. Thus, if a definite constant pattern is found in the behavior of language-use (characteristic of man), which is unaccounted for whether by lower types of learning or by the constants of the environmental situation in which language-habits are formed, then this pattern must be due to man's inherited structure. As definitely correlated with that structure, the constant pattern would represent the structural difference between thinking and animal (motor) learning.] Discussion. C. SPEARMAN. 'A Friendly Challenge to Professor Thorndike.'

Vol. xxix., no. 6. S. W. FERNBERGER. 'Behavior *versus* Introspective Psychology.' [The sciences of consciousness and of behavior are separate and independent. The older psychology then becomes a speculative procedure attempting to correlate the data of these two sciences with each other and with physiology.] H. N. WIEMAN. 'The Unique in Human Behavior.' [Minor units of behavior are integrated into (animal and human) instincts; instincts are integrated into (specifically human) interests. Instinct adapts the organism to a specific situation; interest adapts it to an ever more inclusive situation, making simultaneously every possible contact with environment. Hence arise the unique behavior-traits of philosophy, religion, science, art, love.] R. H. WHEELER. 'Analyzed *versus* Unanalyzed Experience.' [Reply to Calkins. The data of Ach's and Michotte's investigation were unanalyzed meanings; those of the author's, analyzed meanings. Illustrations are given.] C. M. CAMPBELL. 'What can Psychology Contribute to our Knowledge of the Mechanism of Mental Disorder? With some Comments on the Term "Depression."' [Confronted with a case of morbid depression, the psychiatrist finds no useful formula already prepared by biochemist, physiologist or psychologist. Analysing the patient's own account, he comes upon a very complicated biological situation, in which were present unsatisfied cravings, vague yearnings, organic desires, repressed fantasies, stifled irritiations, strivings toward religious solutions. Psychology should evidently train its students in the analysis of complex emotional reactions.] C. E. FERREE and G. RAND. 'The Effect of Variations of the Intensity of the Illumination of the Perimeter Arm on the Determination of the Color Fields.' [With very high intensities of stimulus the limits of R, Y, B are coincident with the limits of white-light vision; those of G are less wide. With medium intensities, and the relative energies of the prismatic spectrum of a Nernst filament, the limits (concentric) are, from widest to narrowest, R, Y, B, G. With medium intensities of equal energy the limits of R, Y, B interlace and crisscross; those of G are narrower. For pigment stimuli we obtain, according to special conditions, zones either interlacing or concentric in the order, widest to narrowest, R, B, G or B, R, G.] J. M. RIFE. 'Types of Dextrality.' [There are three types of right-handedness with three corresponding types of left-handedness. The first type comprises those whose bimanual preference is divided between R and L. The second includes those whose bimanual is the opposite of their unimanual preference. The third embraces those whose unimanual and bimanual preference is the same. Unless these types are distinguished, misstatements of fact are likely.] H. C. WARREN. 'The Significance of Neural Adjustment.' [The attempt to describe organic reactions solely in terms of the initial and terminal data (stimulus and response) ignores the central factor of neural adjustment, which the writer regards as the key to the entire process.] Discussion. M. W. CALKINS. 'The Affiliations of Behaviorism.' [Comments on Washburn and Mursell.]

Arch. f. d. ges. Psychologie. Bd. xliii., Heft I. F. KIESOW. 'Ueber Metallglanz im stereoskopischen Sehen.' [Under the conditions laid down by Rood in 1861 it is possible to produce a true metallic lustre in stereoscopic vision. This fact does not invalidate Kirschmann's theory of lustre (vol. xli.), since the effect is due to assimilative processes.] F. KIESOW. 'Ueber die taktile Unterschiedsempfindlichkeit bei sukzessiver Reizung einzelner Empfindungsorgane.' [Experiments with successive stimulation of the Meissner corpuscles of the hairless wrist give a constancy of the *DL* between the limits 3 and 6 gr/mm at one-seventh; above and below (1 to 8 gr/mm) there are slight deviations. The writer describes a new hair-aesthesiometer.] G. STOERRING. 'Zur Psychologie der Erinnerungsgewissheit.' [Experiments on the reproduction by drawing, immediately and at weekly intervals for five weeks, of meaningless forms exposed for 1, 3, 6 or 10 min. The principal factors in the assurance of recollection are (1) verificatory reproductive processes and (2) the consciousness of insistence (*Sichaufdrängen*) whether of the idea of object or of the observer's condition during impression. This insistence is very like the insistence which, in processes of judgment, is directly connected with the consciousness of validity. Hence there is a strong tendency, when it appears, toward a reproduction of the consciousness of assurance; there is no actual assurance of recollection. Strongly visual observers report an insistence of the idea of object itself; but probably there is here an unremarked consciousness of insistence.] S. FISCHER. 'Ueber das Entstehen und Verstehen von Namen, mit einem Beitrage zur Lehre von den transkortikalen Aphasien,' ii. [Our experience in understanding a word is made up of (*a*) the auditory perception of the sound-complex with passive direction upon the object of perception; (*b*) a quality of familiarity; (*c*) either a consciousness of direction or an intentional experience that points either to a sphere of meaning or to a knowing; (*d*) thereupon (or concomitantly) either an experience of knowing or a consciousness of sphere. Understanding is now complete, though (*e*) illustrative palpable ideas may appear. Our experience in naming a familiar object is made up of (*a*) the experience of the perceptive content with passive direction upon the object of perception; the relations whereby the object was first apprehended may be dispositionally present; (*b*) a quality of familiarity; (*c*) the actualisation of the sound-complex by associative reproductive-tendency; (*d*) earlier (or concomitantly) an intention toward the object of perception, whereby the sound becomes denomination. The two processes are radically different. So far, then, as the 'concept' (*Begriff, Begriffsfeld*) of the aphasic schemata of Wernicke-Lichtheim and Goldstein confuses them, so far are these schemata themselves psychologically unacceptable.] E. MALLY. 'Lineare Regressionen und mittleres Verhältnis.' [Wirth's mean ratio, in the sense of the mean line, gives a fixed relation between the observed magnitudes that has been, so far as possible, freed of their accidental variations and is reduced to the 'normal conditions' of the uncontrolled influences. Another mean ratio, in the sense of the mean regression,—a value demanded by the calculus of correlation,—gives a more direct description of the factual status, since it shows how, with the given 'accidental' distribution of the values, the deviations are on the average interrelated. The two values themselves are definitely related.] Literaturbericht: Referate. W. WIRTH. 'Zur Kritik einer verstehenden Psychologie der Weltanschauungen.' [Criticism, general and special, of K. Jaspers' "Psychologie der Weltanschauungen."] S. FISCHER. 'O. Bumke, Zur Diagnose der Geisteskrankheiten; S. Freud, Zur Psychopathologie des Alltagslebens.'

NOTES

THE EXPRESSION OF SIMPLE FEELING

The occasion of this Note is a quotation recently made from one of our Minor Studies: a quotation which, though literally accurate and made, of course, in all good faith, must nevertheless be misleading to a reader who has not the original paper very clearly in mind. The circumstances are as follows.

In 1921 Young drew from experimental results the conclusion that P, as contrasted with U, is "organically-kinaesthetically negative." He qualified this conclusion by the phrase "so far as our data go," and by the remark that "any result is a function of conditions."[1] It seemed to us that it would be fairly easy to set up conditions that should show P as "organically-kinaesthetically" positive: and Corwin published our results later in the same year.[2]

It is from Corwin's paper that the quotation is taken to which I just now referred. "It is impossible, therefore, to accept Corwin's interpretation," so Young now writes, "that 'the direct response of the organism to P is .... a definite activity of pursuit or of tendencies to pursuit.'"[3] When I read this quotation, I wondered that any student of mine should have made the statement, and not less that I should have allowed it to appear in print. In point of fact—though the quotation is literally correct—Corwin said something very different: so different that Young himself, had he not been prepossessed by his own argument, would have seen that the abbreviated sentence is misleading. Corwin wrote: "The direct response of the organism to P is, as stated above, either relaxation *with a certain degree of expansion*, if the stimulus is weak or stationary; or, if the stimulus is intense, and if the source of the P is withdrawn, a definite activity of pursuit or of tendencies to pursuit." The occurrence of the 'activity' is thus doubly qualified: the stimulus must be strong, and must also be in course of movement away from *O*. It was precisely our point that the result was a function of its conditions; and we were not 'interpreting' but recording.

The words in italics were so printed in the original paper. Every set of psychological experiments is likely to raise issues of systematic interest; and such an issue is here in question. A Minor Study is not, however, the place for systematic discussion: we were content with the bare suggestion that 'expansion' and 'contraction' might be better terms, in this particular context, than 'pursuit' and 'avoidance,'[4] and with the indications afforded by the reports of our *Os*. If we had been led into further discussion we should hardly, I think, have mentioned the teleological theories of feeling without reference at any rate to Külpe,[5] or the movements of sensory attention without reference at any rate to Jastrow.[6]

E. B. T.

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[1] P. T. Young, Pleasantness and Unpleasantness in Relation to Organic Response, this JOURNAL, xxxii., 1921, 53.

[2] G. H. Corwin, The Involuntary Response to Pleasantness, *ibid.*, 563 ff.

[3] P. T. Young, Movements of Pursuit and Avoidance as Expressions of Simple Feeling, *ibid.*, xxxiii., 1922, 522.

[4] On the general question see also E. B. Titchener, Text-book of Psychology, 1910, 487.

[5] O. Külpe, *Grundriss der Psychologie*, 1893, 276 ff.

[6] J. Jastrow (with the assistance of Helen West), A Study of Involuntary Movements, this JOURNAL, iv., 1892, 398 ff.; Fact and Fable in Psychology, 1900, 312 ff.

PROFESSOR MOORE'S "NOTE ON 'AFFIRMATION AND NEGATION'"

Professor Moore's Note in the July, 1922 (33, 449 f.) number of this JOURNAL, in which he criticizes my report of experiments on "Affirmation and Negation" in the January, 1922 (33, 84-96) number, seems to call for a reply. Professor Moore agrees that my data show that a positive judgment (or affirmation) requires less time than a negative judgment (or negation). Then he says: "To infer further from this, however, that consequently there is a psychological *opposition* between a logical affirmation and a logical negation is hardly warranted." But nowhere, in the article referred to, was any such inference made.

His closing sentences also deserve consideration. "In other words, the only *opposition* between affirmation and negation seems to be a *logical* opposition: the only *psychological* difference is a difference in *duration* of process, not an opposition at all. I do not see, therefore, that Dr. Griffitts has proved his point as a result of his experiments." As a matter of fact, an attempt to prove any such point was so foreign to what I actually tried to do that in my article nothing at all was said regarding the possibility, even, of there being any such opposition. My aim was merely to determine whether or not there are any differences—conscious or not—in any phase or aspect of affirmation and negation. I explicitly stated that I was interested primarily in the objective results, and that I therefore took pains to prevent any introspective tasks from interfering with the securing of valid objective data.

Nevertheless, I did secure some introspective data, which I am ready to admit are in themselves inadequate; Professor Moore may therefore be justified in overlooking or ignoring them. If, however, these introspective reports have any value, they indicate differences in the conscious aspects of the processes of affirmation and negation. He admits only the differences in duration, while I believe there are other differences; and the careful reader will find that a good deal of direct and indirect evidence of such differences was reported. But these neurological and psychological differences do not imply neurological or psychological *opposition*.

On p. 86 l. 4 of my article, the expression "made two reactions" should read "made two wrong reactions."

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THE STERN VARIATOR

In 1906 Stern[1] published a note saying that the manufacture of his "Tonvariator" had been transferred from F. Tiessen at Breslau to Max Kohl at Chemnitz. Stern does not say that any change had been made in its structure. The Kohl instrument, however, does not accord with the variator described by Stern[2] in 1902. As made by Tiessen, every degree of arc drawn on the dial of the spiral which lifts the piston represented a change of pitch, say, of 2 vib. The spaces on the dial were all equal, because the piston was set at positions giving equal increments of change in rate of vibration. The equality of spaces made it possible to gear a micrometer to the spiral, by means of which the dial could be set to a fraction of a space and the piston, consequently, could be set to a fraction of a vibration. These adjustments are all specified in Stern's description of 1902. The variators in the Cornell laboratory, however, made by Kohl, do not fulfill the requirements. The spaces on the dial which represent unit-changes in vibration frequency are not equal. This departure from the specified form renders the micrometer useless and interpolation uncertain. If the variators owned by the Cornell laboratory are like others made by Kohl, they are definitely inferior to a variator made according to Stern's specifications. Apparently, Kohl cut a spiral which had approximately the proper slope, set the piston by tuning, turned the spiral until it supported the piston, and then marked the position on the dial attached to the spiral. Any radius which was too short would widen the space between it and the next lower setting, and would narrow the space between it and the next higher setting. A long radius would have the opposite effect. With all radii slightly inaccurate, a haphazard set of unequal spaces would result; and these are what the Kohl dial has. Nevertheless, the instrument is furnished with a micrometer, just as if the spaces were all equal. If made according to Stern's description, it would be necessary to set the piston, to turn the spiral forward the proper number of degrees, and then to cut the radius at that point in the spiral to the length which would just support the piston. It is in this more elaborate and more accurate calibration that the Kohl instruments fail.

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[1] L. W. Stern, Notiz über den Tonvariator, *Ztsch. f. Psychol. u. Physiol.*, 40, 1905-06, 304.

[2] *Id.*, Der Tonvariator, *Ztsch. f. Psychol. u. Physiol.*, 30, 1902, 422-432.

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On The Anthropological Find in Rhodesia

I. *The Finding of the Broken Hill Skull: The Mystery of the Great Bone Cave.* Three reports by: (*a*) William A. Harris, (*b*) A. Smith Woodward, (*c*) Arthur Keith. *Illustrated London News*, 159, 1921, 680-687.

II. *Le crâne néanderthalien de Broken-Hill.* By L. Capitan. *Revue anthropologique*, 32, 1922, 42-47.

III. *The Skull from Broken Hill in Rhodesia.* By G. G. McCurdy. *Am. Anthropologist*, 24, 1922, 97-98.

The "Broken Hill Skull" was found ninety feet below the ground surface in Bone Cave, which has been opened up by the operation of the Rhodesian Broken Hill Development Co., Broken Hill, Northern Rhodesia (near Victoria Falls), So. Africa. Hundreds of tons of bones of small animals now existing in Africa had been removed from the cave. The skeleton, the only human remains found, was unearthed by mine laborers at the extreme interior part of the cave. There was created sufficient interest among the laborers by this for them to send the skull to the "white boss," after which they proceeded to break up the skeleton for mining purposes. There were recovered: a leg bone, collar bone, portion of shoulder blade, portion of pelvis with coccyx attached, a part of the lower jaw, various parts of other bones not identified, and most of the mineralized cast of the body (I, *a*, 680).

The find is undoubtedly human. The measurements by Keith place its brain capacity between that of the average Englishman (about 1480 c.c.) and that of the skull of *La Chapelle-aux-Saints* (Neanderthal race) with a capacity of 1600 c.c. Keith is of the opinion that, even allowing for its great projections of brow-ridges and for possible extra thickness of the bone, it could not fall far short, in point of size of brain, of that of average modern man. The *Pithecanthropus* represents a much smaller skull than this fossil. Woodward attempted to fit the Heidelberg jaw, the largest known human fossil jaw, to this skull, finding it both too short and too narrow. Piltdown has an apelike palate, while the palate of the Broken Hill skull is typically human, "adapted to perfect speech, and bounded by the horse-shoe shape row of large, though absolutely human, teeth" (I, *b*, 682). Keith considers the palate to be less ape-like than even that of the modern negro.

This Rhodesian man had a prominent heavy face surmounted by large brow-ridges, brow-ridges as heavy as, if not heavier than, Neanderthal. The bones of the face are considered by Woodward to be typically human; but their arrangement indicates that the face would be broad and flattened. It is possible that the lower jaw was more prominent than that of Neanderthal man. Woodward states that there are "reminiscences of an apelike ancestor in its face" (I, *b*, 682).

The *foramen magnum* is as far forward as in modern man," and it shows that the skull would be perfectly poised on an upright trunk" (I, *b*, 682), says Woodward, while Keith considers that there would be a hafting of the skull as in Neanderthal man.

The teeth are worn in such a manner as to prove that the lower teeth met the upper edge to edge, a feature common in the dentition of early races. The wisdom teeth are much smaller than the other grinders. Caries had affected the teeth and the bones surrounding the teeth were diseased. It is the opinion of both Woodward and Keith that disease of the teeth is a modern affliction. The teeth diseased were those most likely to be attacked by disease in modern times. Woodward states: "So far as I am aware, this is the only example of caries in teeth of a prehistoric skull hitherto discovered" (I, *b*, 682), while McCurdy makes the contradictory statement: "the teeth had suffered considerably from caries, but perhaps no more so than was the case with the skull from *La Chapelle-aux-Saints*" (III, 98).

This fossil find has not been studied sufficiently as yet to determine absolutely its geological age and race. McCurdy's conclusion is that "with due allowance for variation it would seem reasonable to assume that the man from Broken Hill is a variant of the Neanderthal type" (III, 98). There are paradoxical characters in the skull. The cranial vault is raised, more like man than the Neanderthal. However, this same character appears in *Pithecanthropus*. The profile of the face has very much the aspect of Neanderthal. Keith concludes that this fossil found in Rhodesia "agrees in most features with .... Neanderthal man" (I, *c*, 686), and Capitan summarizes the consensus of opinion to the effect that this skull in all probability is a Neanderthal, but that it is impossible to state exactly.

Woodward does not go so far as to place the Rhodesian man in the Neanderthal group, even considering it of extremely wide variation. His "impression is that the Rhodesian man is of later development than Neanderthal man, of more recent geological date. He is one of the multitude of extinct races yet to be unearthed, who completed their mental equipment before undergoing the change which we regard as refinement of the face" (I, *b*, 682). Woodward bases his contention mainly on the fact that, while Neanderthal's limb bones allowed only for inelegant shoulders and a shuffling gait, the Rhodesian man was found with bones which are in all respects those of ordinary modern man and of a size which would indicate that he stood upright at a height of about six feet. Woodward is the only anthropologist among these writers who appears to take into consideration bones other than the skull.

It is evident that the Broken Hill fossil did not live at so remote a period as known fossil Neanderthals. Capitan raises the question whether he may be a late descendant of the Neanderthal race; holding, furthermore, that it is possible that some Neanderthal descendants may be living in remote corners of Africa to-day. Ten years ago there were found implements of Mousterian culture near Victoria Falls. There have been made numerous such finds in Africa. Capitan considers that we have made our first find of the man who is responsible for these implements.

Because there have been no adequate studies of geological strata in Africa, there is nothing to assist in the placing of Rhodesian man, except European fossils. An ethnological study of Africa might throw some light upon these conjectures. Africa appears to be a valuable source for anthropological exploration in the future.

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MIMICRY IN THE EGGS OF *Cuculus canorus*

(1) Friedrich von Lucanus: Zur Frage der Mimikry der Kuckuckseier, *Journal für Ornithologie,* 69 Jahrgang, Heft 2, April, 1921, 239-258.

(2) Horst Wachs: Zur Aehnlichkeit der Kuckuckseier, *Biologisches Zentralblatt,* 42 Band, Nr. 6, Juni, 1922, 270-278.

That the European cuckoo (*Cuculus canorus*) lays its eggs in other birds' nests is an old and striking observation of popular ornithology, but in the matter of detail even professional ornithologists have long remained in the dark. It has been known, for example, that cuckoo eggs are smaller than might be expected from the size of the bird, and vary considerably in ground-color and markings; but opinion has been divided on the question whether the eggs of the cuckoo resemble the eggs of the birds in whose nests they are laid, some observers asserting that resemblance, if it occurs at all, is rare and accidental, and others that resemblance is the rule. A good deal yet remains to be done before a full explanation of the matter can be offered; but the paper of von Lucanus, noted above, seems to have settled at least the question of resemblance once for all by showing that both resemblance and non-resemblance occur, and approximately in what proportions and in what sorts of nests.

Making use of the great collection of birds' eggs in the *Museum für Naturkunde* in Berlin von Lucanus compared carefully the cuckoo eggs with those among which they had been laid, distinguishing three conditions (close resemblance, tolerable resemblance and no resemblance), and has stated his findings both in concise descriptive paragraphs and in a statistical table. The collection consisted of 728 clutches of eggs from 30 different species with which 765 cuckoo eggs had been laid. The most striking items in the table are the entries for the *Gartengrasmücke* (the Old World warbler *Sylvia borin*) and for the wren, *Troglodytes troglodytes*. The warbler is represented by 481 clutches of eggs with 502 closely similar cuckoo eggs[1]; and the wren by 109 clutches with 120 cuckoo eggs none of which were like wren's eggs. Of the 28 remaining species the number of clutches was much smaller, from one to a dozen or fifteen, and the degree of resemblance varied. With other warblers of the Sylvian genus the resemblance was usually close, while with those of the genus *Phylloscopus* (9 clutches with 10 cuckoo eggs) there were no cases of resemblance. For the rest the figures were mostly scattering, instances of resemblance and its absence often appearing in nests of the same species. If we leave out of account, on the one hand, the cuckoo eggs laid in the nests of the *Gartengrasmücke* and, on the other, those laid in the nests of the wrens and the *Phylloscopus* warblers—as tending unduly to weight the results—there remain 133 cuckoo eggs laid in nests of 27 other sorts of birds. Of these 95 or 72% show resemblance, while 38 or 28% do not. Resemblance is present therefore in general in about 7 cases in 10; and it extends in some instances to practical identity in both ground color and the finest details of the markings.

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[1] Of the 502 cuckoo eggs 453 were extremely close matches with the eggs with which they had been laid. In 49 instances the differences were greater, though still not exceeding the normal range of the *Gartengrasmücke* eggs among themselves, and for this reason were counted by von Lucanus as closely similar. Of the eggs of this species the ground color may be white, dirty white or yellowish, the markings drab, dark brown, reddish, or blue gray and in the form of spots, specks or lines—all of which variety would favor the parasitic tendencies of the cuckoo.

If von Lucanus has settled one question, he has opened up a number of others. How comes it that there should be such consistent resemblance in the case of the Sylvian warblers, and such equally consistent lack of it in the case of the wrens and the *Phylloscopus* warblers? And when there is resemblance, how is the female cuckoo brought to lay in nests containing eggs that hers will match?

The earlier fantastic suggestions that the female cuckoo has the power of determining in advance the color and markings of her unlaid egg in such a way as to make it match those with which it will be deposited, or that the resemblance is due to the food administered by the foster parents to the female cuckoo while still a nestling, are very naturally rejected by von Lucanus in favor of an explanation based on the probable genesis of the habit of parasitic nesting. It seems likely that the ancestors of the European cuckoo began by using a joint nest and brooding in common, as some cuckoos in other parts of the world still do. As time passed the brooding and nest-building instincts weakened in some of the partners; they laid in the joint nest, but omitted to do their share of the rest of the work; *i.e.*, they began to be parasitic on their own species. In their descendants in course of time these instincts wholly disappeared and the habitual use of other birds as foster parents became the price of survival. In selecting nests in which to lay, those nests would be most naturally chosen whose eggs resembled the eggs in the original joint cuckoo nests; for which they might in a manner be taken. A condition similar to this still exists in the case of certain congeners of the cuckoo, which lay eggs of a fixed type in nests of particular species whose eggs theirs closely resemble. The original cuckoo eggs were in all probability very much like those of the *Gartengrasmücke* (*Sylvia borin*), with which they most frequently appear in the Berlin collection (481 clutches with 502 cuckoo eggs). In the earlier stages of its parasitism the cuckoo probably laid in the nests of this species exclusively, a wider variability of its own eggs and a wider range in its selection of nests both arising subsequently. If such were the case, the perfecting of the resemblance could easily be taken care of by natural selection, and distinct varieties of cuckoos would be found imposing their eggs chiefly upon single species, as actually happens in northern Europe where one of the finches suffers and in southern Europe where another warbler (*Sylvia orphea*) is the victim.[1] If at the same time the wrens and the *Phylloscopus* warblers accepted foreign eggs without protest, the lack of resemblance in their cases would also be accounted for.

The data on the tolerance shown by birds of different species toward foreign eggs appear not to be very extensive. Such studies as have been made show great differences among birds of different species and even among different individuals of the same species in this particular, some accepting eggs that differ markedly in size and color from their own and others rejecting those showing but small differences. In view of such variability in the selective factor, von Lucanus is inclined to attach small importance to natural selection as a cause of the resemblances found, holding that natural selection could produce such close resemblances as exist only if *all* eggs that do not match are destroyed, and leaving the possibility open for the operation of other causes.

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[1] For the operation of natural selection it would be necessary, on the one hand, that the female cuckoos should tend to lay their eggs in the nests of one particular species (the species in the nests of which they had themselves been reared or some other) and, on the other hand, that dissimilar eggs should be thrown out by the foster parents or the nest deserted.

In the second of the above papers Dr. Horst Wachs summarizes the foregoing study of von Lucanus' and discusses the problem from his own standpoint. The assumption of von Lucanus, that natural selection can bring about close resemblance only in case *all* cuckoo eggs unlike those in the foster parents' nest are rejected, seems to Wachs unnecessary. Rejection of a reasonable portion of the dissimilar eggs would bring about the same result though less speedily. As a matter of fact we probably have all degrees of intolerance of variant eggs and corresponding thereto all degrees of selective action, and consequently all grades of resemblance. The intolerance of the Sylvian warblers has been high and has held the cuckoo eggs close to their original type. Other cuckoos whose eggs varied from this original type have had to look about for nests having eggs like their own; for Wachs holds that the female cuckoo does not lay an egg to match those in the nest but, laying eggs of a certain sort, she hunts for a nest with eggs like her own. In his opinion "every individual cuckoo, as each individual bird, knows its own egg." ("Denn ich halte mit Bestimmtheit dafür, dass auch das einzelne Kuckucksindividuum wie jedes Vogelindividuum sein Ei *kennt*.") So far as she is successful, there might easily arise strains of cuckoos externally similar but differing in egg-type and in the nests in which they lay—if the female cuckoos were parthenogenetic! The perpetual crossing with males of other strains (since there is no reason to suppose that the mating would be selective in the same way) would, however, produce a continual supply of widely varying egg-types. Females laying eggs of these unusual types would have to hunt for appropriate nests and eggs, would often fail to find them, and would be obliged to lay in nests where the eggs were different, with corresponding risk of their destruction. We should therefore expect to find in any regional collection many instances of nests containing unlike eggs, their frequency depending on (1) the number of cuckoos of variant egg-type; (2) the tolerance of the foster parents; and (3) the thoroughness with which the region has been studied with this point in view.

These considerations cover pretty well the cases of resemblance and scattering non-resemblance. What of those of uniform non-resemblance—the cuckoo eggs in the nests of the wrens and *Phylloscopus* warblers? For the former Wachs offers a solution; for the latter, in which the number of instances is small, he thinks that data for an explanation are not yet at hand.

The critical point in case of the wrens is the character of their nests. Cuckoo eggs can not be laid in wrens' nests in the ordinary way, but must first be laid elsewhere and then be carried to the nest and be placed in it by the female. Since the body of the female cuts off the light when she puts her head into the opening of the nest, she has no chance to see the wren's eggs and to be influenced by their likeness or unlikeness—in a word she has to 'go it blind;' and the wrens for their part either do not notice the strangeness of the egg, or have difficulty in removing it, or are highly tolerant of it. It is also Wachs' belief that cuckoos tend to lay in the nests of the same species as that in which they have themselves been reared. In case of open nests this tendency must operate jointly with that to matching of the eggs. In the case of the cuckoos hatched in wrens' nests this latter factor does not come into play. They choose by nest-form only and thus more simply and easily. This would account for the relative frequency of cuckoo eggs in wrens' nests, as the inability of the female to see the wrens' eggs would account for the lack of resemblance.

Taking all the factors into account we seem to have in this matter of the resemblance or non-resemblance of cuckoo eggs, as Wachs well says, a most interesting biological experiment in process, and perhaps a case of the genesis of varieties taking place so rapidly as to bring the beginning and end within the observational lifetime of a single naturalist.

E. C. S.

Wilhelm Wundt Gedaechtnisheft

The *Deutsche Philosophische Gesellschaft* publishes, as Heft 3-4 of the second volume of *Beiträge zur Philosophie des deutschen Idealismus,* a *Würdigung* of Wilhelm Wundt. The contents of the Heft (80 pp.) are: F. Krueger, Wundt als deutscher Denker; P. Petersen, Die Stellung der Philosophie Wundts im 19. Jahrhundert; F. Sander, Wundts Prinzip der schöpferischen Synthese; A. Kirschmann, Wundt und die Relativität; H. Volkelt, Die Völkerpsychologie in Wundts Entwicklungsgang; O. Klemm, Zur Geschichte des Leipziger Psychologischen Instituts. The Heft may, apparently, be bought separately; no price is given.

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A CASE OF COLOR-BLINDNESS TO YELLOW AND TO BLUE[1]

By Florence Richardson-Robinson, University of Chicago

In November, 1909, a member of a class in elementary psychology in Drake University had difficulty in obtaining after-images. The instructor of the class, the writer of this report, tested him with the Holmgren wools. He matched the red skein normally and quickly, found more slowly a few matches for the green, but these were good, and matched the purple with the light reds and pinks. Upon being questioned he answered that the two skeins (the red and the purple) were of the same color; that one (the purple) was a "thin" red, and the other a "good" red; but that so far as mere color was concerned, any skein that would match the one would match the other. It was here evident that the admixture of blue in the purple skein did not to him affect the color. He was then given a skein approximating a standard blue. This he matched hesitatingly with light yellows, light greens, light browns, light greys and other blues of the lighter tints. A yellow skein was matched with the darker of the light greens, greys, green-blues and yellows. Being further questioned, he stated that the two skeins (the blue and the yellow) were alike except as to brightness, one (the blue) being much brighter than the other, but that neither had color. He suspected from the texture that they might be colored, but to him they appeared uncolored.

It was evident that this was, at least, an unusual case of color-vision and further investigation was carried on as faithfully as possible for the remainder of his college course—a period of somewhat more than two years—and through portions of the summer of 1911. Several weeks were spent at the University of Chicago and at the University of Nebraska, using such apparatus as was available at the psychological and physical laboratories of these institutions.

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[1] A preliminary report of the case was made at the meeting of the American Psychological Association at Minneapolis, Minn. December, 1910.

The writer will be glad to put any one interested into communication with the subject of the investigation.

Beginning June 15th, 1914, more than four and one-half years after the first observations on the case, the subject was given a brief series of tests paralleling those already carried out. The purpose of these was to determine whether there was any evidence of deterioration in his vision, as this might have an important bearing on the question whether the case is of pathological or congenital origin. His reactions in this series showed no sign whatever of any change for the worse in his color judgments, and in several tests his color discriminations were more accurate than they had been in the earlier experimentation. His eyes were in excellent condition, apparently in every respect better than when he was in school. During the past year he had been teaching chemistry and physics in a small High School. He reported that during the winter he had less trouble than usual in strong light, and that the glare of sunlight upon snow had not been annoying enough to necessitate the use of colored glasses.

A letter from him in 1916 indicated a suspicion on his part that he could not distinguish colors as readily as when he was in school. He complained that color now "wanders like a drunken night." He was sent the set of colored disks with instructions to sort out the colored, the uncolored, and the greys. He returned them with the assortment made as usual. In the summer of 1921 the writer was again in communication with him by letter, and he was then quite sure that his vision, both as to color as to acuity, was undergoing a change for the worse. Much of the color, he said, seemed to have gone out of the world. However, he chose the colored, uncolored, and grey disks practically as he did when in college. He, of course, made these selections entirely alone, and said that they were made on the basis of *color*. He mentioned also a grey film that sometimes came over the field of vision of his left eye.

The young man, who will be referred to hereafter as D, was about twenty-four years old at the time the case was first noticed. He was a member of the Drake University football team for the three last years of his college attendance. An ophthalmological examination[1] showed the retina to be normal, acuity normal, and no coloration of the lens. The color-blindness was apparently not pathological, at least so far as gross examination of the retinas would indicate.

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[1] This examination was made at different times in December, 1909, by Dr. W. W. Pearson of Des Moines.

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The hereditary features of the case are interesting. There is color-blindness in the immediately preceding generation on both sides of the house. The subject's father and mother were found (twenty years after marriage) to be second cousins. This seems to be good evidence that the color-blindness is congenital.

An exhaustive examination of this case presented a number of difficulties. D's retinas are apparently sensitive to slight differences in degree of brightness to which the normal eye is not sensitive. This very great susceptibility to slight differences means a most unusual sensitivity to changes in general illumination which makes the work exceedingly difficult to control. Color stimuli in which color was sensed in a certain degree of illumination lost color completely with a slightly increased illumination. On several occasions, when D was matching colors on a color-wheel in north light, the disks then appearing to him colorless, a cloud obscured the sun, and he exclaimed that there was color in the disks. The reverse of this happened several times.

The state of adaptation of his retinas, due to general light conditions or to immediate pre-exposure, was also a continual source of variation in the results. D's daily variation in general physical condition depending upon lack of sleep or anxiety, as well as upon fatigue of the eyes following study, or diversion, or prolonged exposure to out-of-door illumination, greatly affected his ability to see and to discriminate color. The fact, too, that fatigue of the retinas after a very few judgments greatly raised the threshold of color sensitivity made short periods and few consecutive trials a necessity. D, during the entire series of tests, was exceedingly generous in both time and patience, yet with school-work, athletics, and the necessity of financing his college course, could not give an unlimited amount of time.

The history of the case as written by D follows.

"The only case of color-blindness of any note that I find in my father's family is Father himself. The first time I learned of the fact was at a dinner party when Father lost his temper because one of the ladies told him there was a green stripe in his new suit. I learned from Mother afterward that it was always a sore spot with Father, and that if she wanted anything from a drygoods store she always sent a sample along.

"In my Mother's family there are a number of well defined cases. Of the 28 replies to inquiries that I sent out there were seven different cases spoken of. One, that of an uncle, was spoken of in nearly every letter, and it was a sort of a joke in that he was reported to have seldom been seen with a mated pair of socks. He seemed to have no idea concerning either shade or color, and this was especially so in the case of fruit. He was probably totally color blind. Another case was spoken of a number of times, a lady cousin unable to distinguish red and green. Another cousin wrote me that he could see colors if they were not too bright, but if they were very bright they all looked white. Another cousin wrote that he never knew he was color blind until he took a railroad examination, but he thinks he sees color as well as any one. Of the others I remember very little of what they had to say. Besides the seven mentioned there were a number of others that admitted having difficulty with colors, but seemed unwilling to discuss it. Of the seven, four were men.

"A very peculiar condition existed in the case of one of my uncles who was not color blind, but lost his sight entirely for about twelve years. He recovered it in his seventy-eighth year without medical aid.

"My Father's people are German; all large and strong, generally of light complexion with blue eyes. Mother's people are Swiss; small of stature, with dark hair and eyes. Both families, however, are long-lived and very healthy. Both families were members of the Penn colony, but migrated to Somerset County, Pa., before the Revolution. Numbers of them were killed in Indian wars.

"I was born in Iowa in February, 1886, the second of a family of four, an older sister and a brother and sister younger. Father was a farmer, as were three of his brothers also, one brother being a minister and the other a lawyer. There were also six boys and four girls in my Mother's family, four ministers, four farmers, and a dentist and a physician.

"I learned when quite young to consider color-blindness as a great defect, partly because Father often made unfortunate mistakes.

"The first difficulty that I can remember was to find out what people meant when they talked of the sky as blue. I remember when I thought that an apple must be red to be ripe, and I was perhaps six years old before I learned to tell by touch a ripe fruit, and even then I preferred it red. At home we raised three kinds of corn; red, yellow, and white. Red was red, white was smooth, and yellow was rough, and it was years before I could convince myself that a thing could be yellow, and not rough. White corn I could distinguish from yellow only by the shape of the ear, and the kernel. They looked alike in color. Questions often arose, but I dared not ask lest I expose my ignorance. I tried to solve the problem by remembering how a thing looked and felt that was spoken of as colored other than red, and by comparing texture and shade I experienced little difficulty under ordinary circumstances. The only difference between fresh cut grass and the bleached product was that the latter had lost its brilliancy. Standing grass was darker and had a surface that I associated with many other things people called green, though many of the things spoken of as green were to me nearly white. I could not understand why people spoke of a *golden* field of grain and a *green* hayfield, for they looked very much alike to me.

"Paints I soon learned to distinguish, and seldom if ever get them confused as no two have the same kind of surface unless highly polished. Even blue in paints sometimes has texture, though in other things it is usually so bright that I am unable to distinguish anything in its surface.

"At the age of fifteen I left the farm for a small academy and was obliged to work my own way. Railroading suggested itself to me as "easy money," and to prepare for the examination I procured pieces of colored glass commonly used for signal lights and signboards, and set about studying them. I found that the texture of the glass was not the same as that of paint or of cloth. So I procured spools of silk thread and a sample card of house paints, and studied them. I took the examination and passed with but one or two minor errors. I went to work thinking I could tell colors as well as any one, but the eighth day I bumped up against the colored lights alone, ran a switch engine into a box car, and concluded that it was no place for me.

"Later when I entered college I found a complete change of conditions and it was here that I encountered my first real difficulties. I was working my way, which was at times a severe tax on my system, and the first place to be attacked was my eyes, though only twice during my college work was my sight badly impaired for study, and that followed continued loss of sleep.

"I took up chemistry as a freshman science and it was not long until I wished that I had not; however, I had started it and had too much *Dutch* blood to quit. I soon found that there are other characteristics just as distinctive as color. There were colored salts, solutions, flames and precipitates. Flames were easy as no two things volatilize at the same temperatures and their texture is entirely different. Copper and barium are both green, but otherwise are not nearly so much alike as barium and strontium, the barium being very soft and the copper hard, almost flinty.

"Precipitates were not so bad unless in a colored solution as they all have a definite texture as well as a time for precipitation. Salts are not so important as far as color goes, but as for solutions I have never been able to find a way of telling their colors that was reliable, principally because they are transparent and one cannot see their surface.

"My plan was always where possible to note other characteristics than those of color, and as far as I know I have been just as successful in analytical chemistry as the rest of the class. I can see red in very small amounts (except in very strong light) better, I am sure, than most people, and green in dark shades in dull light. Blue is strong white with a glint to it, and yellow is usually a dull grey."

Tests With The Holmgren Wools

Further tests were made with the Holmgren wools,[3] which were spread upon a table with a dull black painted surface.

D's choices in this test were in the main *very* consistent, except in the case of a group of light tints about which he was always doubtful. The following record of one test is typical of the results:

101 test skein (green): selected skeins numbering 5, 11, 13, 15, 19, 43, 45, 47, 49, 51, 53, 55, 57, 59, 63, 67, 69, 97, 99. (These are all greens—see footnote—as are the doubtful skeins.) Doubtful: 1, 3, 7, 9, 17, 41, 61, 65.

Observations:

"Didn't notice green (in 101) at first glance; it came all at once in a flash. I could easily have been mistaken in it."

"These" (7, 53, and 3) "I first thought were green"; then he threw 53 into the pile of greens, and 7 and 3 into the heap of doubtfuls.

"This one (41) I'm dubious about"; then added, "if those others" (indicating doubtfuls) "are green this one is." "This probably isn't legitimate, but I'm picking these last ones out by elimination of the ones I'm sure aren't green"; then he tossed 11, 61, and 99 to the pile of greens.

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[3] The wools are tagged by number from 1 to 103 inclusive, and by the letters of the alphabet. The even numbers from 2 to 20, and from 32 to 68 (the designations are all inclusive), are *neutrals,*—*greys* and *browns*. The odd numbers from 1 to 19 and from 41 to 69 are *greens*. The odd numbers from 21 to 29 are *violets, purples* and *reds*, and from 31 to 39 are *reds* and *oranges*; the odd numbers from 71 to 93 are *pinks* and *reds*. The even numbers from 22 to 30 (except 28, which is violet) and from 70 to 78 are *blues*, and from 80 to 93 are violets and purples. 94, 96, 98, and 100 are dark grey, tan, tan and brown, respectively; 95 is dark red-brown, 97 is light green, and 99 is medium blue-green. 101, 102, and 103 are the green, purple and red test skeins. (Whipple speaks of 102 as rose.) Of those designated by letters of the alphabet, A to L are different tones of browns, as are N and P. R and Z are yellowish tints.

Questioned as to his method of choice, he remarked that if he placed the doubtful skein among light skeins he could see color then if ever. No. 61 was to him green among light skeins, and colorless among dark ones. Given a sheet of white paper he compared the appearance of some of the doubtful skeins against the white and the black background. All of them that were colorless on black were green on a white ground.

He chose 19 as the best green.

103 test skein (red): selected skeins numbering 21, 23, 25, 27, 29, 31, 33, 35, 71, 73, 75, 77, 79, 81, 83, 85, 86, 87, 89, 91, 93, 98, 105, X, and V.

Doubtful: 80, 88, 90, 92, "probably red." These are orange, pink, violet, purple and red.

Looked at 37 (deep orange) in different lights. Being questioned replied, "It's that bad one." (He has occasionally decided "it has red in it.") He said that 37 most nearly matches 80 (a *light* light violet) except that 80 has "lustre" and 37 is a "dull, dead surface."

92 (a dark violet) most nearly matches 37 in *color* (but not in brightness); it (37) "can't be matched in shade as it is darker than any of them."

When asked if he suspected any of the skeins of being yellow, he selected 37, O (a strong light yellow-brown), and 56 (a tan). He chose these, he said, "because of the mixture; yellow has some red texture to it, and also some green.[4] Red on green will darken a white; get one dulled down in shade and I'm suspicious that it might be yellow. I figure that if it weren't so dull it wouldn't be yellow. I can figure blues easier because of the brilliant surface. Two ways of getting blue: pick out the *brightest* in the bunch and call it blue, or hunt for a *mixture* that acts right." For blues, he selected 78 (dark blue), 39 (orange-yellow), and O (light blue-violet). 24 (a dark light-blue) and 69 (a dark yellow-green) might be either blue or yellow. 69 he sometimes selected as green and sometimes refused as green. He *always* studied it.

On several occasions D selected the whites, the greys and the browns. Brown he characterized as a mixture of red and grey, so mixed "that it isn't red any more." He selects browns rapidly. Here is a typical assortment: 10, 12, 32, 34, 36, 38, 48, 60, 66, 68, 94, 95, 100, A, B, C, D, P, and N. These are browns. His selection of greys, which he makes rapidly, is typified by this list: 3, 5, 8, 11, 15, 20, 42, 46, 56, 58, 62, 63, 64, 65, 67, 99, 101, E, F, G, K, and L. He later added 44, 54, 96 and S. These are browns and greens.

As he referred to certain "colorless" skeins in answering inquiries, he made this assortment of those he considered colorless. This was done rapidly: 4, 16, 18, 26, 30, 41, 70, 98, Q, R, T, W, X, Y, and Z. "Some of these are dirty whites, that look as if soap and water would help, especially these (16, 36, and Y)." (This was a *new* box of wools.) He often referred to the "dirty look" of yellowish skeins. The following collection remained after the above selections were made, and he could only characterize them as not white, nor grey, "just leftovers": 1, 2, 6, 7, 9, 14, 17, 22, 40, 43, 50, 52, 57, 61, 72, 97, H, and M. (These were light greens, blues, yellows and tans, but no light pinks, reds, nor violets.)

In short, the Holmgren test afforded him no difficulty with reds, except that the violets and purples were added as reds; he had difficulty with light greens, and with darker greens tinged with yellow or blue. Blues, yellows, and light tints—except reds—were confusing.

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[4] He will insist occasionally that a certain green under consideration has a "reddish cast."

Colored Papers

Given the assortment of Hering colored disks and asked to choose the colored ones, D always immediately selected the reds,[5] violet and purple. If the day were dull, or the illumination of the room reduced, his eyes in good condition, and his attention keen, he could be relied upon to choose the green, and often the blue-green and yellow-green. Very often he would hesitate over the orange. If questioned, he would reply that in "certain lights" he suspected it of having color. Asked *what* color, he would always answer, "Red, if any." He was apparently never inclined to think that it might be green.[6]

The yellow disk was never judged as having color, and the reddish yellow was sometimes said to have the *texture* of color but not color. If he were asked *what* color he would guess if he must guess, he always said red, as it was rather 'darkish' for a green. The yellow was to him a colorless, or a grey surface.

The green-blue and the blue were not only colorless, but *brilliant*. On several occasions he referred to the surface of the one (the greenish-blue) as "enamelled," and that of the other (the blue) as having "two coats" (of enamel). The (blue) surface had a high lustre to it, "little glints all through it," "a gleam that goes with what people call blue."[7]

After a period of over two years (1914), in which he had not seen Hering papers, the series was laid before him and he was asked if he remembered any of them especially. He picked up the orange and answered, "Indeed, I do. That's the worst surface I ever saw, and some day I should like to know what it is." Holding it at different angles, he commented on the fact that it had texture of color, and the shade of color, and occasionally a suspicion of red, "but it (the color) doesn't stay long enough for me to look at it."

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[5] The Hering papers were of the second series, purchased from C. H. Stoelting in the winter of 1908, and were named at that time as follows: red, nos. 1 and 2, orange (Gelb-Rot), reddish yellow (Rötlich Gelb), yellow, yellow-green (Gelb-Grün), green, blue-green (Blau-Grün), greenish blue (Grünlich Blau), blue, violet (Rötlich Blau), and purple (Blau-Rot). The red of the following equations is no. 1.

[6] In fact, in all the notes on his observations, there is but one record of his confusing red and green, and in that instance he judged a green coming into the periphery as "Colored, don't know what yet"; as it came in, he added, "It's green—no, it isn't, it is red!" This confusion was doubtless due to the negative after-image, and was probably not an instance of *color-confusion*.

[7] D says that some blue ink is hard to read: for instance, Sanford's Premium Writing Fluid before turning black is hardly visible on white paper. A blue pencil makes a mark scarcely different from that of white chalk. A blue ribbon on a type-writer gives the same effect on paper as the keys hitting the paper direct without a ribbon, and can be read only with the paper "on a slant." To try out one of these observations, the experimenter put before D a sheet of white paper upon which she had written a message with a blue pencil. D did not notice the message for some time, though he was using the paper for a background. When his attention was called to the note, he tilted the paper downward toward the window and read it easily.

He had no hesitancy at this time in choosing the colored disks, laying out the reds, yellow-green, green, blue-green, violet and purple, and putting aside the orange which he regarded with misgivings.

In 1921 he designated as reds, in each of eight trials on different days, the two reds and the violet; the purple as red seven times and grey once; the green as "probably green though light;" the yellow-green as "green at first" once, the other times grey; the orange as "some color—not grey," "colored?," "can't say," and five times as grey; the reddish-yellow as grey, nearly white, or white; the yellow as grey, or white; and the greenish-blue and the blue as white.

Spectrum Tests

Professor D. W. Morehouse of the Physics Laboratory of Drake University, in December, 1909, first tested D's ability to discriminate colors in the spectrum, using a sunlight source, and Rowland's grating ruled 14438 lines to the inch, mounted on a nine-inch telescope. The writer had asked for only a rough determination, so that the wave-lengths were not given. In this, as in subsequent examinations with the spectrum, the method of color-naming was used. Professor Morehouse made the following statement:

> "The infra edge of the red began at a very low point in the spectrum of the first order, where the violet of the second order overlaps the red of the first.
>
> "The red-yellow band, for him, was about midway between the C and B lines. This is very low indeed; the orange does not begin this low.
>
> "The spectrum was colorless from the D lines almost to the H lines where the red began again.
>
> "The brightest portion of the spectrum was in the green or at the b lines. It was not green for him, but very bright."[8]

The next attempts[9] at accurate determinations of the limits of the visible spectrum as seen by D were made with an ordinary laboratory spectroscope, using a 16 c.p. incandescent electric lamp as the source of light. The spectroscope was fitted up with a Wallace replica of Rowland's diffraction grating of 14438 lines to the inch. The right eye alone was used in these tests and it was light-adapted throughout. Under these conditions the subject did not observe the green band of the spectrum. The readings obtained, therefore, refer to the limits of the red band at the infra end of the spectrum, and those of the violet at the ultra end. The limits, also, afford the boundaries of the neutral band, which in this case extends from the inner margin of the red (somewhere in the vicinity of the D lines) to the inner margin of the violet (in the neighborhood of the G line.)

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[8] The apparent inconsistency between this and later determinations is due to the intensity of the beam used as a source. In such a case the green has always been obliterated, and the brightness determinations have been exceedingly variable, because, as D once remarked, "When it (the green-blue portion of the spectrum) is so bright, I can't tell *where* it is brightest. It is all glare, and it hurts!"

In no case, after this was noted, was it impossible to locate the green band when the slit was sufficiently narrowed.

[9] These were made by Mr. Seth Nicholson, then of the Physics Laboratory of Drake University.

The mean of five readings gave the limits of the red band as 7128 A — 5896 A[10]; the second red zone (in the violet), 4442 A — 4066 A. This, obviously, fixes the limits of the intermediate neutral band, under *these* conditions, at 5896 A — 4442 A.

The loss of the green band was again due to the brilliance of the beam used as a source. This corroborates the statement of the tendency to obliteration of green in strong illumination.

Through the kindness of Professor Henry G. Gale of the Department of Physics of the University of Chicago, and with his assistance, a very painstaking series of observations was made with the use of the Rowland concave grating there installed. This is a 6-inch grating of a 21-foot radius. Sunlight was the source of light, and the tests were carried on in a darkened room.

The limits obtained under these conditions were: red, 7620 A — 6050 A; neutral zone, 6050 A — 5600 A; green, 5600 A — 4920 A; neutral, 4920 A — 4575 A; red band at violet end, 4575 A — 3933 A.

Several interesting features presented themselves during the course of the tests. Excerpts from the notes, as taken, are appended in order to call attention, briefly, to these various points in their appropriate settings. According to the usual procedure, the slit was set at certain known lines, and the observations taken:

6563 A — "good red."
6868 A — "good red."
7620 A — "still a good red, but not so much of it."
7500 A — "trace of red."
7400 A — "seems red" at times and at times "colorless."
5890 A — } (D lines) no color.
5896 A — }
6050 A — little red scattered in the white, but it goes at once.
5580 A — trace of green. (Have to narrow slit to get color. With wider slit, get a colorless backgroud and green lines: with narrower slit get green background and relatively black lines.)

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[10] The spectrum readings are given throughout in Angström units.

5600 A — corrected limit for green; judged one side as slightly greenish, the other side as white light. Cut down slit at magnesium lines, 5175 A, called lines green but background nearly white.
4920 A — possible trace of green.
4900 A — no color.
4565 A — "little smear of thin red."
4535 A — "still red."
4560 A — does not see it.
4575 A — lines reddish on colorless ground; background becomes red when slit is narrowed.
3968 A — "red."
3933 A — red *lines*, but no color in background.

The following observations were made with a mercury arc spectrum:

5790 A — and 5769 A; light grey.
5460 A — white.
4916 A — white.
4358 A — white, same shade as the second one before this (5460 A), but very brilliant.
4078 A and 4047 A — both red lines.

In the arc light spectrum (made with a spectroscope constructed in Ryerson Laboratory) the lines at 3850 A were examined. These for D were red. Immediately after this test, Professor Gale remarked, "He sees violet better than the normal eye; he has an excellent ultra-violet eye." The brightest point in the entire spectrum (with the carbon arc) was at 4300 A. It was white and most brilliant. This line for the normal eye is in the blue band. In the potassium spectrum D saw red unmistakably at 7699 A, and it could not be determined how much farther out he might perceive red.

The limits of the visible spectrum are given by Baly as from about 7600 to 3600 Angström units,[11] and by Helmholtz[12] as from 7604 A to 3968 A. So it is evident that there is at least no *shortening* of the spectrum. On the contrary there is some evidence that there is an extension of the spectrum at both the red and the violet ends.

The limits of the two neutral bands are given only on the basis of readings taken with the normal grating-spectrum, rather than upon the basis of readings taken with the prism spectrum. These limits of the neutral band, given as the region *between* the limits of the colored bands, are based upon Professor Gale's reading with the sunlight grating-spectrum. The first neutral band

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[11] Baly, Spectroscopy, 30.

[12] Helmholtz, Physiol. Optik, quoted by Parsons, Color Vision, 28.

(normally yellow) extends from 6050 A to 5600 A; the second (normally blue) extends from 4920 A to 4575 A.[13]

In order to observe the consistency in D's color judgments, a more extensive set of readings of the limits of the colored bands was carried on in the Physics Laboratory of the University of Nebraska, using the Feuss monochromator set up with a Pellin and Broca prism, 3° dispersion. By means of a calibrated cylinder, the wave lengths may be read direct. Four different sources of light were used: sunlight, diffuse north daylight, a Nernst filament, and an arc lamp. The readings given below are for the right eye. The left eye was given the same number and kind of tests, but the only differences in the sensitivity of the eyes were irregular and less than the variations due to changes in illumination and fatigue, so that no constant noticeable difference in the sensitivity of the two eyes was observed.

These readings from the prism spectrum are not so reliable as those from the normal spectrum on account of the dispersion of the prism, which contracts the width of the colored bands through the middle of the spectrum. Therefore, their greatest value as indicated above lies in the fact that, owing to the number of readings obtained, they afford opportunity to estimate the degree of uniformity in D's judgments at different portions of the spectrum. Any other deductions from the readings are merely corroborative of phases otherwhere noted.

The following tables give the readings taken with the monochromator using the different sources of light as indicated.

TABLE I. READINGS OF RED BAND

NERNST FILAMENT	SUNLIGHT	DIFFUSE DAYLIGHT	ARC LAMP
7750 A – 5900 A	7550 A – 5900 A	7100 A – 5900 A	7750 A – 5890 A
7750 A – 5900 A	7750 A – 5900 A	7170 A – 5920 A	7750 A – 5900 A
7750 A – 5890 A	7750 A – 5920 A	7630 A – 5900 A	7750 A – 5900 A
7750 A – 5890 A	7310 A – 5940 A	7750 A – 5920 A	7750 A – 5910 A
7750 A – 5900 A	7470 A – 5920 A	7750 A – 5910 A	7750 A – 5890 A
		7750 A – 5900 A	
		7750 A – 5900 A	
		7690 A – 5930 A	
		7470 A – 5900 A	
		7750 A – 5890 A	

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[13] The averages of 70 people with normal color vision (Burch) give the extent of the color bands as:

red = 760.6 to 551 mm.
green = 593 to 484 mm.
blue = 517 to 443 mm.
violet = 454 to 397.5 mm.

pure red = 625 mm.
pure green = 525 mm.
pure blue = 471 mm.
pure violet = 415 mm.

Quoted by Parsons, *ibid.*, 114-115.

TABLE II. READINGS OF GREEN BAND

NERNST FILAMENT	SUNLIGHT	DIFFUSE DAYLIGHT	ARC LAMP
5760 A – 4900 A	5700 A – 4960 A	5690 A – 4800 A	5660 A – 4820 A
5750 A – 4890 A	5740 A – 4980 A	5810 A – 4900 A	5600 A – 4810 A
5630 A – 4870 A	5760 A – 4920 A	5770 A – 4840 A	5620 A – 4810 A
5600 A – 4860 A	5750 A – 4860 A	5770 A – 4860 A	5690 A – 4870 A
5620 A – 4880 A	5700 A – 4860 A	5790 A – 4940 A	5720 A – 4840 A
		5750 A – 4890 A	
		5760 A – 4890 A	
		5780 A – 4950 A	
		5730 A – 4880 A	
		5690 A – 4860 A	

TABLE III. READINGS OF "SECOND RED BAND" (Violet)

NERNST FILAMENT	SUNLIGHT	DIFFUSE DAYLIGHT	ARC LAMP
4680 A – 3920 A	4400 A – 4000 A	4750 A – 4090 A	4600 A – 3880 A
4770 A – 4050 A	4750 A – 4000 A	4780 A – 4050 A	4650 A – 3910 A
4650 A – 4150 A	4600 A – 4000 A	4780 A – 4050 A	4620 A – 3940 A
4640 A – 4140 A	4580 A – 4070 A	4800 A – 3900 A	4640 A – 3930 A
4660 A – 4160 A	4610 A – 4050 A	4770 A – 4000 A	4650 A – 3900 A
		4740 A – 4000 A	
		4750 A – 3960 A	
		4790 A – 3880 A	
		4720 A – 4120 A	
		4720 A – 4100 A	

The consideration of the readings reveals only what might be expected, when one recalls the amount of dispersion in prism spectra. The yellow, green, and blue bands are disproportionately narrowed, and the violet band broadened. Therefore there is much less chance for variation in the middle, and a likelihood of a comparatively high variation in the extremes. This is precisely what is found to occur in the case of D's color judgments. There is a high variation in the lower edge of the red band, and particularly in both edges of the violet, or "second red band." In the case of the lower red limit taken from the spectra of the Nernst filament and of the arc lamp, there is no variation because the readings were limited by the capacity of the apparatus when D was observing a spectrum of a relatively low degree of intensity.

These low and consistent variations are a witness to the integrity and care which characterized D's work. There is no suspicion of ambiguity in D's judgments of the spectral bands. The spectrum for him is not shortened—in fact it may be slightly extended. The red band, beginning low in the spectrum, continues until it begins to merge into the yellow near the sodium lines; its limits seem to be, with prismatic spectra, under *these* conditions, about 7560 A and 5910 A with the best red at about 6670 A. The yellow band is to him colorless and of only medium intensity, and extends from about 5900 A to about 5700 A. The green band is visible in low but not in high intensity, owing

TABLE IV

Showing Readings of Bands of Grating Spectrum, and *Range* of Readings of Different Prismatic Spectrum Bands

SOURCE	RED	NEUTRAL	GREEN	NEUTRAL	RED
Sunlight (Rowland Grating)	7620A—6050A	6050A—5600A	5600A—4920A	4920A—4565A	4565A—3933A
Sunlight (Prism)	{7750A 7310A}—{5940A 5920A}	{5940A 5920A}—{5760A 5700A}	{5760A 5700A}—{4960A 4860A}	{4960A 4860A}—{4750A 4400A}	{4750A 4400A}—{4000A 4070A}
Nernst (Prism)	7750A —{5900A 5890A}	{5900A 5890A}—{5760A 5600A}	{5760A 5600A}—{4900A 4860A}	{4900A 4860A}—{4770A 4640A}	{4770A 4640A}—{4160A 3920A}
Diffuse daylight (Prism)	{7750A 7100A}—{5930A 5890A}	{5930A 5890A}—{5810A 5690A}	{5810A 5690A}—{4950A 4800A}	{4950A 4800A}—{4800A 4720A}	{4800A 4720A}—{4120A 3880A}
Arc Lamp (Prism)	7750A —{5910A 5890A}	{5910A 5890A}—{5720A 5600A}	{5720A 5600A}—{4870A 4810A}	{4870A 4810A}—{4650A 4600A}	{4650A 4600A}—{3940A 3880A}

partly, it seems, to the obliterating effect of brightness in the green itself. Its limits are roughly 5700 A and 4880 A, with the best green at between 5000 A and 5100 A. The second neutral band (in the blue) extends from about 4880 A to 4680 A with prismatic spectra. The second red band, or the "thin red band," runs between about 4650 A and 4010 A with the prism. This is in the violet. The blue band is of maximal brightness and no color, with the maximal luminosity at 4650 A to 4680 A. The judgment of the point of maximal brightness, like the judgment of the best green band, was an unwelcome proceeding for D on account of the painful glare of these regions, and he had not a great deal of confidence in their accuracy. The violet band is perceived as a "thin red," of the same hue as the red band, but unsaturated. This "thin red" band is often spoken of as having somewhat of the *lustre* that he notices in surfaces that to the normal eye are bluish.[14]

Color Mixtures

The color equations with colored papers are of value in a qualitative way, but are too variable to have much reliance placed upon their quantitative phase. This work suffers as does all work in color-vision investigations done with pigments. The fading of the color, even after slight use, the change in the surface due to handling, and the occasional differences in tint in new papers were all noticed and criticized by D. The variation in the amount of general illumination was an almost constant source of error. An attempt to work only at 10 o'clock in the morning on clear days was not entirely successful as a control of the illumination feature, on account of the varying density of the city atmosphere, and threatened to render the experimentation interminable. Reliable *quantitative* work must be done where the illumination can be controlled and kept constant to a chosen standard. Certainly the equations derived from work done in the shifting light of ordinary days are not to be made the basis for accurate deductions.

A rough attempt to control the amount of illumination was made by raising or lowering the curtain of the large window just behind D as he sat facing the color-mixer. Even so, the variation due to the temporary fatigue of his retinas, and to a lowered vitality, was greater than that caused by the light. On several occasions work was discontinued for several weeks while he recuperated. D insists that his color sensitivity furnishes the best indication of his general health.

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[14] In 1916 arrangements were being made for further investigations at the Nela Research Laboratory, but D was unable to go to Cleveland. It may yet be possible to obtain spectrum data for a luminosity curve and for spectral color mixtures and matches.

It will be remembered that, of the set of Hering papers, D always judged the reds, violet and purple to be varying "shades" of red. The green he often reported as colored, and sometimes the blue-green and the yellow-green (v. p. 00). On several occasions he has spoken of the orange as having the texture of color, red if anything, but probably no color. This leaves the yellow, the greenish blue, and the blue as uniformly uncolored.

An alternating current motor, with a maximal speed of 5000 revolutions per minute, was used as the color mixer. In the color matching, a large uncut disk, 20 cm. in diameter, was used as a background, against which were placed the several smaller disks 11 cm. in diameter, cut to allow changes in proportions. These changes were made in such a manner that D was ignorant of their character; he did not observe the disks until the motor was in full operation.

The discussions of the equations will be taken up in order.

(1) *Red.* D strongly opposed any attempt to match red with any disk or combination of disks not containing red, and could not be deceived in such an attempt. An increment of 12° of white, or 5° of black in the inner red disk was noticed by him as a difference in the amount of red in the inner, as compared with the outer disk.

While no combination could be made to match 360° of red, a mixture of red with black or white or both could be matched with other mixtures containing another color with a reddish component; as for instance,

360° carmine = 105° blue + 255° red
360° reddish yellow = 65° white + 267° black + 28° red
360° violet = 58° white + 225° black + 77° red

(2) *Orange.* D had a particular aversion for this paper. It annoyed him because he could not characterize it adequately. Except in dim light it ordinarily had no appreciable color, but when he examined it closely he suspected red. *When his eyes were tired* he did not suspect color, and if, as he said, he was not "watching for color" *he would judge a black and white mixture to match it.* On one occasion he described it as looking "just like a broken steel surface"—"uneven"—"once heard it called a 'hard blue-grey'."

In ten such trials, in which he accepted a black and white mixture, the average proportions were black, 198.8°, and white, 161.2°, when the amount of white was decreased from 360°, and 220° of black and 140° of white when the black was decreased from 360°. The average of the twenty equations is

360° orange = 209° black + 151° white.

The orange disk can also be matched with red, violet, or orange, and black or white. Notice this list:

360° orange = 265° reddish yellow + 95° black
= 285° violet + 75° black
= 100° blue + 260° black
= 342° yellow + 18° white

The following matches were correct in shade, but not in color: notice the greater amounts of black needed in the last two as the greens approach the blue.

360° orange = 160° yellow-green + 200° black
= 170° green + 190° black
= 160° blue-green + 200° black
= 140° green-blue + 220° black

(3) *Reddish yellow.* Reddish yellow also appeared colored under the most favorable conditions. D says that "the color is not evenly mixed;" "it looks a little as though it might have been mixed red and white, and again as though it might have been black and white *unmixed*."[15] At another time he said that he would infer color from the surface, for "it doesn't stay flat. If I have to make a guess at it, I'll call it a blue."

D insisted that reddish yellow and black would not mix to make grey, that "it mixes in black and white chunks, and doesn't make an even surface." He noticed a change in surface when 15° white, or when 38° black were added to the reddish yellow.

The following equations were satisfactory for "shade," but not for brightness *or texture*, and were made when he was not regarding the *color*, either on account of fatigue, or of rather strong illumination.

360° reddish yellow = 126° black + 234° white
= 105° black + 255° yellow
= 44° black + 316° yellow-green
= 65° black + 295° blue-green
= 112° black + 248° greenish blue

No match could be made with green. The contrast considerably accentuated the green of the inner disk, so that on one occasion, when he did not at first notice color in the green, he remarked when he saw it against the reddish yellow, "*That's* a good green, where did you get it?" Reddish yellow and black mixed almost invariably raised in D a suspicion that red had been added. The following mixtures contain the red that he demanded.

360° reddish yellow = 65° white + 267° black + 28° red
= 35° white + 25° black + 225° red + 75° yellow
= 49° white + 85° black + 117° red + 109° blue
118° reddish yellow + 242° black = 270° black + 90° violet
As a mixture of black and blue has a "reddish cast"
360° reddish yellow = 307° black + 53° blue

(4) *Yellow.* The yellow disk will exactly match a mixture of black and blue as to shade, but never as to brightness, as the yellow is duller even than a mixture of black with blue. A black and white mixture will exactly match a yellow, so well that D once remarked, "That's a well mixed pair of greys—we don't often get them better." An addition of a small amount of black to yellow gives a texture as of a colored surface; amounts upward of 15° of black have several times had this effect. D characterized this surface as brown; when asked if there could be color in it, he remarked that if there were it was so well mixed that it was "not evident *as color*."

Such equations as these were obtained:

360° yellow = 310° white + 50° black
= 181° black + 179° blue
= 135° white + 225° blue-green
= 121° white + 239° yellow-green
= 28° black + 332° greenish blue

In no case did he attempt to match a mixture containing red with a yellow. As yellow is darker than blue, 181° of black were necessary to dull the brilliancy of the blue to the shade of the yellow.

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[15] On one occasion when D was being closely questioned regarding his statement of an *uneven* or *not well mixed color* he replied: "The color isn't smoothly mixed, it is on in chunks. You know how black and white granite looks when you look at it closely: can't you imagine how it looks when you are just too far away to see the *pieces* of black and of white, but not so far away that you see it as all one grey?—That is how these *unmixed* colors and greys look."

(5) *Yellow-green.* In naming the colors of the disks, which he did only upon request, D sometimes called yellow-green a dull blue. An added increment of black resulted in a change to a color texture, and in a certain amount gave a fairly good green.

46° yellow-green+314° black="fair green."

Yellow-green mixed with blue-green, even in illumination that cancelled color in each, resulted in green. The best proportions for green that D could get with this mixture were

138° yellow-green+222° blue-green.

Any attempt to add red to the mixture on the inner disk was detected as soon as the mixture began, for the experimenter, to take on a red tinge, and sometimes before. Often the yellow-green induced a red in a neutral mixture.

The following are representative mixtures:

360° yellow-green=260° yellow +100° black
=142° blue-green+218° white
=125° black +235° white

In shade, but not in color,

360° yellow-green= 80° white+280° reddish yellow
=126° white+234° blue-green
= 52° black+308° green
= 40° black+320° greenish blue
=262° black+ 98° blue
=132° black+228 yellow

(6) *Green.* The green of these papers was, according to D, too light in shade to be a good green. Often under ordinary illumination he did not notice the color. A slight decrease in illumination brought out the color very noticeably. The addition of black also brought out the green, when without the black no color was observed.

The best green for him was:

36° green+324° black.

D often spoke of "a flash of green" or "flashes of green." These 'flashings' are characteristic of a surface on which the color was about at his green-limen. They are so brief that often he could not be sure what the color was, for he sometimes, though rarely, observed red in "flashes." *It was under these circumstances that he insisted that the state of his attention was a much more important factor than he felt the investigator seemed to believe. If he were expecting color he said that he watched for the flashes; if he was not watching for them he did not attend to them, therefore he did not see color in a surface that had color for him when his attention was keen.* He was very emphatic on this point, and his comments on these faint green surfaces seemed always to corroborate his emphasis.

On bright days

360° green=164° white +156° black
=180° blue +180° black
=332° blue-green + 28° black
=308° yellow-green+ 52° black
=198° yellow +162° black

He did not attempt to match green in color to any reddish mixture. In the case of each of the group of greens there was a strong tendency to induce red and thereby render futile the effort to match these greens with a black and white mixture.

224° green+136° red=53° white+307° black.

(7) *Blue-green.* These papers, D said, have the texture of color, and sometimes the colored flashes came as he first looked at the paper, and did not come again. In this connection he spoke of "colored flashes" but not of

"green flashes." When questioned, he said that he would suspect it of green on account of the 'shade' being lighter than a red surface.

360° blue-green = 172° black + 188° white
= 120° black + 150° white + 90° yellow
= 18° black + 108° white + 180° yellow + 54° blue
= 116° black + 174° white + 70° blue

The equation could be maintained by the addition of yellow, or blue, or both. In the last equation a red tinge was noticed, either a contrast from the blue-green background which he has obtained at other times or the effect of the mixture of black with blue which has been spoken of before.

Also:
360° blue-green = 185° black + 175° blue
= 105° black + 255° greenish blue
= 75° black + 285° green
= 84° black + 276° yellow-green
= 113° black + 247° yellow

Blue as usual required the most black to reduce it to the tone of blue-green.

Disregarding color, but matching in "shade"
360° blue-green = 68° white + 292° violet
= 105° white + 255° orange

The orange needed the more white and the violet the less to be reduced to the "shade" of blue-green.

(8) *Greenish blue.* This paper afforded a match for mixtures of black and white, blue-green and white, green and white, and yellow and white in the following proportions:
360° greenish blue = 314° white + 46° black
= 294° white + 66° blue-green
= 123° white + 237° green
= 83° white + 277° yellow

In "shade," but not in color, the following equations were good:
360° greenish blue = 90° white + 270° reddish yellow
= 130° white + 230° violet

(9) *Blue.* The standard blue "sparkled" in a way unlike any other disk of the series. This disk was *very* fatiguing, and D found work with it unpleasant on this account. The surface was smooth and white, in fact whiter than a somewhat soiled white disk, as was shown by the fact that, in 5 out of 10 trials, the white could not be made light enough to equal the "shade" of the blue. He once remarked, "How that centre white does shine; it is terribly hard on the eyes." Asked for synonyms for his use of "shine," he discussed the terms *sparkle* and *glint*, but here preferred "shine."

On this account few equations could be made that were even fairly satisfactory to D, and none that were entirely so. The blue, of course, could be mixed with any colorless disk to get the shade of the uncolored disk, but none could be mixed with white to match blue. For example:
360° reddish yellow = 18° blue + 342° black
360° yellow = 64° blue + 296° black
360° yellow-green = 260° blue + 100° black

As the mixture of black and blue had a reddish tinge, these matches were only in shade. His statement that such a mixture had red in it is borne out by the fact that it would induce green on a contiguous grey surface. Possibly this red was due to the decrease in illumination which may change the hue as well as the intensity of the surface, hue being a function of wave-length *and* intensity.

(10) *Violet.* D described the violet as "Looks like grey with some red in it. It is very light." He often referred to it as "the very light red." It could not be matched in color with black and white, for red must be added; also it induced green in a colorless surface, which added to the difficulty of obtaining color equations.

360° violet = 225° black + 58° white + 77° red
= 36° black + 214° red + 110° blue
= 206° white + 154° red
= 84° black + 276° blue
= 72° white + 288° reddish yellow

230° violet + 130° white = 360° purple
270° violet + 90° black = 185° red + 175° yellow
285° violet + 75° black = 360° reddish yellow

Violet, then, could be matched in color with any mixture in which a small amount of red was apparent, but could not be matched in color with any mixture in which green was apparent.

(11) *Purple.* D expressed difficulty in color matches for purple. It had a "tone" to it which necessitated the mixture of some neutral, and this of course was detected as a greyish tendency in the surface.

360° of purple could be matched in color by a mixture of white or black with red, orange, or violet; or by a mixture of black and blue which resulted in a reddish surface; and could be matched in "shade" by a mixture of white and yellow-green, yellow, or blue-green. D asserted that, in the case of "thin reds" and greens, he could disregard the slight amount of color and match the shade or brightness only.

The following is a representative series of matches for purple:

360° purple = 90° white + 270° orange
= 130° white + 230° violet
= 54° black + 306° blue
= 44° black + 12° white + 304° red
= 105° blue + 255° red
326° purple + 16° black + 18° white = 101° blue + 259° red

The following three equations refer to matches in shade, but not in color.

360° purple = 18° white + 342° yellow
= 28° white + 332° yellow-green
= 50° white + 310° blue-green

Summary. These results show that red, purple, and violet can be matched in color with one another, *plus* black or white or both, but not in color with yellow, yellow-green, green, blue-green, greenish blue, or blue; that green, yellow-green, and blue-green can be matched in color with one another when mixed with black or white or both, but not in color with red, purple or violet; that yellow *plus* white can be matched in color with certain proportions of blue *plus* black, but not with any green or any red.

The ability to detect small amounts of red is apparent, and the lack of ability to observe small amounts of green under certain conditions of saturation and illumination is quite as apparent. Yellow is equivalent to light grey, and blue is equivalent to white; neither has color, and the blue is "brighter" than any white available.

After-Images

D had never observed positive after-images, and was not able to obtain them under experimental conditions. A light of low intensity did not seem to produce them, and the fixation of a stronger light was to him exceedingly unpleasant, and rendered his eyes incapable of further use for a considerable length of time. In the fixation of even a low c.p. incandescent lamp he did not see the glowing wire, but only a mass of light. After a few trials, the attempt was abandoned.

For obtaining negative after-images, disks of colored papers of the Hering series were used. These disks were 2 cm. in diameter, and were unmounted.

The first attempts were made in daylight in a north-exposure room. A white background was employed for both the fixation of the stimulus and the development of the after-image. The disks were given in irregular order. The following are typical results of such a test:

AFTER-IMAGES. TEST IN LIGHT ROOM

White background both for development and fixation. Diffuse north daylight, cloudy day; 10 a. m. D "feeling fine". Stimuli, Hering disks taken in irregular order. The reds, violet, and purple are judged to be "colored."

STIMULUS	AFTER-IMAGE
Red (no. 2)	Barely bright spot
Red (no. 1)	Nearly white
Orange	*Very* light grey
Reddish yellow	White spot
Yellow	Medium grey
Yellow-green	Light grey with thin red
Green	Thin red
Blue-green	Thin red
Greenish blue	Light grey
Blue	Grey
Violet	Light grey
Purple	Very light grey

It is apparent that the only colored after-images he has perceived are those from the green stimuli, the color of which he has not perceived. His failure to obtain colored after-images from a red stimulus is obvious. Assuming that here, as was so pronounced in the spectrum tests, the strong illumination, accentuating the brightness of the stimulus, obliterated the color in the after-image, a change was made in the conditions of the test. A dark room was used,[16] the background for fixation and development being illuminated by single incandescent electric lights of different candle-power values. The comparison of these results shows mainly the effect of the decreased illumination of the stimulus, of the fixation background, and of the development background, although to what extent these differences are a result of partial dark-adaptation of the retinas is not known, but should be determined.

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[16] An attempt was made to obtain data on the Purkinje phenomenon, but this was abandoned. It was found, however, that D possessed very keen twilight vision, as he was able to discriminate small objects in the darkroom when the experimenter could not, with dark-adapted eyes, see a large sheet of white paper passed back and forth immediately before her eyes. D insisted that he had always been able to see his way about at night even when comrades had collided with objects in what they had termed "pitch darkness."

After-images. Test in Dark Room

White backgrounds. 2 c.p. light in opaque metal reflector hung 9 inches above horizontal background, so as not to throw light into reagent's eyes. The red, green, violet, and purple under this illumination have color. D "feels fine."

STIMULUS	AFTER-IMAGE
Red	Dark grey
Orange	Same as the first
Reddish yellow	"Brilliant light grey"
Yellow	Shade darker
Yellow-green	"Nice pink one, different surface from any other"
Green	Very faint red
Blue-green	Light brown
Greenish blue	Very bright grey
Blue	Bright light grey
Violet	Light grey, dull
Purple	Medium light grey, fairly bright

Here as before he reported red after-images from green disks which he perceived as colorless, and reported *no* colored after-image for a red disk that represented to him almost a maximum of color. Yet the green disk, to him colorless under high illumination, was perceived as colored under low illumination. With the same illumination, but a grey background for both fixation and development, he suspected a tinge of green in the after-image from a red stimulus but "wouldn't swear to it."

With 4 c.p., and white backgrounds, the results were practically the same as above and so are not quoted, but with 4 c.p., and grey backgrounds, he developed for the first time a green after-image. The stimulus was the purple disk.

When a 2 c.p. was substituted for the 4 c.p. the result was the same except that the green was a lighter green. When the two reds were fixated simultaneously side by side and developed under the same conditions (grey backgrounds, 4 c.p. illumination) the hue of the green after-images was the same, but the after-image of the red (no. 2) was brighter and lighter than that of the red (no. 1).

With 16 c.p. illumination, and white backgrounds there was no color in the after-images of the reds, and only one green (the standard green) gave a colored after-image, whereas under the previous light conditions the yellow-green, the green, and the blue-green produced colored after-images.[17] The higher illumination, it seemed, annulled color in the after-images. In this illumination either fixation or development on grey of a red stimulus gave only a "suspicion of presence of green on account of the texture; it gives the surface of green but not the color." The stimulus fixated on white and the after-image on grey revealed no color. Black backgrounds used with grey and with white interchangeably for fixation or for development produced no color results.

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[17] Illuminations of 32 and 64 c.p. were objectionable to D and were, therefore, not used.

Even though a white background is most effective for rendering a low degree of green visible, it is not most effective as a fixation or development background for the production of green in the after-image. Nor is any use of a black or a white background so conducive as is a grey.

At the time of the latest test (1914) D perceived a very feeble green in the after-image of an orange stimulus fixated and developed on white under the ordinary indirect illumination of a dark cloudy day. On the same occasion and using the same stimulus when grey was used as the background, he remarked, "The darkest green ever in an after-image." He reported a "good green" on black, and none at all whenever the stimulus was fixated upon white, though it was only when a green stimulus was against white that its color was perceived.

The comparison of brightnesses in the after-images offers another opportunity for getting at the matter of relative brightness of different bands in the color series. For this purpose paired comparisons were made. Two disks of different colors were placed tangent to each other upon the white fixation sheet, and D was asked to compare the brightness of the after-image of the two. This was done in diffuse daylight.

AFTER-IMAGES COMPARED FOR BRIGHTNESS AND COLOR

Notes assembled from work in daylight on various days. Stimuli referred to by number in pair as indicated.

STIMULI		AFTER-IMAGES
Red (no. 1)	Red (no. 2)	About alike
Red (no. 1)	Orange	1 is darker
Red (no. 1)	Purple	2 darker
Orange	Reddish yellow	1 lighter, "stimulus of 1 was darker, though."
Reddish yellow	Yellow	Hard to judge; 1 *seems* darker
Yellow-green	Green	No difference in brightness, 2 redder, 1 lighter
Yellow-green	Blue-green	1 lighter
Green	Greenish blue	1 redder than 2, the 2 slightly lighter; close match
Green	Blue-green	1 redder, 2 thin red
Blue-green	Greenish blue	Alike in brightness, but 1 has red
Greenish blue	Blue	2 brighter
Blue	Yellow	2 lighter than 1, 1 dull
Blue	Reddish yellow	1 brighter but no lighter
Blue	Reddish yellow	Same shade but 2 has "glint"
Violet	Purple	No difference

Not all possible combinations were made, but those tried out are consistent with other remarks concerning the relative brightness of the series of papers and of colors in general.

The after-image of blue was duller than that of yellow (the stimulus blue, he said, was "bright," and yellow "dull"). The after-image of reddish yellow had the "glint" of a blue surface.

This test could have been carried farther very profitably, but time was of consequence.

Simultaneous Contrast

Only incidental observations were made regarding contrasts, but these were frequent and pronounced. In working with the color mixer, a green often induced a red on the contiguous grey surface, and this at times when the green was not perceived as green. Red more rarely induced a green, but this happened several times. This color induction often made it impossible to match a subliminal green with a black and white mixture, on account of the presence of the induced red.

D judged the series of Hering papers (excepting the purple) put up in squares of varying sizes—0.5 cm., 1 cm., and 2 cm.,—on a black, a light grey, and a white background at a distance of 18 inches from the eye. The reds and the violet he judged in each case to be red, and the green on white and on light grey he judged to be green, but the green on black was to him in every case colorless.

After-images in the Peripheral Retina

The possibility of peripheral after-images had not been mentioned to him. One day while the peripheral field was under investigation he remarked that these observations were difficult to make because at times the "spots" seemed to act so unaccountably—to come and go abruptly, with flashes of color and changing texture—changes so rapid that they were hard to keep track of. He asked if after-images could be obtained so far out. Receiving an equivocal answer he began speculating and observing, and decided that they were after-images, and that his previous reports had probably been "no good." On account of the lack of time, of apparatus, and of a well controlled method of experimentation, no attempt was made to investigate the conditions of these peripheral phenomena.

Indirect Vision

In December, 1910, an attempt was made to ascertain the facts regarding D's retinal field. A Hering campimeter was used, and a rough survey gave evidence of fields for both red and green, the green being much smaller. There was no point on the retina at which blue or yellow was judged as color. The field for red extended irregularly from about 30° to 60° out from the fovea, and the green *very* irregularly from about 5° to 35° out, the short axis being on the left *retinal* meridian of each eye. The really interesting facts were the discovery of three color islands, two in the left and one in the right, farther out in the periphery than his so-called limits for color. These islands were small areas sensitive to red and to green *entirely surrounded* by an area insensitive to color.

A few readings with a Skeel Perimeter (given in the Skeel terminology) under dubious illumination and background control revealed the following facts (June, 1914).

D's left eye showed a normal field for white on the nasal retinal area, but a large blind area developed *within* the field as an *island*; it began on about the 150° axis, and extended from 32° to 50° out; on the 155°, it extended from 22° to 66°; on the 170°, from 20° out to 86°; on the 210° from 15° to 86°; on the 220° from 20° to 86°; on the 240° from 28° to 60°; and on the 270° from 35° to 42°. This blind spot extended roughly 70° in its widest portion along an axis, and from axes 150° to 270°.

His right eye showed a blind spot on the lower axis (at 270°) of about 10° in diameter. The sittings were discontinued before the entire retina could be thoroughly plotted.

Red, coming in from the periphery was judged to be white; going out, it "gets *thinner* in color just before it goes," and occasionally "goes out in a flash of white." It "darkens as it comes in." Green went out and came in as white. His field for red, left eye, showed blind spots practically coextensive with that of the white except that there seemed to be several widely scattered color islands within this large area, where islands for brightness had not been discovered. This must have been due to his greater attentiveness where he perceived the stimulus as colored, as he felt that he had to "watch colors closer." The field for green had the same blind spots and islands as the red. These should have been thoroughly mapped out, but D was in town for only a short time and we could not work long at a sitting.

Summary of Facts

It is evident, from these results, that this case of color-blindness presents unusual characteristics. The question of the name of the class to which it should be assigned is not nearly so important as the determination of the facts regarding the case.

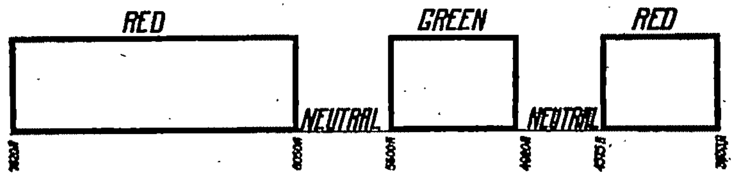
In the test with the Holmgren wools the subject matched without hesitation the red skein with other skeins, all reds. The green skein he matched with greens, but with only a few of the available greens, and with skeins of no other color. He was slow in matching the blue skein and selected as matches many of the very light tints, the light blue-greens, greys, green-blues, and yellows. The yellow skein was matched with the same range of tints, but darker in shade. His replies to questions as to the basis upon which he made the matches were direct and consistent. "Red and green are colors and may be matched as colors; those two other skeins are uncolored and must be matched on the basis of brightness and texture." In the Nagel test he picked out correctly the all-red-spots card as well as cards with some red spots; he also picked out correctly cards with some green spots but failed to find the card with only green spots. He could easily distinguish between the yellow-green and blue-green of the card A-6, but not on the basis of color. (According to the Nagel tests he would be classed as one of "die sehr seltenen Fälle von Blaugelbblindheit".) Work with the Hering color mixer (transmitted light) was futile; he found the four colored glasses to be red, green, grey (yellow) and white (blue). No matches could be made to satisfy him.

In color matching with Hering papers, he referred always to the red, violet, and purple papers as reds, though the experimenter was always exceedingly careful to use no color names. He did not often refer to the "green" of the paper, but when he did, he used the term correctly. He usually adopted the experimenter's terminology of "this" or "that" in referring to other papers, though he occasionally spoke of them as uncolored.

In the examinations with different spectra he judged both the red and the violet ends to be red, even distinguishing the red from the violet end by referring to it as the 'good red' as contrasted with the 'thin red' end. He was insistent that any band in the thin red could be matched with some band in the good red end in color though not in brightness, as the thin red possessed a brilliance peculiar to itself. When he referred to color in the green band, he was consistently correct in both his color judgment and his color naming. The blue region he referred to always as white, and the yellow band sometimes as white, though usually as having no color.

A careful examination of his color sensitivity made with a Rowland 6-inch grating of a 21 foot radius with sunlight, gave the following limits for the different bands: red, 7620 A — 6050 A; neutral, 6050 A — 5600 A; green, 5600 A — 4920 A; neutral, 4920 A — 4575 A; red at violet end 4575 A — 3933 A. The brightest point in the spectrum with the carbon arc was about 4300 A. It was white and most brilliant. This point is in the blue. With the arc light spectrum, he saw as red the lines at 3850 A, which for the ordinary eye are not visible, and his description of the region was correct. In the potassium spectrum he saw as red the lines at 7699 A. Other observations support these determinations.

The spectrum diagram indicating the colored and neutral bands is based on these readings (v.p. 165) as these were most painstakingly obtained with the best conditions and apparatus. These readings are therefore the basis upon which his spectrum color judgments should be discussed, except as indicated on p. 156.



The green band could be observed only by cutting down the intensity of the source of light.[18] With sunlight spectra he located the brightest point twice at 4650 A and once at 4680 A. (These are in the blue.) The best green was judged with a prismatic spectrum once with a diffuse light at 5090 A and once with sunlight at 5010 A. (Professor Gale thinks the best green, for himself, is at the magnesium lines, C and C2, at 5184 A and 5174 A respectively.) Often, when near the limits of the green bands, D complained of an alternation of green and red at the same point. The possibility of after-images had not then occurred to him, and he was disturbed.

When working out color equations with Hering papers it was found that the reds, violet, and purple can be used with black or white or both in matches in color with one another, but not with yellow, yellow-green, green, blue-green, greenish blue, or blue. Green, blue-green and yellow-green can be matched in color with one another when mixed with black or white or both, but not with the reds, violet or purple. Yellow mixed with white can be matched in color with a mixture of blue and black, but not with any green or any red. He is sensitive to small amounts of red in a mixture, but not to small amounts of green, except when mixed with black. The question as to whether D sees red and green as the presumably 'normal' eye sees them cannot, of course, be determined. But it is no more of an issue here than in comparing the color vision of any two people.

After-images were colored, uncolored, or grey. Those following the fixation of a green were red or uncolored, depending upon the brightness conditions. They were never called red when they did not follow the fixation of a green, though often the green in the stimulus was subliminal. Under low illumination he perceived green in an after-image following a fixation of a purple or a red disk. After-images resulting from the fixation of a reddish yellow disk had no color but unusual brilliance (that of a blue). Those from other colors were different tones of grey.

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[18] The narrowing of the green band is dependent upon the intensity of the beam and is not a condition necessarily peculiar to a colorblind eye. Professor Gale tells me that the discrepancies in the width of the yellow band shown in colored spectrum plates in various textbooks is apparently due to the intensity of the beam used as a source of the spectrum from which the copy is made. With a beam of sunlight direct upon the slit, the yellow band is *very* broad, reaching even into the blue. With a narrowed slit and indirect light the yellow band becomes narrow. Considering D's sensitiveness to brightness and its effect of obliterating color, it might easily be that his apparently diminished sensitivity to green is instead sensitivity to brightness, much in excess of that of the ordinary eye.

When a green mixture on the color-wheel was against a grey background, red was often induced in the grey. This happened both when the green was perceived and when it was subliminal. His insistence that the mixture of black and blue contained a small amount of red is verified by the fact that it induced green on a contiguous grey. In working with a prism spectroscope, he often commented on the fact that the slit was green, with red on the edges of the aperture. On one occasion, when observing a "good green" band with a sunlight spectrum he remarked, "The sun went under and left the disk red. Why was that?"

The Frauenhofer lines, however, were often spoken of as colored, against a colorless background, when the intensity of the band obliterated the color. For instance, with a green band of strong intensity, he would remark: "There is no color in the slit, but the lines are green." In nearly all the observations the color was noted at the lines before being noted in the background.

The extent of his peripheral visual field, though not very thoroughly examined, showed a much more restricted area for green than for red. Yellow and blue were judged always as grey and white and therefore there were for these, as colors, no fields. He frequently obtained red after-images from green stimuli in the indirect field in regions in which he was insensitive to green. These after-images were strong enough to obliterate the color of the stimulus, and the rapid fluctuation of colors caused him both confusion and annoyance. Several scotomata were found.

There were several interesting 'color islands' in his peripheral field, that is, small regions sensitive to red, and affording green after-images for red, and sensitive to green, which were entirely surrounded by areas insensitive to color.

D had developed several effective secondary criteria in judging colors. These were texture, flatness or smoothness of surface, shade, brilliance or dullness, and mixture. Colored or uncolored surfaces had a texture that grey surfaces lacked. (This texture had nothing to do with textiles, as paints had it.) Uncolored surfaces were not *well-mixed* and *flat* as were grey surfaces. Orange was a particularly "unmixed" surface. A mixture containing red was darker in *shade* than green when both were subliminal, and this shade was sometimes the basis for distinguishing green. Blue was *brilliant* in surface—had a *glint* to it—while yellow was *dull*. Purple and violet were both described as being "thin reds" with the *lustre* that characterizes some blue surfaces. With the help of these secondary criteria he concealed his color-blindness from his friends and even from his students when he was teaching physics in a small High School. The experimenter had as much difficulty in understanding what these criteria meant to him as he had in understanding what blue and yellow were, as colors, to her.

The family history shows color-blindness on both sides, and the father and mother are distantly related. There is every reason to believe the case to be one of congenital color-blindness. D believes that his color discrimination is undergoing a deterioration, though he has chosen colors in the Hering series in 1914, 1916, and 1921 as he did in 1910. He says also that he cannot now (1921) use his eyes for study for so long a period as he did in his student days, and he is sure that the acuity of the left eye, particularly, is diminishing. There are, then, apparently degenerative phases of the case. But it is probable that the categories, degenerative and congenital, are not so distinct as in our literature, at least, they are assumed to be.

Conclusions

This case of color-blindness presents the following features:

1. No shortening of spectrum.
2. Sensitivity to red.
   Weak sensitivity to green.
   No sensitivity to yellow or blue *as color*.
3. Brightest band in spectrum in the blue.
   Blue-green next in brightness.
4. Brightness obliterates color as shown (1) in spectrum tests, and (2) in after-images.
5. Brightness discrimination keen.
6. Achromatic zone of vision normal in size.
   Red zone slightly constricted.
   Green zone much constricted.
   Both brightness and color-blind areas with 'islands' in peripheral field. Several scotomata.
7. Effective secondary criteria in judging color.
8. Congenital basis and possibly degeneration.

SOME PROBLEMS OF MEANING

By Raymond Holder Wheeler, University of Oregon

The purpose of this paper is to discuss the following problems of meaning from both the theoretical and experimental points of view. (1) Are the contents of meaning exclusively sensory and imaginal? (2) Is meaning structure, function, or both? (3) What is the relation between meaning and kinaesthesis? (4) What are the attributes of meaning? (5) What is the relation between meaning and the so-called mental elements? (6) What are the criteria of meaning? (7) A theory of meaning.

The first problem need not detain us long; for experimental literature overwhelmingly supports the view that meanings are exclusively sensory and imaginal in their content.[1] The author recently acted as subject in a prolonged introspective study of meaning,[2] typical introspections from which furnish added indications that such apparently indescribable processes as Kakise's "feelings of meaning," imageless thought, Moore's "pure meaning," etc., are kinaestheses which the reagent has failed to analyse.

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[1] Pyle, W. H., An experimental study of expectation, this Journal, 20, 1909, 530-569; Okabe, T., An experimental study of belief, *ibid.*, 21, 1910, 563-596; Book, W. F., On the genesis and development of conscious attitudes, *Psychol. Rev.*, 17, 1910, 381-398; Jacobson, E., On meaning and understanding, this Journal, 22, 1911, 553-577; Kakise, H., A preliminary experimental study of the conscious concomitants of understanding, *ibid.*, 22, 1911, 14-64; Chapin, M. W. and Washburn, M. F., A study of the images representing the concept "meaning," *ibid.*, 23, 1912, 109-114; Woods, E. L., An experimental analysis of the process of recognizing, *ibid.*, 26, 1915, 313-387; Geissler, L. R., Analysis of consciousness under negative instructions, *ibid.*, 23, 1912, 183-213; Clarke, H. M., Conscious attitudes, *ibid.*, 22, 1911, 214-249; Fisher, S. C., The process of generalizing abstraction and its product, the concept, *Psychol. Monog.*, 21, 1916, 213 pp.; Fernberger, S. W., An introspective analysis of the process of comparing, *Psychol. Monog.*, 26, 1919, 161 pp.; Gleason, J. M., An experimental study of feelings of relation, this Journal, 30, 1919, 1-26; Wheeler, R. H., An experimental investigation of the process of choosing, *Uni. of Ore. Publs.*, No. 2, 1920, 59 pp.; Crosland, H. R., A qualitative analysis of the process of forgetting, *Psychol. Monog.*, 29, 1921, 159 pp.

[2] This Journal, 33, 1922, 223-233. Compare the introspection on the meaning "knife," p. 224, with Moore on a similar meaning, *Psychol. Monog.*, 27, 1919, 162.

See also *Psychol. Rev.*, 29, 1922, p. 425. Here it was found that when the reagent assumes an unanalytical attitude toward his experiences, meanings appear as complexes of content whose sensory and imaginal ingredients do not rise to the focus of attention; but in the presence of an analytical attitude these same meanings invariably stand out in their detail of exclusive sensory and imaginal content.

The results of these investigations demonstrate that "sensationalism" so-called is able to meet the issue fairly and squarely and to show a real identity between the content of "thought" and the structural elements which it recognizes.[3] "Thought" recognizes its structural elements in such fashion as is illustrated by "interpretative periods" in the act of choosing and in the development of meaning.[4] A recent effort to demonstrate the contrary can hardly be accepted as valid evidence.[5]

An extended series of experiments on synaesthesia with check data clearly supplements the evidence so far brought forward against imageless and sensationless meanings.[6] It seems a waste of time, then, to pursue our illustrations farther.

Our second problem deals with meaning as structure *versus* meaning as function. Jacobson[7] found that meanings of words varied as did their contextual associates, and that where such associations were absent so were the meanings. In our studies on synaesthesia this fact was clearly evident. Our synaesthetic reagent perceived and understood as did our asynaesthetic reagent, but his mental contents were radically different. The content of meaning-contexts varied in our two reagents, but the function involved, *i.e.*, the behavior of these contents, was identical. Further, where in our synaesthetic reagent the behavior of contents within different contexts varied, meanings varied.

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[3] Demanded by Moore, *op. cit.*, 224.

[4] Wheeler, *Uni. of Ore. Publs.*, 1, no. 2, 33-35, 51. This JOURNAL, 33, 1922, 231f.

[5] Moore, *op. cit.*, and McDonough, A. R., The development of meaning, *Psychol. Monog.*, 27, 1919, 443-515.

The following is an introspection from one of McDonough's observers. "I do not know how to analyse this consciousness of knowing but I know the meaning. It is something of the same order as the recognition of the word as familiar. There seemed to be a judgment in both cases . . . . although I was not at the time conscious of judging" (477). All of Moore's and McDonough's introspections are of this type. Processes of knowing, recognizing, judging, realizing, waiting, imagining, defining and the like are mentioned over and over again with such dearth of analysis and vagueness of expression that we are forced to doubt very seriously whether their reagents ever knew what they were experiencing when they reported meanings.

If higher standards of introspective technique than these are not used in favor of imageless thinking and meaning, why waste time with experimentation at all? Indeed the psychologist need not go to the trouble of employing supposedly trained introspectors in order to find ample alleged evidence for pure meanings. Any untrained but intelligent person can tell us that he thought so and so, realized this, judged that, etc. Would the trained histologist conclude that his own observations were histological dreams because his untrained friend failed to see all that he himself could see? Cf. Moore, *op. cit.*, 223.

[6] Wheeler, R. H., The synaesthesia of a blind subject, *Uni. of Ore. Publs.*, 1, no. 5, 1920, 61 pp.; Wheeler, R. H. and Cutsforth, T. D., The synaesthesia of a blind subject with comparative data from an asynaesthetic blind subject, *Uni. of Ore. Publs.*, 1, no. 10, 1922, 104 pp.; Wheeler, R. H. and Cutsforth, T. D., The rôle of synaesthesia in learning, *Jour. Exper. Psychol.*, 4, 1921, 448-468; Wheeler, R. H. and Cutsforth, T. D., Synaesthesia a form of perception, *Psychol. Rev.*, 1922, 29, 212-220; Wheeler, R. H. and Cutsforth, T. D., Synaesthesia and meaning, this JOURNAL, 33, 1922, 361-384.

[7] *Op. cit.*, 564, 565.

Clark[8] pointed out that functional rather than structural stages differentiated familiar, generic and unfamiliar images. Meanings, then, were not demonstrable in structure and became meanings only when they behaved in certain fashion with respect to associated imagery or sensory processes.

Our own findings are in direct accord with Clark's and Jacobson's, in that no mental process turned out to be meaningful in the absence of context and shifting contents. In our synaesthetic reagent meanings developed by means of shifts, enlargements and complications of colors, together with developing synaesthetic visual imagery of kinaesthetic sensation. Unless mental contents of sensation and image developed from antecedent contents before the latter had entirely vanished from consciousness, meaning invariably failed to develop. Everywhere a detached or dissociated process meant nothing. Conversely, a given mental content contributed to meaning only when it led to a subsequent content or to a motor response. In fact a motor response seemed quite as essential to the development of meaning as did imagery and sensation in context. After all, this is what one would expect if meaning accrues to the behavior rather than to the content-aspect of experience; for of what value in mental life is the process-aspect[9] of experience unless it should be motor in character, leading to a motor adjustment of the organism?

In our view, then, meaning, as structure, is sensation and image or it is context; as function, it is the behavior of sensation and image or it is motor response. No treatment of meaning is complete if confined either to structure or to function alone. Structures imply concomitant functions and *vice versa*. In evidence of this we need only point to Moore's statements in spite of the fact that he insists meanings are structures only. After defining meaning as a structure[10] he writes, "all meanings as conscious events" etc.[11] He admits after all that meanings are functions; for an event is something which takes place; it is action of some content, some structure or some object. We read further that "meanings have the power of calling forth other meanings."[12] Need it be pointed out that meaning, structurally considered, does not call forth other meanings;

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[8] Visual Imagery and Attention, this JOURNAL, 27, 1916, 466, 491.

[9] Cf. Crosland, *op. cit.*, 118, 119, 133.

[10] *Op. cit.*, 185.

[11] *Op. cit.*, 189.

[12] *Op. cit.*, 190.

that something "static" never did anything? A structure is by definition inert, inactive and arbitrarily separated from everything else. Obviously then it is meaning as function or action which calls forth other meanings.

Again we read: "meanings are capable of being in the focus point of consciousness."[13] But as structures how did they get there? Before a content can be conceived as entering the focus of consciousness it must be regarded as changing or moving in some way; it arrives in focal consciousness either by having been placed there bodily by some external agent or else it "flows" or "processes" in. In the latter case meaning, of course, is dynamic or functional as well as being static or structural.

We have said that meaning is both structure and function. A corollary of this view is found in the evident fact that what is function on a given level of complexity of phenomena becomes structure on a higher level of complexity. For example, the contents of a task-consciousness, to choose, may be exclusively visual imagery, vocal-motor imagery and bodily kinaestheses. With respect to itself such a task-consciousness is not content; rather it is function; for it consists in the fashion by which sensations and images operate in the individual's behavior. The rôles of such processes, the work they perform, make the complex a task-consciousness. Further, the task-consciousness cannot be construed as content in this situation because, upon examination, the content is seen to vary from individual to individual and in the same individual from time to time. The content is not uniform while the function is uniform. On the other hand, with respect to something more complex, namely the entire act of choosing, the task-consciousness, now definable as a phenomenon of meaning, becomes a content. It is a content of the volitional consciousness. Likewise volition, as such, is not content but function; but with respect to a prolonged and more complex period of mental life it is content, along with reasoning, judging, etc.

The same consideration holds for the *Bewusstheit*, the *Bewusstseinslage*, the feeling of relation, the feeling of activity, consciousness of self,—any meaning. Abstracted from its setting along with equally complex processes, no one of these phenomena can be regarded as a structure. They are all functions or modes of behavior whose structures are exclusively sensation and image. Regarded less analytically, however, they become unanalyzed structures when referred to a more complex mental process of which they are the parts.

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[13] *Op. cit.*, 190.

A second corollary also follows from the view that meanings are both structure and function. Meaning and image or sensation are equally important in consciousness. We are hardly falling into a logical dilemma, as Pillsbury thought,[14] by positing two ultimates in consciousness, meaning and image (or sensation) with no possible interrelation between the two. When meaning and image exist together, the latter is the content of the former; the behaving image is meaning. Evidently structure and function are two ultimates; they are mutually exclusive but dependent factors, by definition opposite, thus defining one another. An essential relation does exist between these two factors in that one is inconceivable and has no meaning without the other; one implies the other. Thus, rather than falling into a theoretical dilemma, we are obeying an inevitable law of logic, namely, that nothing functional is conceivable without its structural aspect and *vice versa*. Function, let us say, is the thing itself and structure is the "form" in which the thing exists. In other words, structure is the mode of taking place of function; it is means, while function is the "end." [15]

Our third problem deals with the relation of meaning to kinaesthesis. One need not point to the numerous attempts in psychological literature to explain a problem too simply by reducing a great many varied phenomena to a common element. Nevertheless, the relation of kinaesthetic processes to meaning has received belated recognition. We are adjusted continuously to one stimulus or another; incipient strains and delicate movements are always present; in fact kinaesthesis is so common in mental life, its localization so wide-spread, its complexes and variations so numerous, and its changes so delicate, that an introspective description of almost any process whatever is incomplete until incipient changes in tonus and delicate movements are taken into account. Until it is generally observed that kinaesthesis is always with us and that it functions in all mental activities, we are violating a scientific law dealing with the control of all processes associated with the object of experimentation. Indeed, ignoring kinaesthesis in a majority of introspective studies leaves it open to question whether the very essence of the desired process is overlooked.

In our view kinaesthesis is an essential component of the consciousness of meaning and the mechanism of the development of meaning is motor response.[16] That quality of experience which marks a given mental process as meaningful is unquestionably the quality of kinaesthetic sensation. Consciousness of meaning is a species of recognizing; it may involve either an incipient or a well-developed feeling of familiarity; attitudes of acceptance are frequent constituents of consciousness of meaning. Indeed no matter what the variety of that consciousness may be, its structural core is kinaesthetic sensation or image and its development—its essentially flowing character—depends upon the circumstance that the individual is responding to a stimulus motor-fashion. The stimulus is an antecedent complex of sensation and image. Meaning, then, is response to one's mental processes. A physiological stimulus causes a mental process, sensation and image; the latter is then response; the mental process then becomes a stimulus for further response; and the latter is meaning.

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[14] Pillsbury, W. B., Meaning and image, *Psychol. Rev.*, 15, 1908, 150–158.

[15] Cf. Bode, B., *Psychol. Rev.*, 15, 1908, 255-264 and Woodbridge, F., *ibid.*, 15, 1908, 397-398, on the problem of meaning. No doubt their difficulty disappears when the concepts of structure and function are adequately applied to the problem of meaning.

One need not look for a structural differentia of meanings in different patterns of kinaesthetic sensation, for he will find, no doubt, that kinaesthesis, as such, will not distinguish one meaning from another. Meanings are distinguished, labelled and defined by their setting and not by their content. For example, a visual image of a knife may be meaningful in virtue of kinaesthetic accompaniments but the meaning, knife, is not a datum of consciousness until the image is explicitly and concretely labelled either vocal-motor fashion or by such a process as visualizing a knife in use.

On the other hand almost inconceivably slight shiftings in muscular tonus about the face, or in the throat, constitute changes from the consciousness of one meaning to that of another, given corresponding changes in the mental processes to which the motor reactions are responses. When mental processes are syncopated, such delicate changes constitute the development of new meanings.

The fourth problem dealt with the attributes of meaning. Attributes of mental processes in general are divisible for practical purposes into two groups according to the structural and functional points of view. Let us confine structural attributes to the field of content or quality. These we will call substantive attributes. Color, brightness, tone, olfactory and gustatory qualities and the like would fall into this group. Such qualities possess certain attributive aspects—certainly not attributes on a par with quality—such as extent and possibly duration. On the other hand, mental processes possess functional attributes or attributes of behavior, a better name for which might be *modal* attributes for the reason that these attributes are really modes of behavior. In this group fall such factors as generality and concreteness of meaning, and no doubt clearness and complexity of mental processes.

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[16] In the view of the writer such concepts as determining tendency, perseverative tendency, and like attempts to envisage the dynamic would be far more serviceable to psychology were they expressed in the behavioristic terminology of stimulus and response. Functional psychology is superficial and relatively meaningless until interpreted in terms of reflex mechanisms and motor reactions.

Moore makes generality an attribute of structure. Further he insists that only pure meanings possess this attribute; that sensation and image do not.[17] We point out again that Moore's introspections were so faulty, so lacking in analytical detail which many other investigations unquestionably demonstrate, that they can hardly be accepted as serious competitors of Fisher's work.[18] Fisher, it will be remembered, found that generality consisted of the fashion-in-which contents behaved in consciousness. That is, generality was a mode of behavior of sensory and imaginal processes, describable in terms of function but not in terms of structure. In other words, generality turned out to be a modal rather than a substantive attribute. Obviously the same thing can be said of concreteness of meaning.

A second attribute of meaning is said, by Moore, to be "analysableness into simpler meanings." [19] All that this means, of course, is that meanings are complex, something which we have known for a long time. Moreover, could anything be more of a tautology than to assert that meanings are analysable into simpler meanings? Would a large dose of medicine be analysable into smaller doses if it were not medicine first and last? All analysis is the act of resolving complex meanings into simpler meanings. One does not analyse anything in mental life without analysing meanings. It is only meaningless experience which cannot be analysed. Moore seems to have discovered a very obvious fact.

We can hardly accept Moore's statement that "consciousness of relation" is an attribute of meaning.[20] No better place in which to observe consciousness of relation could be found than in acts of comparing. Accordingly, if we turn to Fernberger's[21] study of judgment, we note that consciousness of relation consists exclusively of sensory and imaginal contents. The "reference" to something which a relational consciousness always implies is a momentary shift of attention to some sensory or imaginal datum appearing at first in the fringe of consciousness and possibly becoming focal. In other words, the relational consciousness consists of more than one mental content; a change in intensity, quality or in some other attribute of sensation and image takes place; the reagent responds to this change by employing shifts, visual, verbal or other imagery. The meaning of the relation is not explicit until interpreted by a subsequent mental state. Moore's third attribute of meaning, then, accrues to behaving sensory and imaginal contents and not to attributes of structure.

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[17] *Op. cit.*, 198, 201.

[18] *Op. cit.*

[19] *Op. cit.*, 191.

[20] *Op. cit.*, 191.

[21] *Op. cit.*

Another attribute of meaning, according to Moore, is interpretativeness.[22] One is involved in function no matter how he defines interpretativeness; for if by it is meant a capacity or a potentiality, one refers to a latent function; and if by interpretativeness is meant that meanings interpret, action is again implied by definition. "Images do not interpret," Moore insists,[23] "they remain what they are . . . . images and not meanings." What then is the function of imagery in mental life? We let Moore answer this question; for he goes on to assert that images are illustrative of meaning after the meaning has taken place.[24] Surely, one illustrates a point for the purpose of clarifying the meaning; illustrations are concrete examples of the meaning. Why insist that an illustration is something beyond or aside from the meaning which it illustrates, when it is really a part of that meaning? Contrary to Moore's assertion, then, the very function of imagery is that of interpretation.

We now turn to the fifth point, the relation of meaning to the problem of the mental element; and we posit the following considerations. (1) There are no irreducible and *sui generis* mental contents, not excepting sensation and image as data of consciousness; (2) the simplest possible mental content derives its existence from the functioning of a companion-process. In other words, the simplest existing mental content is an analysable meaning, a dual and a synaesthetic process. (3) Unanalysability of structure only means that we have reached a point of introspective analysis beyond which no further contents can be segregated by attention. The simplest possible act of attending takes place in the presence of two contents, one from which attention may shift and one toward which it may shift. Further, added effort of attention results in a necessary shift to processes other than the object of analysis.

Our reasons for positing these considerations are as follows. First, in synaesthesia the primary sensation exists only in connection with a stereotyped associated image. The simplest possible existing mental content in the consciousness of a synaesthetic reagent is a dual process involving a shift of attention from a sensory to an imaginal component. In the absence of the image-component the reagent has no means of knowing that the sensory component exists in consciousness other than in the form of an unrecognizable "plus" or "something." From a functional standpoint, however, synaesthesia is a normal process; it is an act either of perceiving or of cognizing. One half of the synaesthetic process defines or attributes meaning to the other half, making it possible for either one to become a datum of consciousness.

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[22] *Op. cit.*, 191.

[23] *Op. cit.*, 216.

[24] *Op. cit.*, 216.

Secondly, when the synaesthetic reagent is instructed to attend solely to the sensory component and to ignore the imaginal component one of two things invariably happens: either he finds that he is unable to fulfill the instructions, or his attention becomes absorbed in strains of effort and the sensory component disappears altogether. In either case the result is negative as far as isolation of the primary process is concerned.

Thirdly, there are certain individuals—and the writer is one—who find that in their own consciousness sensation and image never stand alone. The writer has never, to his knowledge, been able to find a pure red (not excepting spectral lights) having in it neither observable yellow or blue; green always has either yellow or blue in it; blue has in it either red or green; yellow is likewise never pure, for it always has in it some observable red or green as well as white; every white has some black, and every black has some white, and so on. Similarly, cutaneous sensations are never "pure"; the writer never experiences a pressure sensation, for example, in the absence of a certain amount of visual imagery and kinaesthesis. No amount of practice entirely eliminates them. There is always a visual or a verbal tendency associated with organic and kinaesthetic sensation. Likewise, olfactory and gustatory sensations are equally as complex. The important fact about all of this seems to be that any so-called "sensation" is inconceivable, unimaginable, and unrecognizable in the absence of the so-called secondary or companion-process. In connection with one quality there invariably appears another and different quality by means of which the first is perceived and by means of which the former becomes a datum of consciousness, a conscious content. Any so-called "simple" of experience turns out to be a complex.

Fourthly, the "flowing" nature, the process-aspect, of consciousness precludes the possibility of irreducible and *sui generis* processes of any kind. One experience, no matter how simple, is continuous with some other process, and the latter is at the same time alike and different in some way from the former; else the change or continuum would be inconceivable on the one hand and unrecognizable on the other. Thus the changing or durative aspect of a content is unobservable until there are two contents in consciousness, constituting a dual process and at the same time a "mental element."

Fifthly, before one can become aware of a simple content, a separate content is essential to provide the two terms of the subject-object (stimulus-response) relation. This additional consciousness is not consciousness as an entity, but it is another content, different in quality from the first, as a result of whose presence there follows an implicit process of judging. On the other hand, unless these two contents constituted the same, original and practically simultaneous experience, there would be no way in which a third act of consciousness (an interpretation) could identify either part of the original experience. All of this means, of course, that mental processes are not "given" or "existential." Indeed, if given, to what are they given? A better concept, by far, seems to be that the simplest datum of consciousness is not a "state" or a "content" which is "given" but is a function, a meaning, whose ingredients are observable only when they exist in pairs or in larger complexes. Under these circumstances the members of the complex derive their qualities from one another and from a subsequent interpretative period. The so-called "given" comes into existence by growth, by function; it is action involving a shift of attention from something to something; experience, then, is not given but creative; not passive but active.

Sixthly, if the simplest possible experience consisted of but one content, quality or attribute, by definition we have made that experience absolute; we have compartmentalized it and have precluded the possibility of ever becoming conscious of it. We have broken the continuum.

Seventhly, the position occupied by psychology in the sciences suggests that the psychologist's element is a complex rather than a simple. The element or basic structure of mathematics is the simple, absolute and fixed number, 1. By "1" is not meant any number smaller or larger than "1" no matter how slight. The unit is fixed and thus the science is exact. In physics, structures are more complex and the units or elements are less fixed as the facts of relativity show. By the time we reach chemistry more complex elements are added to the rapidly growing list of "simples" (units of time, space, vibrations, ions, etc.); and from the chemical elements are derived by synthesis a vast array of complex structures or substances running into unknown complexity in the organic compounds like the proteins. Hence from a structural standpoint chemistry not only becomes more complex than the sciences upon which it is based but this complexity is increasing in geometrical ratio.

The simplest strictly biological element, the cell, represents a vastly higher order of complexity of structure; and yet the increasing complexity advances, still, with accelerated speed, as these cells are found to be combined into anatomical systems and tissues whose functions constitute the subject-matter of physiology and hygiene. Obviously, the variability of biological phenomena is derived primarily from the complex and variable nature of the basic biological unit, the cell.

Should we not expect, then, upon reaching psychology, that our strictly psychological unit or element—the sensation or image—and the element of behavior—the reaction—should be even more complex in proportion? New syntheses have evidently been built upon the structural and functional phenomena of biology. These syntheses we call consciousness and behavior.

We find that the biological, the chemical and perhaps the physical "elements" are analysable, although less and less so the nearer mathematics one approaches. Should we not expect to find our psychological element analysable even by our own methods of introspection, especially since complexity has been increasing with accelerated speed through the hierarchy of sciences? Obviously, the element of psychology is an analysable complex, a variable thing, whose natural variability and complexity are responsible for the difficulty of predictability in the science and for the so-called lack of exactness. Exactness will not rise above its source! With a variable and inherently "inexact" unit to begin with, added exactness cannot be found.

Seventhly, the nature of analysis itself would lead us to conclude that we should find nothing irreducible in consciousness. For example, what do we mean by unanalysability except to portray the idea that as yet no technique has been found sufficiently elaborate or delicate to tear apart a given phenomenon? Thus an element means nothing more than something which resists analysis; there is no excuse for calling it an irreducible or *sui generis* thing. "Element" should never mean something "ultimate".

So, in psychology, we regard a process as irreducible when it cannot be "torn apart" by means of attention-behavior. But when we have thus reached the limit, we fail to take into account what is happening to the act of attending, which, if the content had been reducible, would have resulted in the separation of the complex into its ingredients. Attention is not static and cannot be directed to an "irreducible" and there remain fixed! As in case of the sensory component of a synaesthetic complex, an "irreducible" is always a "step behind" attention. Evidently what happens is this: when we have reached a process so simple that under the circumstances it cannot be separated into further ingredients, the effort of analysis continues in the form of further shifts; but not being able to shift over a "simple" which has no details, it must shift elsewhere. Hence the companion-process; and hence the so-called element as an analysable complex. [25]

Eighthly, the so-called attributes of sensation, namely, duration, intensity, extensity and clearness, do not seem to be irreducible features of experience. The trained synaesthetic reagent finds that the sensory component of the synaesthetic complex possesses no attributes. Its intensity, duration, extensity and clearness are data of consciousness only as meanings accruing to the associated image and its behavior. Extension, in the writer's experience, is in part actual or imaged movement; clearness is not an attribute of sensation or image but a matter of complexity of qualitative detail (cognitive clearness) or a matter of visualizing some process in focal or blurred vision (attributive clearness); duration is in part muscular strain and is never a simple attribute.[26]

If such a position is true it means that many investigations of sensation, perception and affection have led us to a misinterpretation of the facts or have failed to give us a true picture of the situation.[27] The reason for this may not be hard to find. Reagents are trained to ignore the so-called "secondary" processes in experiments on sensation and perception. In such a training, which requires several months in some instances, the intention is to teach the reagent to detect minor differences in sensory attributes; in actuality, however, there is evidently built up a mental "set" or bias toward meaning. As a result of the training he receives the reagent becomes less rather than more analytical toward the object of his introspection; he is prevented from noting the real complexity of the very experience which he is reporting (by the verbal-report method) as simple. A more complex introspection throughout such an investigation would guard against such a dilemma.

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[25] This situation explains why the synaesthetic subject thinks that in "seeing" a tone he is "hearing" it. The experience is a complex; it is context; it means a "tone" even in the absence of an outstanding auditory quality. It also explains why the expert introspector regards his experience of red, pressure, or what not, as a "simple." He is looking for a simple. The simplest datum which will stand alone in consciousness is "meaning", a complex; hence the meaning, pressure or red, is the experience which he designates as ultimate. In case either of the red or of the pressure it is more probably true that the experience is synaesthetic and a process which will stand out in consciousness as a complex the instant the bias toward meaning is eliminated.

[26] Cf. Calkins, M. W., Attributes of sensation, *Psychol. Rev.*, 6, 1899, 506-514; Rahn, C., The relation of sensation to other categories in contemporary psychology, *Psychol. Monog.*, 16, 1913, 131 pp.; Talbot, E. B., The doctrine of conscious elements, *Philos. Rev.*, 4, 1895, 154 ff.; Washburn, M.F., Psychological analysis in system making, *Philos. Rev.*, 11, 1902, 445-462.

[27] Cf. Young, P. T., An experimental study of mixed feelings, this JOURNAL, 29, 1918, 237-271.

Let us take the case of pressure qualities. The purpose of a certain investigation may be to ascertain the number of distinguishable pressure qualities. At the outset the reagent may have reported visual imagery and incipient kinaestheses, but these are thought to be extraneous; under the preconception that pressure qualities are existential and not developmental he is instructed to ignore these secondary processes. Such reagents who ultimately fail to do so are regarded as possessing some peculiar individual difference or else they are regarded as poor introspectors. On the other hand, it is a matter quite open to debate whether the experience of pressure is really simplified in any reagent by such a procedure. It is the writer's experience that such a method only leads to a less analytical attitude on the part of the reagent, as a result of which he fails to cognize these secondary processes which do not drop out. He synthetizes them; *i.e.*, he learns to become conscious of the process as "pressure" rather than as "pressure plus"; he develops a bias toward unanalysed experience, thus preventing himself from realizing the sensory and imaginal details constituting the so-called simple meaning, pressure; just as the untrained observer possesses a bias toward the meaning, consciousness of self, and fails to realize its sensory and imaginal components. There is quite likely just as much danger of over-training as of under-training where introspection is involved.

It is the verbal-report method which leads to this over-training, this bias toward the "simple"! One never knows exactly what sort of a mental process he is dealing with, since to give a verbal report puts a premium on superficial analysis in spite of the good intentions of the experimenter and the reagent. The process appears simple for the same reason as meanings appear unanalysable to the untrained reagent. The psychology of affection, clearness and sensory qualities will undoubtedly be reinterpreted when the real complexity of these processes is generally recognized.

Our sixth point deals with the criteria of meaning. The above considerations as well as rapidly accumulating experimental evidence point to the fact that mental processes are not to be identified by structure but by function. Woods found no structural criterion for recognition; Fisher found none for the concept; Fernberger found none for the judgment; Wheeler found none for the volitional consciousness. Mosaics, cross-sections, and longitudinal sections reveal no particular uniformities, for these are to be found in function and not in content. Thus criteria of mental processes are features of behavior.

In fact, had there really been any definite structural criteria even of attributes of sensation, the literature of psychology would not have been rife with the disagreements of fact and interpretation as to how many such attributes exist and what they are like. Likewise the problem of affection will be solved only when we shift the emphasis of our point of view from structure to function.

Two outstanding facts throw light upon the problem of criteria. First, not only affection but any given phenomenon of meaning loses its original form of existence when analysed. The meaning, task-consciousness, for example, disappears as one attends to its components, and the meanings become those of "visual imagery", "kinaesthetic sensation" and the like. Hence the reagent reports: "I was conscious of the task and this consisted of so and so," or "I experienced certain sensations and imagery which meant to me a consciousness of the task." Secondly, a given mental process is not to be identified in terms of itself, even of its parts.

Rather, the entire situation in which a process takes place must be employed in defining a certain process. A task-consciousness is defined quite as much by the situation in which it arises as by its own structural contents. The latter will in no way differentiate it from any other process. Moreover, a certain grouping of mental contents may constitute a comparative judgment in one situation and an act of choosing in another. Another group of contents may constitute a perception in one situation, an act of recognizing in another; another group may constitute an awareness of an experimental situation at one time and a task-consciousness at another time; a kinaesthetic complex may constitute a consciousness of a bodily attitude now, unpleasantness later, and so on. The manner in which these processes arise, run their course and disappear, together with their antecedent and subsequent processes, constitutes their criteria. Even the reagent's past and the environmental situation are added criteria and in many instances are the features which define the mental process in question.

For the purpose of convenience let us divide the criteria of process into three classes: (1) a function, or process-pattern; (2) a behavior-pattern; (3) a situation pattern. As an example of these various criteria we take the case of unpleasantness in a certain synaesthetic reagent. On trying to recall the name of a person whose voice is reddish-brown, our reagent experiences unpleasantness. He reports that strains appear about the brows and in the throat; eye-movements take place; the entire kinaesthesis is visualized in "moving streaks and splotches of black"; strains increase in their intensity and location. Now the black-kinaesthesis constitutes unpleasantness.

The gradual increase in intensity of "colored" strains, their persistence and irradiation constitute criterion (1), the process-pattern. But this fails to identify unpleasantness; for there is nothing so far that will differentiate unpleasantness from muscular strain. Black-kinaesthesis may mean either the one or the other; which it will mean does not depend upon its content, or upon the immediate behavior of these contents, but upon antecedent and subsequent processes. We must thus rely upon a second criterion, a behavior-pattern.

This criterion is somewhat as follows: a definite conscious purpose has been thwarted; the reagent's recall of a name has been delayed; conflicts have developed. Owing to this situation the flow of conscious contents does not stop with the development of strain; the strains are to be *interpreted*. This interpretative consciousness may consist of a shift of attention to a certain limited region of the body which is in a tensed condition; there develops a beginning of a consciousness that the task is not being fulfilled; there follows a slight strain in the throat to pronounce a word; the word may be "unpleasant" or some equivalent. Altogether this entire behavior, patterned and conditioned by antecedent events, is our second criterion. But even yet unpleasantness has not been differentiated fully from other modes of behavior.

A third criterion remains to be ascertained. This can be found only after ascertaining the environmental conditions under which thwarted purposes lead to tensed bodily strains and organic experiences or to this or that bodily attitude. One would have to take into account the reagent's emotional predispositions. This genetic and environmental situation might be called the situation-pattern.

Is it any wonder, then, that Crosland found no specific structural pattern either for pleasantness or for unpleasantness[28] and that no specific structural patterns have been found for recognition, the concept, the judgment, the choice? None of these processes can be differentiated from any other without having recourse to the temporal relations of contents as they are running their course, to antecedent and subsequent processes, including *Kundgabe*, and to the entire situation in which the process in question is occurring. Moreover, of what psychological significance is it to reduce functional aspects of mind to mere temporal relations and shiftings in clearness, to determining tendencies of *Aufgaben*, to perseverative tendencies and the like? Do not these same phenomena mean far more when described in their natural setting of stimulus and response? Are not the real criteria of mental life behavioristic?

To state the matter briefly in another way, we have suggested three functional criteria of mental processes. (1) The process-patterns of so-called original mental contents. These are responses to physical and physiological stimuli. Moreover they are not functions existing independently of contents; they are contents in action. Every mental process runs its course in consciousness in a certain way, terminating in a different process from the original. This criterion refers to the so-called elements of consciousness, but emphasizes them as dual or synaesthetic processes. (2) Behavior-patterns in which these dual processes are functioning. Here, in any given mental process the contents may vary from individual to individual and from time to time in the same individual while mental functions remain the same. This behavior-pattern is a "setting" involving antecedent and subsequent processes of any original content as well as simultaneous groupings of original contents. Its mechanisn of development is the motor response; hence we find that kinaesthesis is an essential "structural" component. This criterion refers to such processes as perceptions and ideas whose contents may vary but whose functional aspects do not. (3) The situation-patterns in which the behavior-patterns are functioning. Here are to be included the peculiar setting of the environment which conditions the behavior-pattern in question, the predispositions of the individual contributing to his behavior of the moment, and the response of the moment, regarded as a complex, single act. Here again the mechanism of development and action is motor response; hence we find kinaesthesis an element common to all individual cases. This criterion refers to the higher intellectual processes such as reasoning, judging and the attitudes, including the phenomena of comprehension and interpretation. Here are to be found processes whose original contents may be totally unlike except for kinaestheses, but whose functional aspects are the same. Moreover, the same processes, structurally considered, may constitute different phenomena of behavior in different situations.

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[28] *Op. cit.*, 143.

These three criteria correspond to our three stages in the development of meaning; in fact they represent three levels of complexity of meaning. The first stage consists of original process-aspects of mental contents; the second stage is a behavior-pattern by means of which we "implicitly" know, and by means of which data in consciousness become data of consciousness, and further by means of which the environment becomes implicitly meaningful. Under these conditions the individual is reacting to objects and to his own actions as if they were consciously meaningful without being reflectively aware of these meanings. The third stage is the situation-pattern by means of which knowledge becomes explicit; by means of motor response the individual labels, recognizes, or defines an environmental situation, an object, or one of his own acts. Here reactions whose contents vary come to be defined by the conditions under which they take place.

Our last problem deals with a theory of meaning. We propose the behavioristic view that meaning bears the same relation to acquired human behavior as do instinct and modified instinct to animal behavior. Instinctive or modified instinctive motor responses in animal behavior are its ways of "labelling" or of "defining" its environment and its own bodily conditions. For example when a chicken sees a hawk and runs for cover, the instinct of flight "defines" the hawk as a dangerous object; flight and the attending fear, if any, are the only forms of the "meaning", hawk, apparently observable in the behavior of the chicken. During the course of human evolution, however, there gradually developed from this "lower" type of response a secondary and derived mode of reaction, superimposed upon the older and more basic instinctive type. Consequently the human being may react instinctively to a dangerous object by running from it, but he may respond intellectually as well. In either case the mechanism of defining is motor response. The human being "knows" a dangerous object by making a motor response *to* it.

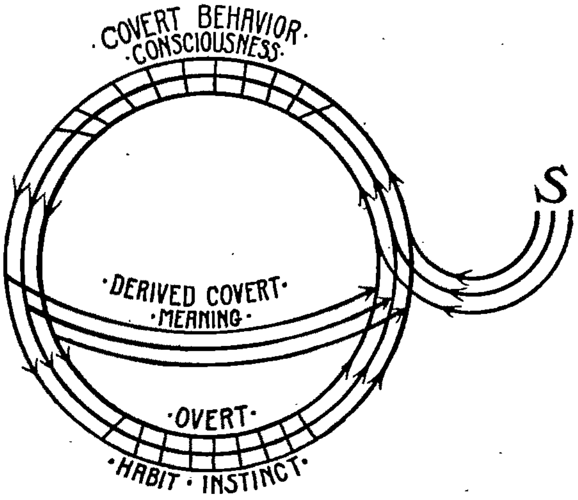

FIG. I. SCHEME SHOWING RELATION OF MEANING TO BEHAVIOR IN GENERAL

The accompanying diagram illustrates our point. Let the circle represent the concept of the reflex arc. *S* is an external stimulus as a result of which there is set up an integration of nerve-discharges. The physiological feature of this integration is the transfer from sensory to motor discharges, while the psychological feature is covert behavior or consciousness.[3] We assume that consciousness is no epiphenomenon or parallel phenomenon but rather the integration itself, a synthesis. Covert behavior thus stimulated leads directly to the overt or explicit act of running for cover in the case of instinctive response.

In the case of intellectual activity, however, the original covert behavior leads to a second type of response which might be called derived or secondary action, consisting of incipient throat, facial movements and bodily attitudes. By this means there is added to or there emerges from the original covert behavior a derived covert behavior the kernel of which is kinaesthesis; and this behavior is meaning. The antecedent of this response was a previous mental state; hence the response itself is reflective, as offset against non-reflective covert behavior; it is what has been called "self-consciousness"; it is *consciousness of* anything as offset against mere consciousness.

This scheme explains the difference between perception and recognition, between mere sensory discrimination and the conscious noting of relations, between responding to a repeated feature of a class of objects and conscious noting of abstract relations. In other words, the shorter arc is the basis of intelligence,—recognition, comprehension, interpretation; the longer arc means instinct and non-deliberate habit. The second is non-intellectual and quite possibly represents a truer picture of what the Freudians mean by the subconscious.

When unanalysed, meanings as states of consciousness are different qualities of experience as truly existing as is a chemical compound, but no more existential or *sui generis* than the chemical compound. Thus the meaning, tree, is a different quality of experience from the meaning, house. The quality is a novelty of existence derived from context, synthesis, and complexity of detail, but when these meanings are analysed it is found that nothing but patterns or modes of function will identify them. As a conscious process meaning has nothing to identify it from a structural standpoint; no specific elementary content differentiates it any more than hydrogen or oxygen, as such, will differentiate water from peroxide. In other words, on their own, unanalysed level, meanings need no structural differentiation; as such they *are* different by action, not by content. When analysed down to the level of their component contents, the meanings have disappeared and hence need no structural differentiation. The important problems, then, in connection with meaning are functional and not structural; meanings are to be described, as are all phenomena, in terms of structure, but are to be identified not by contents but by modes of behavior. The best functional treatment of meaning is behavioristic, therefore, for only such a point of view seems to do justice to the facts.

THE EQUATION OF THE LEARNING FUNCTION

By MAX F. MEYER and F. O. EPPRIGHT

Introduction

In spite of the fact that *the learning function is the central problem among all those problems of human and animal life which interest the psychologist*, very little has been done to describe it exactly by a suitable equation. Only one serious attempt has been made a few years ago by L. L. Thurstone,[1] who deserves much credit for trying to do this pioneer work. It is therefore necessary to state here, briefly, why I cannot follow Thurstone on the particular road which he has chosen to travel.

My objection to his procedure is that it is not based on sufficiently "rational" considerations. He says (p. 25): "As long as we do not have the basis for a rational equation for learning, I have been content to abide by it." (By a common hyperbolic equation!) Why so pessimistic? I believe that we possess much more of a rational basis than he admits.

I assert that in Thurstone's equation, $Y=f(X,L,R)$, neither the variables nor the constants have been chosen on a sufficiently rational basis. He defines Y as the number of successful acts per unit time; and X as the total number of practice acts since the beginning of formal practice. There is a very serious objection to this definition of X. It is perfectly well known, ever since Ebbinghaus if not for an even longer time, that the practice value of a particular number of practice sets varies enormously according to how they are temporally distributed. X is therefore no independent variable in $f(X,L,R)$, but both Y and X are functions of time, a fact which throws out Thurstone's definition of X in so far as it claims to be an axiomatic definition.

Even more objectionable is his definition of Y. He defines it as the number of *acts per unit time*. He has obviously been misled by the common sociological, but artificial and not fundamentally biological concept of "speed in working." A fundamental biological concept is, rather, that of the *time consumed in performing one complete and errorless act* (meaning by 'errorless' that any errors which may have occurred must have been corrected before the act is called complete).

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[1] L. L. Thurstone, The Learning Curve Equation, Psych. Mon. 114, 1919, 51 pp.

A hungry cat is safe from starvation when the act of catching the mouse is complete. The completeness of the act is the primary biological concept; the speed of catching is a secondary one. No matter how great the speed of the watch and of the chase, the cat will starve if it 'almost' caught the mouse, but not quite. Even in civilized society,—the shoemaker can not make a living, no matter with what speed he has made a pair of shoes, if he tries to deliver them to his customer without the eyelets. And the tailor, no matter with what speed he has sewed the left sleeve on the right armhole and the right sleeve on the left armhole, will starve until he has corrected his error. Completeness of the task is the primary biological concept. Speed is a secondary and artificial concept. Speed cannot enter the equation as an axiomatic term.

The biological argument that we have just applied to the variable Y, applies of course also to Thurstone's first constant, L, which he defines as the upper limit of *acts per unit time.*

The second of his constants, R, is called by Thurstone the "rate of learning." This, on account of the *name chosen,* impresses the present writer as a confusing and irrational definition. R appears in his equation

$$Y=\frac{LX}{X+R}=\frac{L}{1+\frac{R}{X}}$$

as the numerator of a fraction of which X is the denominator. R, therefore, could be defined very well as that inherited property of this particular animal which determines the practice value of each of the *practice acts (whose number is X) of this animal* in comparison with the practice value of the same kind of practice (kind of task) when performed by a different animal of different heredity; that is, R could be defined as the reciprocal value of the "individual learning ability." But no mathematician, except in a moment of inattention, can assent to meaning by the "rate of learning" anything other than the first derivative of f(X,L,R), that is, than $\frac{dY}{dX}$. The "rate of learning" cannot be conceived of as one of the *constants* of the "learning" equation, unless this equation is what it undoubtedly is not, the equation of a straight line. In spite of this adverse criticism, however, Thurstone deserves very much credit for having been the first to attempt a solution of the problem.

Definition of the Primary Variables and Constants

(*t*) Let the variable "*t*" be the duration of any test (which is also practice) in which a certain finite task has been finished errorless (that is, including the correction of any errors which may have occurred).

(n) Let the variable "n" be that number of such *tests repeated at regular intervals*, which have *preceded* any certain test,—the duration of the third test, for example, being that t which corresponds to n equal to 2.

(a) Let the constant (or parameter) "a" be the "practice value" of the particular test in comparison with any other kind of "practice" which the individual may undergo during each of the regular intervals aforementioned, or in comparison with another and independent test series, where another task, larger or smaller than this, will of course on account of its magnitude have a larger or smaller practice value than the practice value "a."

(b) Let the constant (or parameter) "b" be *a certain fraction of* the "practice value" of any other kind of "practice" which the individual may undergo during each of the regular intervals in addition to the test-practice "a." By *a certain fraction* is meant that fraction of its practice value which influences, not only the practice "b," but also the outcome of the test "a." The influence of this fraction "b" may be either positive or negative according as this other kind of practice may have been chosen to be *helpful* toward the practice "a" or intentionally to *interfere* with it through building up habits opposed to those of the test-practice "a."

Let "c" etc. be those fractions of the practice value of any further kinds of practice which are intended and expected to be helpful or harmful to progress in the "a" test.

If there is no practice other than the test practice, then b, c, etc. are each equal to zero.

(p_a) Let the constants (or, rather, parameters) "p_a, p_b, p_c, etc." be the measure of the combined influence of all our instincts predetermined by heredity and of all that previous life experience of ours which is capable of extending its influence to any of the specific learning tests having the nature and practice value called "a, b, c, etc.," respectively.

The value of "p_a" can then be regarded as the *equivalent* (*positively or negatively*) of a certain number of regularly repeated practice units "a" exerting their influence on the outcome of the actual "a" tests. That is, it amounts to the same as if so many tests had been taken before the first actual and formal test which occurred at the time $n=0$. That is, again, to n will have to be added p_a before multiplying by the practice value "a" of each of these tests.

(p_b) Let the constant "p_b" be regarded as the equivalent of a certain number of formal practice periods devoted to the "b" practice, which actually have not occurred before the time $n=0$. That is, to n will have to be added p_b before multiplying by the practice value "b" of each of the "b" tests (practice periods).

Let "p_c, etc." be defined correspondingly.

$$(n+p_a)a+(n+p_b)b+(n+p_c)c \text{ etc.} = \\ =n(a+b+c \text{ etc.})+ap_a+bp_b+cp_c \text{ etc.}$$

In case there is no formal practice of the nature "b," "c," etc., then "b," "c," etc. are severally zero. And the above measure reduces to $(n+p_a)a$, which may be written simply $(n+p)a$.

In the new-born baby, if we could and would subject him to formal learning tests of such make-up as to be *an absolutely simple(and therefore also purely abstract, unreal) sensory-motor task to be learned*, p_a would by definition be negative, since all true learning[2] consists then and thus, never in improving a substitution already forming, but first in breaking down the inherited reaction. In *concrete* tests of formal learning the probability that

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[2] True learning means "substituting a different reaction." Compare the author's *Psychology of the Other-One*, 1922, 119.

p_a is more and more negative corresponds, leaving out of the question the make-up of the tasks, to the greater and greater youthfulness of the animal tested.

(O) Let the constant (or parameter) "O" be the "original" duration of the test. This duration would depend on the individual's "original nature" (if such a test could have been given previous to and without any and all life experience). Of course, the value of O is a purely theoretical quantity, never to be directly observed in any test unsuitable to new-born babies. O corresponds to the condition which philosophers of former centuries used to call "tabula rasa." O is also equal to the product LT. See below.

(T) Let the constant (or parameter) "T" be the limit which the duration of a test approaches without ever reaching it, no matter how large *n* becomes.

(L) Let the constant (or parameter) "L" be the "learning ability" with which the individual has been equipped by heredity; that is, the factor by which O must be divided in order to yield T. O is therefore the product LT. The number L is most likely to be in the neighborhood of or less than 10 (as experience seems to indicate) and of course more than 1.

Determination of the Learning Function in its Primary Form

It seems rational to assume that the function F, which is yet to be determined, of the variable *t* and the parameters L and T should be proportional to the sum of all the products $(n+p_a)a$, $(n+p_b)b$, $(n+p_c)c$, etc. This is provisionally expressed by writing the following equation:

$$\text{I.}\ F(t,L,T)=F[f(t,L,T)]=n(a+b+c\ \text{etc.})+ap_a+bp_b+cp_c\ \text{etc.}$$

Our world experience seems to show, further, that the value of the subfunction f, which will later be determined as a function of *t*, L and T, is such a function of F, $f=\varphi(F)$, that the variable, f, for an increasing F in $\varphi(F)$, *drops from a finite value*, O, *to a finite value*, T, first at an ever increasing rate, later, after *passing through an inflection point*, at an ever decreasing rate. The value of $f=\varphi(F)$ seems to be asymptotic to the limits O and T aforementioned.

The inflection point is rationally demanded by the universal testimony in the beginning of learning a difficult performance that "I shall never learn that," and at a later stage that "I am surprised at the progress I am making." It goes without saying that at a still later stage this progress becomes again inconspicuous as it was at first.

But the inflection point is rationally demanded even more by our world experience that nothing "occurring" ever occurs abruptly, but that everything that starts to change starts with a change which is zero. And everything that ceases to change, ceases with a change which is zero. Example: a train leaving one station in order to stop at another. What "occurs" is that the train at some time is found out of the station. At the moment of starting to change position its velocity is never anything but zero, and at the moment of ceasing to change position its velocity is again zero. The mathematical physicist asserts that discontinuous functions do not exist in nature.

Such a function $\varphi(\mathrm{F})$ is, for example, the *arc of a cotangent* if that cotangent, F itself, varies from *minus infinity* to *plus infinity*.

We therefore write $\mathrm{f}=\varphi(\mathrm{F})$ in the form $\mathrm{f}(t,\mathrm{L},\mathrm{T})=\operatorname{arc}\cot \mathrm{F}$ and therefore

$$\mathrm{F}[\mathrm{f}(t,\mathrm{L},\mathrm{T})]=\cot \mathrm{f}(t,\mathrm{L},\mathrm{T})$$

and we rewrite the equation I. in the following more determined form:

$$\text{II.}\quad \cot \mathrm{f}(t,\mathrm{L},\mathrm{T})=n(\mathrm{a}+\mathrm{b}+\mathrm{c}\ \text{etc.})+\mathrm{ap}_{\mathrm{a}}+\mathrm{bp}_{\mathrm{b}}+\mathrm{cp}_{\mathrm{c}}\ \text{etc.}$$

Since the function F has been determined as a cotangent, the subfunction $\mathrm{f}(t,\mathrm{L},\mathrm{T})$ is to be determined in accordance with the two conditions, first, that it becomes π when t equals the constant O, and second, that it becomes zero when t equals the constant T, which two constants have been defined as the limits of the possible duration of the test. Therefore we write

$$\text{III.}\quad \mathrm{f}(t,\mathrm{L},\mathrm{T}) = \frac{(t-\mathrm{T})\pi}{\mathrm{O}-\mathrm{T}}$$

And since $\mathrm{O}=\mathrm{LT}$, we derive from equation II.

$$\cot \frac{(t-\mathrm{T})\pi}{(\mathrm{L}-1)\mathrm{T}}=n(\mathrm{a}+\mathrm{b}+\mathrm{c}\ \text{etc.})+\mathrm{ap}_{\mathrm{a}}+\mathrm{bp}_{\mathrm{b}}+\mathrm{cp}_{\mathrm{c}}\ \text{etc.}$$

$$\text{IV.}\quad \cot \frac{\left(\frac{t}{\mathrm{T}}-1\right)\pi}{\mathrm{L}-1} = n(\mathrm{a}+\mathrm{b}+\mathrm{c}\ \text{etc.})+\mathrm{ap}_{\mathrm{a}}+\mathrm{bp}_{\mathrm{b}}+\mathrm{cp}_{\mathrm{c}}\ \text{etc.}$$

The equation IV. is the equation of the Learning Function in its primary form.

In order to find from any series of experimental data the constants or parameters whose discovery is the purpose of the experiment, it is necessary to transform equation IV. so that it can most easily be applied to the experimental data of the particular case. We shall make this transformation presently.

It seems possible to understand rationally that the inflection point — at which $(t-\mathrm{T})$ is one-half of $(\mathrm{O}-\mathrm{T})$ and also the sum of the products $(n+\mathrm{p}_{\mathrm{a}})\mathrm{a}$ etc. is zero — should be reached just where this sum is zero. Until the inflection point is reached, the biological processes are such that "breaking down" prevails among the sensory-motor units of function. After the inflection point, "building up" prevails. (For a hint as to the real significance of these two stages, "breaking down" and "building up," compare the discussion of the two stages of learning in the author's *Manual of Psychology Demonstrations*, 1922, 10.)

Numerical Determination of the Parameters p, L, T *and* a *by Plotting*

The most convenient method (requiring no greater mathematical knowledge than reading a trigonometric table) of finding the constants of the Learning Curve of any experiment consists in plotting the experimentally found values of t against n on a sufficiently large sheet of coordinate paper and then plotting the computed values of t for various constants until one is satisfied that, but for accidental variations, the computed curve is identical with the experimental curve. An example of this method will be given below.

The preliminary thing necessary, then, is to solve equation IV. for t. This is easily done and yields as result equation V. For simplicity's sake we choose an experiment in which b, c, etc. are severally equal to zero, that is, in which there is no practice other than that of the repeated tests; and in which the constant a may be made "unit," that is, in which no comparison is made with another experimental series setting a different task for the test, and whose task then might, and probably would, have a different practice value.

$$\text{V.}\quad t=\mathrm{T}+\frac{(\mathrm{L}-1)\mathrm{T}}{\pi}\operatorname{arc\,cot}(n+\mathrm{p})$$

It is convenient to substitute K for that factor by which the arc cot is multiplied.

$$\mathrm{K}=\frac{(\mathrm{L}-1)\mathrm{T}}{\pi}$$

$$\text{VI.}\quad t=\mathrm{T}+\mathrm{K}\operatorname{arc\,cot}(n+\mathrm{p})$$

Below is given a table of experimental data consisting of the records of a test made on the present writer during the spring of 1922. The experiment was not at all an experiment on learning, but on the effects of certain drugs taken day after day in a certain rotation. That is the reason why the data in the table are arranged in four columns. However, irrespective of the influences of the drugs which may be regarded as accidents, the data are the records of the time necessary to read and rewrite in shorthand a certain definite and constant list of words presented in shorthand.

SUGAR			TEA			ALCOHOL			TOBACCO		
Date	t	n	Date	t	n	Date	t	n	Date	t	n
May 26	5503	0	May 27	4424	1	May 28	4331	2	May 29	3974	3
May 30	3371	4	May 31	2945	5	June 1	2961	6	June 2	2725	7
June 3	2712	8	June 4	2536	9	June 5	2594	10	June 6	2470	11
June 7	2643	12	June 8	2411	13	June 9	2333	14	June 10	2295	15
June 11	2203	16	June 12	2253	17	June 13	2325	18	June 14	2117	19
June 15	2158	20	June 16	2110	21	June 17	2102	22	June 18	1972	23
June 19	1985	24	June 20	2048	25	June 21	2143	26	June 22	——	27

In order to find now by plotting the parameters T, L, and p for this experiment, we proceed as follows. First we prepare a large enough sheet of paper with eight blank columns and a large number of lines. (See Computation Table I.)

The first column from the left we call n. It will contain the ordinal numbers, 0 for the first test, 1 for the second test, 2 for the third test, and so forth. The second column will place before our eyes the constant value of p as we guess at it with increasing approximation to its true value. The third column will contain the sum of n and p. The fourth column is open for our entries of the arc belonging to that cotangent which is equal to the just mentioned sum in the preceding column. It is most convenient to use trigonometric tables which give the arc in degrees and minutes. We write down the degrees immediately, then place a decimal point, and find the next two decimals by dividing, mentally, the minutes of the table by 6. The fifth column serves to place before our eyes the constant K. guessed at with approximation gradually increasing. The sixth column receives the product of the numbers contained in the fourth and fifth. The use of Crelle's *Calculating Tables* is to be recommended for this purpose. The eighth column receives the values found experimentally as corresponding to the various values of n. Now we have to scrutinize columns six and eight in order to choose a value of T which, when added to the sixth column, will give the best possible approximation (entered in the seventh) to the somewhat irregular values of the eighth column.

Successive Approximations

(1) $p=2.0$, $K=100$, $T=1765$.
(2) $p=2.0$, $K=200$, $T=1545$.
(3) $p=2.5$, $K=200$, $T=1551$.
(4) $p=2.5$, $K=220$, $T=1508$.
(5) $p=2.5$, $K=180$, $T=1670$.
(6) $p=2.4$, $K=180$, $T=1700$.

Now we guess, for using our example of experimental records, first at $p=2.0$ and at $K=100$. We choose then from our table of experimental records either the extreme values of n or some values near the extremes, say, $n=2$ and $n=24$. In the eighth column we may at once write the corresponding values of t found during the experiment, *i.e.*, 4331 and 1985. The computation gives us (1) for the sixth column 1433 and 220. Choosing 1765 for T, we get for the seventh column 3198 and 1985.

COMPUTATION TABLE I.

n	p Constant	n+p	arc cot(n+p)	K Constant	K arc cot	+T	Experimental t
(1) 2	2.0	4.0	14.33	100	1433	3198	4331
24		26.0	2.20		220	1985	1985
(2) 2	2.0	4.0	14.33	200	2866	4411	4305
24		26.0	2.20		440	1985	1985
(3) 2	2.5	4.5	12.53	200	2506	4057	4305
24		26.5	2.17		434	1985	1985
(4) 0	2.5	2.5	21.80	220	4796	6304	5503
2		4.5	12.53		2757	4265	4305
6		8.5	6.71		1476	2984	2961
12		14.5	3.94		867	2375	2643
24		26.5	2.17		477	1985	1985
(5) 0	2.5	2.5	21.80	180	3924	5594	5503
2		4.5	12.53		2256	3926	4305
4		6.5	8.75		1575	3245	3371
6		8.5	6.71		1208	2878	2961
12		14.5	3.94		709	2379	2643
20		22.5	2.55		459	2129	2158
24		26.5	2.17		391	2061	1985
(6) 0	2.4	2.4	22.62	180	4070	5770	5503
2		4.4	12.80		2304	4004	4305
4		6.4	8.88		1598	3298	3371
6		8.4	6.78		1220	2920	2961
8		10.4	5.49		988	2688	2712
12		14.4	3.98		716	2416	2643
16		18.4	3.10		558	2258	2203
20		22.4	2.55		459	2159	2158
22		24.4	2.35		423	2123	2102
24		26.4	2.17		391	2091	1985
26		28.4	2.02		363	2063	2143

We notice that the range of the seventh column is insufficient. We therefore try a *greater* K, taking it this time as equal to 200. Computation (2) then yields for the sixth column 2866 and 440. Choosing 1545 for T, we get for the seventh column *a greater range*, 4411 and 1985, which is about the range we need. K, therefore, must lie in the neighborhood of 200, unless our guess at p was very far off the mark.

Let us see what difference it will make if we choose a slightly different p. We take p a little *larger*, equal to 2.5, and computation (3) yields for the seventh column a slightly *smaller range*, from 1985 to 4057.

Let us now (4) keep p equal to 2.5 and take K still a little *larger*, equal to 220, at the same time making the computation for five n's instead of two only. We now plot on a sheet of coordinate paper the experimentally found data consisting of the twenty-seven pairs of values for t and n. On the same paper we enter the five values for *t* just found by computation, 6304, 4265, 2984, 2375, 1985. If the reader will do this, he will convince himself that *the left branch* of the computed curve in the neighborhood of $n=0$ appears now to *fall too steeply.* In order to make it *less steep,* we might try to *reduce* K somewhat. Let us reduce it to 180.

We now compute (5) for seven judiciously chosen values of *n* the corresponding *t.* Plotting the values of *t* computed we observe that the left branch of the curve in the neighborhood of $n=0$ now has a better direction or bearing, but that it does not yet fall steeply enough. Nevertheless, we hesitate to reduce K still more, since the range of *t* is now pretty good, and we know already that an increase of K has a great effect on increasing the range. We therefore try to get still a little *more steepness* in the neighborhood of $n=0$ by *reducing* p *a little.*

We now take $K=180$ and $p=2.4$ and compute (6) for eleven values of *n* the corresponding values of *t* and plot them.

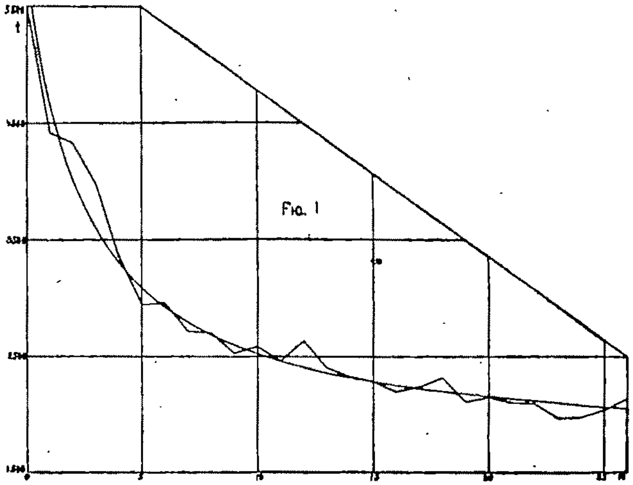


Fig. 1

The illustration shows how the curve of which they are points (*i.e.,* the smooth curve) passes among the experimentally found values of *t.* He who desires a still better approximation may let himself be guided by a rule which usually works out well, that *when the approximation is already pretty good and it is desired to make the curve more curved, more hollow, one should reduce both* p *and* K *a little. And to make the curve straighter, shallower, one should increase both* p *and* K *a little.* (Anyone interested in knowing why that is so, should study the mathematical properties of the cotangent.) Always last adjust T to suit that part of the experimental curve which runs most nearly horizontal.

As soon as we are satisfied with the values of p, K and T (for example with $p=2.4$, $K=180$, $T=1700$), we find L by reference to the substitution formula directly preceding equation VI.

$$L=1+\frac{\pi K}{T}=1+\frac{180\cdot 180}{1700}=1+\frac{324}{17}=1+19.1=20.1$$

Another Method for Determining the Constants p, L *and* T

(By F. O. Eppright)

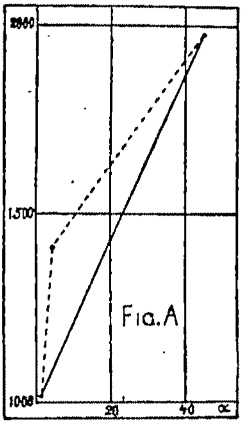
It may be readily seen by inspection of equation VI. that if t is plotted against the value of arc cot $(n+p)$ the resulting curve will be a straight line provided the correct value of p is chosen. This, of course, is true only if the empirical values of t vary approximately as this function of n. The fact that when the proper value of p is chosen this curve is seen to be approximately a straight line, or rather, a straight ribbon, is itself a justification for using, as a function of t and n to represent the learning curve, the kind of function we have used.

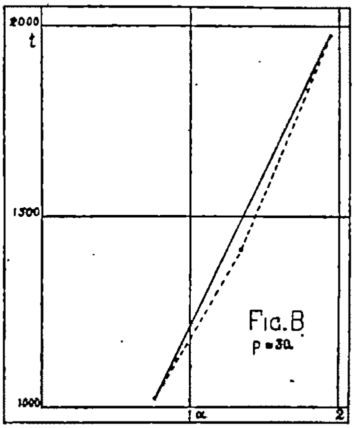
In plotting the values of t against the values of arc cot $(n+p)$ it is convenient to let $\alpha=\text{arc cot }(n+p)$. Then equation VI. may be written $t=K\alpha+T$. Now, when the correct values of α have been determined for the corresponding values of t, we get a more or less ragged ribbon as is shown in Figure D. K is then equal to the ribbon's slope, and T is equal to the ordinate value of that point at which the line of the ribbon, if extended, cuts the t-axis.

The procedure for determining the value of the constant p which will give the proper value of α is as follows. From the table of experimental records shown the writer chose three pairs whose n-values were 0, 13 and 44. Three points were chosen because at least three points are required to determine a straight line as distinguished from a curve. It is advantageous to choose one of these points near the beginning, one from near the end and one about one-third the way from the beginning to the end of the experiment.

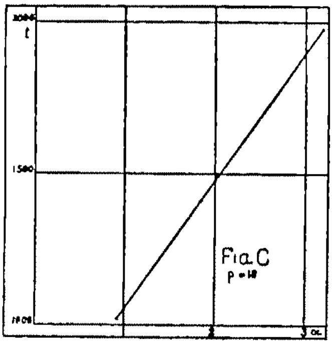
TOBACCO		SUGAR		TEA		ALCOHOL	
n	*t*	*n*	*t*	*n*	*t*	*n*	*t*
0	1977	1	1790	2	1796	3	1724
4	1671	5	1552	6	1498	7	1676
8	1509	9	1410	10	1421	11	1553
12	1386	13	1411	14	1449	15	1392
16	1312	17	1360	18	1333	19	1291
20	1254	21	1228	22	1253	23	1244
24	1242	25	1181	26	1199	27	1201
28	1132	29	1157	30	1127	31	1143
32	1127	33	1124	34	1105	35	1138
36	1091	37	1115	38	1053	39	1075
40	1078	41	1094	42	1035	43	1064
44	1024						

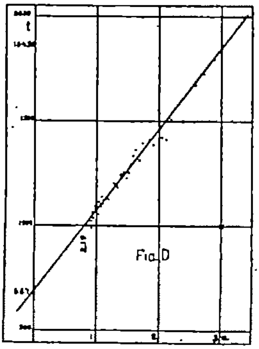
Rule columns as shown in Computation Table II. and write in the selected values of n and the corresponding values of t. Then arbitrarily assign some value to p, for instance 1, and fill in the column of the values for $n + \mathrm{p}$, in this case, 1, 14 and 45. By the aid of a trigonometric table of cotangents determine the corresponding values for α and write them in the proper column. Now plot on coordinate paper α against the values of t, that is, 1977, 1411, 1024. The three points thus located will form a triangle whose obtuse angle will open toward the right as in Figure A. This is due to p having been chosen too small.





We now try the same three values of n, 0, 13 and 44, for p = 30. This value of p has been taken too large as is shown by the fact that the obtuse angle opens toward the left as in Figure B. We now try a value for p which lies between 30 and 1, for example 18. By taking pains with further approximations of p we can make the three points of the angle lie as nearly in a straight line as may be desired. To make them lie exactly in line would ordinarily be a useless procedure, because the best that can be expected of the remaining points not yet located is that they will fall within a straight and reasonably narrow path whose direction has been indicated by the three points already placed. It happens here that p = 18 gives a sufficiently straight path as indicated in Figure C.





The value of p having been approximately determined by experimenting with three points, it is now well to calculate the remaining values of α and plot them as shown in Figure D. The path now containing more points, it will be easier to decide whether or not its trend is straight, and if necessary a further correction of the value of p can be made.

COMPUTATION TABLE II.

n	p	n+p	α = arc cot	t
0	1	1	45.00	1977
13		14	4.08	1411
44		45	1.27	1024
0	30	30	1.91	1977
13		43	1.33	1411
44		74	.77	1024
0	18	18	3.18	1977
13		31	1.85	1411
44		62	.92	1024

In the writer's opinion it is well to stop with the *smallest* value of p that will give a reasonably straight path. It may be expected to flare somewhat at the upper end. The *width* of the path is not particularly significant in choosing the value of p. A broad path merely means that the experimental values of t have deviated considerably both above and below the theoretical values as determined by the equation. After the path has become straight a further increase in the value of p may cause the path to become narrower, but it will not make the theoretical values of t approach any more closely the experimental values. On the contrary, it will cause the difference between them to become greater, which is obviously not to be desired.

After a suitable value of p has been determined in this manner, the values of K and T might be calculated by the method of Least Squares. This method is more or less time-consuming in that it requires a great deal of computation. There is also a difference of opinion in regard to its superiority to the method of inspection; but it does have the advantage that different computers will get identical results from the same data. For the convenience of anyone wishing to try this method we reproduce here the formula used by the mathematicians, in which "r" is equal to the number of pairs of values of t and α used for the computation.

$$T = \frac{\Sigma t_1 \, \Sigma \alpha_1^2 - \Sigma \alpha_1 \, \Sigma \alpha_1 t_1}{r \, \Sigma \, \alpha_1^2 - (\Sigma \alpha_1)^2}$$

$$K = \frac{r \Sigma \alpha_1 t_1 - \Sigma \alpha_1 \Sigma t_1}{r \Sigma \alpha_1^2 - (\Sigma \alpha_1)^2}$$

The writer therefore recommends the use rather of the method of inspection, the procedure for which is as follows.

After the points have been plotted on a rather large scale, hold a string stretched tightly between the two hands in that position which seems to coincide best with the general bearing of the points. Then draw a straight line to take the place of the string, as, for instance, in Figure D. Extend it to cut the t-axis. The ordinate value of this point is the value of T,—in this instance it is 687. To determine the value of K, select two convenient points on the line a considerable distance from each other, and by reading *from the scale of the drawing* determine their vertical and horizontal distances apart as indicated by the marked lines in Figure D. The value of K is equal to the vertical distance between the two points divided by their horizontal distance,— in this instance K is equal to 386. (This high quotient looks as if it referred to an almost vertical line. That the line in Figure D is not nearly vertical is due to the fact that the horizontal scale is greatly exaggerated in comparison with the vertical.) From K we easily find L.

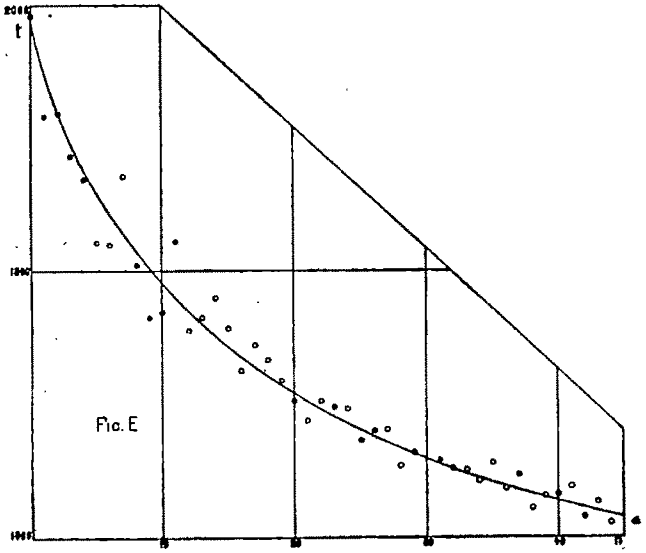


Fig. E

Figure E shows how the computed curve runs among the experimentally found dots.

If these constants are to be determined for several sets of data, the writer has found it desirable to make tables giving the values of α for a series of values of $n+p$. This is because the ordinary trigonometric tables give the values of the cotangents to degrees and minutes, but not to seconds. In consequence the value of the cotangent sought is seldom given in the table very exactly. It more often gives values lying considerably on each side of the one wanted. For example, if we wish to determine the angle whose cotangent is 61.0, we find that the table gives 61.3829 for the cotangent of no degrees and 56 minutes, and 60.3058 for the cotangent of 57 minutes. To find the angle whose cotangent is 61.0 we have to interpolate by substracting 60.3058 from 61.3829. This gives 1.0771, which is the amount the cotangent decreases as the angle increases from 56 to 57 minutes. But in order to reach the value of 61.0 the value of 61.3829 would have to decrease 0.3829, which is about 0.36 of 1.0771. Correspondingly the angle of 56 minutes would have to increase 0.28 of one minute. This gives us the angle of 56.36 minutes as the angle whose cotangent is 61.0. This value in minutes must now be converted to decimal parts of a degree by dividing 56.36 by 60, which gives us 0.94 degrees, the angle sought. It is therefore obvious that if these values are used more than once it is economical of both time and effort to arrange them in tabular form as is illustrated in the samples shown below.

FOR DECIMAL .0		FOR DECIMAL .5	
n+p	α=arc cot	n+p	α=arc cot
0.0	90.00	0.5	63.43
1.0	45.00	1.5	33.36
2.0	26.56	2.5	21.48
3.0	18.43	3.5	15.95
4.0	14.33	4.5	12.53
5.0	11.31	5.5	10.30
etc.		etc.	

We need only tables in which the successive values of n+p differ by unity. And since the p-values need never be chosen more exactly than including the first decimal, we need only ten such tables. The writer has made these ten tables for the decimals .0, .1, .2, . . . to .9, each table extending from zero units to 65 units of n+p. He will be glad to send mimeographed copies of them to those wishing to use them.

Definition of the Secondary Variables and Constants

Next to the *duration* of one act which, when complete, fills a biological need,— the *average speed* at which the interval between two certain moments of *time is filled by a multitude* of homogeneous and complete acts, is a factor of biological importance. For example, a society of bees which, during the season when plants bloom, can fill a larger number of cells with honey, establishes thereby a greater survival probability for itself (*i.e.*, both the living individuals and the future offspring). Another example: the farmer who during the season when corn planting is possible — which in some climates is limited to a few weeks of the year — can place a larger number of seeds properly in the soil, is less likely to starve from one harvest to the next.

(s) Let the variable "s" be the *average speed* at which a certain test (and practice) period has been filled by a multitude of homogeneous and complete acts of the nature and practice value "a" each.

(n) Let the variable "n" be that number of such test periods which, at regular intervals, have preceded any certain test,— the average speed of the third test, for example, being that which corresponds to n equal to 2.

(Θ) Let the constant "Θ" be that duration which has been chosen as convenient for each and every test (and practice) period of the experiment. The product of Θ and s is then obviously the equivalent to "the practice value" as primarily defined; with this difference, however, that now the practice value of the work done during a test period is a variable, "Θs," and not, as formerly, a constant, "a," of the equation.

(σ_a) Let "σ_a" be the *variable* number which indicates how many times that work unit whose size and nature is "a" can be (and is) performed at any of the speeds "s" during the constant test (practice) period lasting Θ time units. Briefly, it is *the amount per* Θ and depending, then, of course on n.

Since σ_a thus becomes the measure of *the quantity of practice*, it is obvious that it must, in this secondary form of the equation, take the place of "a" as used in that primary form.

It is also obvious that Θs can be *substituted* for σ_a, since the number σ_a divided by the duration Θ must be the speed s.

$\frac{\sigma_a}{\Theta}=s$ and therefore $\Theta s=\sigma_a$.

(σ_b) The variable number "σ_b" (depending on n) shall indicate the product of two factors, one being the fraction of "b" either helpful or harmful to progress in "a," the other factor being the number of (with n variable) times the task "b" can be performed at any of the speeds "s" during the test (practice) periods lasting Θ.

But we cannot substitute for σ_b anything more definite without a special experimental investigation of the varying speed of "b."

Further, "σ_c" shall indicate the correspondingly understood variable product for the task "c." And so forth for other terms of the kind σ.

(N_a) Let "N_a" be the constant which indicates the number of imaginary formal practice periods devoted to "a" which, if instead of his previous life experience they had preceded $n=0$, would have enabled the individual to work at the task "a" with the same speed with which he really does work at the task "a" when n is equal to zero.

(N_b) Let "N_b" be the constant which indicates the number of imaginary formal practice periods devoted to "b" which, if instead of his previous life experience they had preceded the moment $n=0$, would have enabled the individual to work at the task "b" with the same speed with which he does work at the task "b" when n is equal to zero.

Let "N_c, etc." be defined correspondingly.

(Ω) Let the constant "Ω" be the "original" speed. It is a purely theoretical quantity (like the former "O"), never to be directly observed. It corresponds to the philosophers' "tabula rasa."

(S) Let the constant "S" be the limit which the average speed per working period approaches without ever reaching it, no matter how large n becomes.

(L) Let the constant "L" be an hereditary factor having the same meaning as our formerly defined "individual learning ability." That is, the reciprocal value of Ω is equal to L times the reciprocal value of S. And $\Omega L = S$.

Determination of the Learning Function in its Secondary Form

It seems rational to substitute in equation III. $\frac{1}{s}$ for t, $\frac{1}{S}$ for T, $\frac{1}{\Omega}$ for O, and in equation IV. σ_a or Θs for a, σ_b for b, σ_c for c, etc., N_a for p_a, N_b for p_b, N_c for p_c, etc.

We thus obtain

$$\frac{\left(\frac{1}{s}-\frac{1}{S}\right)\pi}{\frac{1}{\Omega}-\frac{1}{S}}=\frac{\left(\frac{1}{s}-\frac{1}{S}\right)\pi}{(L-1)\frac{1}{S}}$$

and further (compare equation II. in order to see the reason)

$$\cot\frac{\left(\frac{1}{s}-\frac{1}{S}\right)\pi}{(L-1)\frac{1}{S}}=n(\sigma_a+\sigma_b+\sigma_c\text{ etc.})+\sigma_a N_a+\sigma_b N_b+\sigma_c N_c\text{ etc.}$$

$$\text{VII.}\quad\cot\frac{\left(\frac{S}{s}-1\right)\pi}{L-1}=n(\Theta s+\sigma_b+\sigma_c\text{etc.})+\Theta s N_a+\sigma_b N_b+\sigma_c N_c\text{ etc.}$$

The equation VII. is the equation of the Learning Function in its Secondary Form. But a glance at the equation, if we remember the definitions agreed on, is sufficient to convince us that, exactly as it stands there, we shall never have any use for it. It must be simplified for the following reasons, which are obvious enough. The coefficients σ_b, σ_c, etc. are not constants, but are themselves functions of n similar to the function $s = f(n)$, but differing from it at least in their own constants. And to discover the constants of these functions σ_b, σ_c, etc. experimentally, too, while experimenting on finding the constants of the main function $s = f(n)$, seems to be a task too complicated for mere human intelligence.

It is a different matter, however, if we restrict our experiment to a single kind of practice, leaving out all other kinds of formal practice of a helpful or harmful nature to the test practice. Then σ_b, σ_c, etc. are all zero, and equation VII. reduces to the following more comfortable equation:

$$\text{VIII.}\quad \cot\frac{\left(\frac{S}{s}-1\right)\pi}{L-1} = (n+N_a)\Theta s,$$

in which we might just as well write simply N for N_a.

This is the only equation of the Learning Function in its Secondary Form for which we shall have any use in psychological experiments,— that is, this equation VIII. or one to which it reduces by regarding any one of its four constants as an absolute constant, a fixed number, and only the other three as parameters which may be differently selected in the experiment.

For example, if we have correctly interpreted the meaning of the constant L, then, by performing all the experiments on the same individual, that individual's *learning ability*, L, should be found to be the same number in all the experiments. Experimental work still to be undertaken will have to prove or disprove this point.

Or, if every test (practice) period lasts invariably half an hour, Θ may as well be called "unit" and our equation would be:

$$\text{IX.}\quad \cot\frac{\left(\frac{S}{s}-1\right)\pi}{L-1} = (n+N)s$$

The equation IX. is that equation of the Learning Function in its Secondary Form which will be most useful to most experimenters, for generally they will select a definite number of minutes as a convenient test (practice) period to be adhered to throughout their experimenting.

Numerical Determination of the Parameters N, L, S *and* Θ *by Plotting*

The method of finding the constants of the Learning Curve by plotting is much less convenient in the secondary form of the equation than in its primary form. That is reason enough why *experimenters should shun as much as possible the secondary form* and plan their experiments so that working efficiency is measured by *keeping a record of the durations*, and not, as here, of the speeds.

The inconvenience is due to the fact that such an equation as VIII. or IX. is *by no analytical method* solvable for s. It is solvable only for n.

$$\text{X.}\quad n = -\text{N} + \frac{1}{\Theta s}\cot\frac{\left(\frac{\text{S}}{s} - 1\right)\pi}{\text{L} - 1}$$

When Θ is called "unit," the equation X. becomes of course a little simpler, since the cotangent is then to be divided simply by s instead of by Θs.

Below is given a table of experimental data consisting of the records of tests made by Mr. F. O. Eppright on himself. The experiment was not at all an experiment on learning, but on certain drug actions. It is as such a further typical example of an experiment ideally unrelated to learning which cannot actually be performed without taking into account the learning curve. The data are arranged in four columns in order to make it easier to see which of them were influenced by this or that drug. But the drug effect will not be discussed here, but in a different publication. The value of s in the table shows the number of words of a small and supposedly known vocabulary translated during $\Theta = 30$ minutes from one language into another and written down. The drug effect, left out of consideration here, is mentioned here only as one among many factors making the experimental curve irregular.

ALCOHOL			TOBACCO			SUGAR			TEA		
Date	s	n	Date	s	n	Date	s	n	Date	s	n
May 28	123	0	May 29	130	1	May 30	138	2	May 31	142	3
June 1	143	4	June 2	151	5	June 3	149	6	June 4	165	7
June 5	170	8	June 6	175	9	June 7	182	10	June 8	184	11
June 9	173	12	June 10	193	13	June 11	200	14	June 12	200	15
June 13	194	16	June 14	203	17	June 15	207	18	June 16	209	19
June 17	203	20	June 18	207	21	June 19	212	22	June 20	217	23
June 21	212	24	June 22	221	25	June 23	226	26	June 24	219	27
June 25	214	28	June 26	—	29	June 27	—	30	June 28	—	31

In order to find now by plotting the parameters S, L and N for this experiment, we proceed as follows. First we prepare a large enough sheet of paper with nine blank columns and a large number of lines. (See Computation Table III.)

The first column from the left we call *s*. It will contain the records successively taken in the experiment. The second column is open for our entries computed through dividing S by *s*. The third column will contain the value of the second column diminished by 1. The fourth column will place before our eyes the constant value of π divided by $(L-1)$, taking as the value of π the number of degrees, 180. The fifth column will receive the product, with not more than three places after the decimal point, of the third and the fourth entry. It goes without saying that for all divisions and multiplications we have Crelle's *Calculating Tables* on hand. The sixth column will receive the cotangent of the product just mentioned, as far as three decimal places. We look up the cotangents in any small trigonometric table of degrees and minutes (seconds are not needed). In order to use the tables, we have to translate the decimals of the last column's product from hundredths into sixtieths, that is, into minutes. This need not be done in writing. Mental arithmetic suffices since it requires only multiplying the decimals by 6 and rounding off the minutes thus found. The seventh column stands ready to receive the cotangent divided by the number of the first column. The eighth column, being the quotient just mentioned minus N, tells what the value of *n* ought to be if this *s* had been the true *s*, free from accidental variations. And the ninth and last column gives us the actual value of *n* in the experiment.

COMPUTATION TABLE III.

s	$\frac{S}{s}$	$\frac{S}{s}-1$	$\frac{\pi}{L-1}$ Constant	Product	cot	$\frac{\text{cot}}{s}$	$\frac{\text{cot}}{s}-N$	Experimental n
(1) .123	2.44	1.44	20.00	28.8	1.819	14.8	0.0	0
.214	1.40	.40		8.0	7.115	33.2	13.2	28
(2) .123	2.44	1.44	10.00	14.4	3.895	31.7	0.0	0
.214	1.40	.40		4.0	14.301	66.8	35.1	28
(3) .123	2.60	1.60	10.00	16.0	3.487	28.4	0.0	0
.214	1.50	.50		5.0	11.430	53.4	25.0	28
(4) .123	2.60	1.60	9.47	15.152	3.693	30.0	0.0	0
.142	2.25	1.25		11.837	4.773	33.6	3.6	3
.149	2.15	1.15		10.890	5.201	34.9	4.9	6
.175	1.83	.83		7.860	7.237	41.4	11.4	9
.173	1.85	.85		8.049	7.071	40.9	10.9	12
.203	1.58	.58		5.493	10.381	51.1	21.1	17
.226	1.42	.42		3.977	14.33	63.4	33.4	26
.219	1.46	.46		4.356	13.12	59.9	29.9	27
.214	1.50	.50		4.735	12.07	56.4	26.4	28

Now we guess at what might be suitable numbers for S and L. Before this, however, we ought to decide what would be a convenient measure of the "unit" of *s*. This unit ought to be chosen so that in looking up the cotangent we can use the most convenient part of the trigonometric table, which is that between angles of about 3 and 30 degrees. For this reason, in our case, 1000 words per Θ would be a convenient speed unit. Instead of 123, for example, we shall therefore write *s* equal to 0.123.

Successive Approximations

(1) S=0.30, L=10.0, N=14.8.
(2) S=0.30, L=19.0, N=31.7.
(3) S=0.32, L=19.0, N=28.4.
(4) S=0.32, L=20.0, N=30.0.
(5) S=0.32, L=17.2, N=25.5.

We reached the highest record in our experiment on June 23, as our record table here reproduced shows, with 226 words after having started on May 28 with the low record of 123 words. It is therefore a fairly good guess that the limit S will lie in the neighborhood of 300 words. It is more difficult to guess at L. Let us use for a first approximation L=10 and compute *n for only the first and the last experimental record.* (See Computation Table III.) We thus get for the seventh column, cotangent divided by *s*, the numbers 14.8 and 33.2. Choosing 14.8 as our first approximation of N, we have for the eighth column 0.0 and 13.2, instead of the experimental values 0 and 28.

Since the range of *n* obtained with the above values of S=0.30 and L=10.0 is too small, we try if L=19 will give a better approximation. In this case the constant quotient π divided by $(L-1)$ is not 20.0, as it was the last time, but only 10, as entered in the fourth column. The seventh column suggests to us to try N equal to 31.7. And the eighth column gives us then 0.0 and 35.1 instead of the true values 0 and 28.

This time the range of *n* turned out a little too large. We now try out what difference it will make if we leave L equal to 19, as we just used it, but take S a little larger, say, equal to 0.32. The eighth column then gives us 0.0 and 25.0 This time the range of *n* obtained with the larger S is smaller, but only a trifle smaller than the quantity we want. We might try to remedy this in the manner which we have already discovered, that is, by taking L still a little larger.

Let us, then, this time, try out what approximation we get by taking S equal to 0.32 and L equal to 20.0 The constant quotient used in the fourth column is then 9.47. Let us also, since we have already reached a considerable degree of approximation, compute *n* for a larger, but judiciously chosen number of the *s* values in the experimental record table. For example, the *s* values corresponding to the experimental *n* values 0, 3, 6, 9, 12, 17, 26, 27, 28 seem quite suitable.

The eighth column, if we choose N equal to 30.0, gives us as the result of the computation 0.0, 3.6, 4.9, etc. The second value is a little too large a number; the third, on the contrary, is too small. The fourth, 11.4, is again too large; 10.9 is too small; 21.1, 33.4 and 29.9 are too large; and 26.4 is too small. This already looks like a very admirable approximation.

Plotting this curve and marking in addition all the actual, experimental, *n* values corresponding to the *s* values, we obtain the curve and dots of Fig. III. The thin, smooth curve is the curve of the constants last selected. Comparing its bearing with the location of all, and not merely a few, of the experimental points, we are not yet satisfied with the approximation. We therefore try still further variations of the constants by small amounts. The broken and heavy curve of the same figure, whose constants are S = 0.32, L = 17.2 and N = 25.5, appears to be a good approximation to all the points combined. The last mentioned constants might therefore be used in the interpretation of the experiment, which, we recall, was not an experiment on learning at all. But this interpretation must be left to another article.

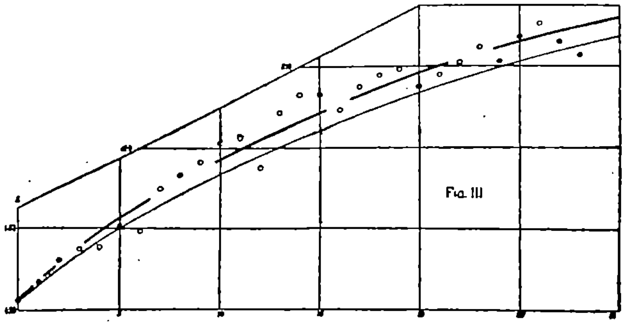


FIG. III

THE PHENOMENON OF 'MEANING'

By R. M. OGDEN, Cornell University

'Imageless thought' has apparently passed into the limbo of inert conceptions, but, stillborn though it was, it had a positive influence upon subsequent developments in systematic psychology. No one, I think, who surveys the field to-day can escape the conclusion that the controversy which arose over the admission of a 'thought-element' among the contents of consciousness was settled less upon its merits than by its absorption into a broader question whether, indeed, psychology has any conscious contents at all.

Into this newer controversy which is still being debated I have no wish to enter here, but I do suggest that the obligation placed upon psychologists to re-define their contents in scientific terms first became obvious after the inadequacy of *sensations*, *images*, and *feelings* was revealed by the introspective studies of Binet, Woodworth, and 'the Würzburg School'. It was this argument over the nature of conscious contents which more than anything else, I think, created a favorable moment for the appearance of *behaviorism*, and its denial of scientific validity to any and all these phenomena of consciousness. In short, I doubt if behaviorism could have arisen if the conception of mind as an aggregate of conscious entities had not itself seemed to be breaking down. Accordingly, in venturing to rake over the ashes of an extinguished fire I am moved by the thought that the question which was then raised has not been answered, but only set aside.

While the pendulum of psychological interest in America swung abruptly from a seemingly hopeless attempt to give a useful definition of mind in terms of conscious elements, to a radically opposite attempt to re-define psychology in terms of behavior, German psychology has been continuing its inquiry into the phenomena of meaning. And now that the veil of war-time obscurity has been raised we find ourselves confronted with a new German psychology, in many respects radically differing from the old, yet still intent upon a scientific pursuit of the phenomena of experience.

It is especially from one of these newer points of view—that of the *Gestalttheorie*—that I now invite you to assist in a post-

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[1] Some Experiments on the consciousness of Meaning. *Studies in Psychology*: *Titchener Commemorative Volume*, 1917, 79-120.

mortem of one of my own previous efforts to examine into the psychology of meaning[1].

Structural psychology formerly maintained that sensation was the chief if not the only element of consciousness. With this principle established, the inference was readily drawn that mind could be regarded as a summation of static entities held together by the gravitational influence of association by contiguity. This theory seemed to stand the test of the work on memory initiated by Ebbinghaus; and it was not until the thought-processes were in turn subjected to observation that a paucity of both sensory and imaginal material became evident. Instead of the traditional *sensation* and its counterpart the *image*, the observers of thought reported *whats* and *thats* and *ifs*, globular masses of interrelated tendencies, individual flashes of circumspection, and, indeed, a large assortment of vague and ill-defined phenomena, none of which bore any manifest likeness to the sensory, imaginal, and affective 'elements' our text-books had always taken such pains to describe for us.

When thought was thus discovered as a new star in the firmament, conservatives promptly denied the validity of the method of its discovery, and as promptly challenged the discoverers to define their new element in positive descriptive or attributive terms. This was not easy; for the thought-datum had no definite spatial or temporal setting, its intensity was doubtful, and its quality showed none of the systematic uniformities that had made the observation of sensation so engaging a task. Although the innovators themselves, I fancy, have never been quite ready to admit that their experiments were only pseudo-experiments, or that the data they observed and reported upon were but the crude manifestations of a totally unrefined methodology, still theirs was a losing fight, because they were unable to reduce 'imageless thought' to the scientific terms then current. Perhaps the method was faulty—though it seemed to be the same that had been used with such brilliant success in the observation of colors, tones, and varied types of imagery—but more seriously at fault, was the presupposition that conscious phenomena are reducible to elements. Such a presupposition worked well enough with sensory experience, where one could control the physical source, and thus reduce the content to manageable proportions. With meanings which spread all over the phenomenal content it was a wholly different matter; because apparently it was of the very nature of meanings to spread, and they had no physical counterpart which could be controlled. Thus the conservatives, fortifying themselves by reference to the exact sciences, insisted that there are no existential meanings. Is it not an axiom of the physicist that he is and must be totally unconcerned about the meaning of matter? And if the biologist has been somewhat less certain in his complete abandonment of teleology, he can still appear to maintain a scientific detachment by employing the term 'function.'

Facing the alternative of mechanism or teleology, we have been wont to assume that there can be but two horns to this dilemma; either an out-and-out mechanism, or 'an increasing purpose,' and that we most therefore grasp the one horn and avoid the other.

It is a special merit of the new *Gestalt*-psychology, as I see it, to have destroyed the terrors of this alternative[2]. By a bold declaration that there are no elemental conscious contents gathered together in 'bundles' through the agency of association, the investigation of mind has been turned to concrete commonsense experience as the only truly empirical point of departure. Instead of setting out either with a preconception of machine-parts and the ideals of an assembling-plant, or with the vague though much more human notion that all that is works together for the good of man; the attempt is at last being made to look experience in the face, and to observe what it is really like. In taking this departure we find ourselves in the way of discovering some very interesting things—as the numerous and exciting reports upon the phenomena of perception have already shown. But we do not find 'sensations', 'images', or 'affections', or even 'imageless thoughts', either singly or in combinations. We do, indeed, find sensory aspects of experience, and others which we may call imaginal, affective, and cognitive; but they do not fall into slots or specially prepared bins from which they can be taken and put together again to reproduce the experiences from which they were derived.

Neither are they *intentional* in the sense in which a universal purposiveness or teleology would suggest. On the contrary, these phenomena are as concrete and as capable of accurate description, as those one observes in a test-tube. They are not shot through with impending consequences, nor do they hark back to antecedents. Instead, they are immediate and definite phenomena set off in curious ways from the vaguer ground of consciousness upon which they are cast, and they possess an unique construction which is partly immanent or internal to themselves and partly dependent upon their setting as indicated by the nature of their 'contours' or margins.

Now the analysis of such a perceptual configuration is carried out in terms of its members, and a complex whole must, of course, be reduced to a smaller whole before its organization can be fully comprehended. Here we revert to the same means of control that we have always employed in experimental psychology. Ultimately, I believe, we must study the nature of all these configurations in terms of the same attributive aspects we formerly applied in our search for elements, adding, however, such other attributes as may come to light in the course of our investigations. Yet our search is no longer for elements, but for the underlying integration of attributes which makes compulsory the apprehension of concrete phenomenal wholes: things of various sorts, and movements of various sorts—and thoughts of various sorts.

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[2] *Cf.* K. Koffka, *Die Grundlagen der psychischen Entwicklung*, 1921, 73 ff.

When, therefore, I view some of my earlier experiments upon the consciousness of 'meaning',[3] undertaken while 'the Würzburg School' still flourished, I am struck, first of all, by a certain remoteness of the point of view I then held. But in rereading the reports of two rather extraordinary undergraduate observers, I am also impressed by the vivid freshness of many of the data they recorded in describing the meaning of a word. Indeed, I find in these reports certain *qualities* which, to me at least, possess evidential value as true descriptions of the phenomena of 'meaning,' for they strike me as far more realistic than any mere assemblage of elemental bits of mind-stuff ever could be. While the inferences then drawn from these data seem to me no longer tenable in their precise intention, I still find such phenomenological essentials as even a perverse elementalism could not entirely destroy, and it is upon these that I think it now possible to construct a more adequate interpretation.

The experiments to which I have reference consisted in series of words presented to the observer singly with the instruction to secure the word's meaning and to react in a predetermined manner as soon thereafter as possible. A report was called for, and the data obtained were later analyzed. The net results of these analyses were then summarized in a series of statements[4] to the effect that the meaning of a word is given as a *mental act of relating* in which the terms of the relation may be the stimulus-word and a second content, or the terms may both be contents other than the stimulus-word. What appeared to control the whole procedure was referred to as a 'dispositional matrix' called the *nucleus*. This nucleus, formed under the joint influence of the instruction and the stimulus-word was found to determine the range of association which the word might have. The contents emerging from the experience were therefore regarded as associates of the word, or of some aspect of the instruction, or of the surrounding circumstances. Since the 'mental act of relating' which gives meaning was assumed to be selective, the relevancy or irrelevancy of the emerging contents was described as being *felt*. A distinction was then drawn between felt relevancy or irrelevancy and a tendency towards *expression*, *illustration* and *example*, leading to the appearance of other associative contents, usually imaginal or verbal, which, by virtue of other relations established with the nuclear meaning, gave an illustrative precision to the thought. These illustrative expressions, however, were not found to be essential to the meaning, and were often felt by the observer to be inadequate to it. Yet being, as a rule, obvious, definite and concrete, they often seemed to make out the complete content of the experience. Nevertheless, the meaning-process could be readily distinguished by the observer from these supplemental processes of expression.

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[3] *Op. cit.*

[4] *Op. cit.*, 116 ff.

In my own defence I may be allowed to remark that my conclusions were expressly labeled 'tentative', for what strikes one immediately is the indescribable *act of relation* with its selective function. Otherwise the description would perhaps even yet pass muster. My present purpose, however, is to review it briefly from a more recent, and, it seems to me, a more significant point of view.

What does the observer have before him in an experiment of this kind? First of all, an instruction which somehow, as we should probably all admit, paves the way for his response. Let us call this a dispositional readiness for the appearance of certain mental phenomena. The first phenomenon of the experiment itself is the stimulus-word; this is perceived, and in its perception varied experiences emerge, simultaneously and in train.

If we proceed to analyze these phenomena from the point of view of elementalism we must look for entities, and they must appear in associative constellations or chains. But if we renounce elementalism and associationism along with it, then our phenomenal data take on quite a different aspect.

We said that the stimulus-word is perceived; to the older view this only meant that it was *sensed* or *felt*; the interpretation came afterwards, when, through association, it had revived some other content or contents than itself. According to the newer view, the word itself, as sensed, is at once the *nucleus* of the experience, the dispositional matrix being its neurological counterpart. Phenomenally, that is psychologically, the 'felt' word is already its meaning; for *there are no meaningless experiences*.

The error of previous interpretations lies, I believe, in neglecting the stimulus-word. As I reread the reports of my observers I find references again and again to pronunciation of the word, when it was presented orally, and to the 'looks of the word', when it was presented visually; even the association, as I then called it, of the word's sound or appearance with its later more completely expressed meaning was repeatedly signalized by the observer. What is a word, indeed, but a member of a complex situation in which it is employed? Without employment it has no existence, but once heard or seen, or otherwise apprehended, it is and must be accepted as part of a phenomenal whole.

The relation of the stimulus-word to a subsequent datum now seems to me a mistaken inference; for there really are no separable data in these experiments. The instruction which determines the configuration of the experienced phenomenon holds these focused upon the perceived word until the experiment is over. To be sure, the mind may wander from its task, and new centres may be focused in new perceptions, but so long as the instruction controls, the meaning is phenomenally the perception of the word: and all else is logic rather than psychology.

The relation and its terms, phenomenally considered, comprise a subject too large to be treated in this paper, but I should like to suggest two conceptions which I believe will be helpful in elucidating this problem. The first is the conception of the crude figure of any experience as it emerges in consciousness; this, I believe, is what my observers experienced in the first moments after the word was presented to them. This figure will be more or less crude according to a variety of circumstances—the ability of the observer, his previous experience with this word, his readiness for its reception, etc. All these condition the 'feel' of the 'nucleus.' But something more is necessary before the word is phenomenally defined; for the conscious figure of the nucleus must be bounded. With respect to the figure as centred or focused no logical relations can be established, because no datum is given to supply a second term. But let the figure be bounded in any way, and relations are at once possible with respect to various aspects of its boundary. Thus the meaning of a word *just so* is phenomenally the pattern of the whole experience that is given in the initial perception of the word. To this there accrue in short order details of integrative pattern, themselves partial integrations, which contrast with and set off one member from another. The nature of these detailed members is in part reproductive or imaginal, as we have been wont to say, but partial integrations are likewise manifest which, when later expanded in the observer's report, appear as if they were self-sufficient data or contents. Certain of these may lack all semblance to any sensory quality. In principle at least, partial integrations of *intensity*, *extensity*, and *protensity* are quite as possible as the integration of *quality* and *protensity* which seems to account for the pattern of seen-movement. In this sense 'imageless', or non-qualitative, 'thoughts' may be experienced as dependent members of a larger phenomenal construction, which nevertheless are capable of a certain isolation and of an accurate description in attributive terms. So far as I know, this isolation of non-qualitative phenomenal members has not yet been attempted, under exactly controlled experimental conditions, but I see no reason to question that it may be done.

It is, however, the definition of contours in the total moving pattern of thought that plays the greater part in the development of meaning, since it provides the basis for a description of the partial integrations to which we have referred. Without observing the margins of a figure, its structure remains incomplete and indefinite, and while the vague 'feel' of any integrated unit is already a meaning, its meaningfulness increases, that is, it can be subjected to a more refined logical treatment, only after its pattern is clarified by the observation of its margins or contours.

Rubin in his study of visually perceived figures[5] remarks that we know as yet little or nothing of the psychological nature of the 'contour,' but he goes on to show in a variety of interesting experiments that the internal structure of a perceived figure is not dependent upon a definite contour even though some 'margin' or 'edge' to the structure must invariably be given. So it is, I believe, with the meanings of words seen, heard, and articulated; they too are set in a complex matrix of 'feeling'—not primarily affective but sentient; and this 'feeling' constitutes the phenomenal character of the thought, its configuration being, in principle, always capable of reduction to attributive terms. Thus, though meanings are often ambiguous and complex, they have a quite simple origin in the perception of a systematic unit or whole which is conditioned by physical and biological data equally systematic in nature.

Which brings me to a final word upon the philosophy of this interpretation. Instead of being compelled to humanize or idealize the foundations of psychology and to assume *intentionalism* as the guiding principle of the unity of mind, we find here a phenomenal unity describable in terms of its own attributive characteristics without reference either to utility or to an *ego*. On the other hand the position taken is equally remote from that of supposing a *mechanism* which necessarily deals in entities either sensory or neural, fitted together by some unknown agency—mere chance, perhaps—acting and reacting upon each other as particles conditioned by blind and inscrutable forces.

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[5] E. Rubin, *Visuell wahrgenommene Figuren*, 1921, 106.

Instead, we have the conception of a system inherent in the data themselves, not somehow added either by arbitrary juxtaposition or by the Author of the Universe. And though for convenience it may seem desirable to hold apart the system of physics from the correlative system of conscious phenomena, and perhaps likewise an intervening system of biology, the inference is clear that these are but different ways of approaching the nature of one reality; and that although phenomena are observed and defined in different ways and by different means, according as we call ourselves physicists, chemists, physiologists, zoologists, or psychologists, the correlations we are able to establish between these fields are justified by the fact that one meaning underlies them all; mathematics and logic being but the tools man has devised to approximate this meaning in his attempt to describe its pattern.

THE CONSCIOUSNESS OF RELATIONS

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Several modern investigations of the conscious content have agreed that the consciousness of relatedness, whatever else it may be, is not an "elementary feeling" in the sense that the consciousness of blue or other sensations is an elementary feeling. Thus, Gleason[1] finds: "In all of our experiments with nine observers, a relational element was not once observed. The existential processes are the usual imagery, the kinaesthesis attending bodily movement, and organic pressures and warmth."

Gleason's conclusions may be criticized on the grounds that in the set of experiments she most emphasizes, namely those employing as stimuli pairs of words such as "nebula-solar system", the relation, the consciousness of which it was desired to investigate, was not present in fact between the stimuli or between the resultant images or "ideas," but must be invented by the observer. My experiments have shown that this invention is, in reality, a complicated psychological process. Thus:

> No. 41 Observer B. Non-visual type.
>
> Instructions: "Two objects will be named which commonly stand in familiar relation to one another. When you are aware of what this relation is you will indicate it by saying yes. You will be asked afterwards for a full report of your conscious process."
>
> Spoken stimulus: "cat—bird."
>
> Report: "Fore-period. Felt relaxed. Attention wandering. Vague kinaesthesis as of movements in the skull. I did not expect the stimulus just then, and on hearing it I sort of moved forward and drew up in the chair. This was on hearing the word *cat*. I don't remember any special effect of the word *bird*. My attention dropped and I had vague auditory and verbal kinaesthetic images of the lecture (a preceding lecture on zoology). Verbal image 'feline' stood out. The next thing I remember was feeling my strained position. Verbal image 'bird'? as if said with a rising inflection. Kinaesthesis in forehead meaning, I suppose, 'What family do birds belong to?' Verbal image 'avis.' The next I remember was 'coelenterate vertebrate.' These were strong verbal images. Then a feeling of comfortable warmth. Said 'Both vertebrates'." Time 3.2 sec.

Moreover, Gleason's instructions and my earlier ones (for example in the illustration above) may be criticized on the grounds that they call for a report of "relations". Gleason herself remarks on the difficulty her observers had in deciding what was a relation and what was not a relation between her stimuli, whether verbal or graphic; thus, one of her observers reports "two parallel lines" and yet is unable to say what is the relation between them. I also early found that very few of my observers had a clear idea of what a relation was. In fact, regardless of the fundamental part taken by relations in logic and philosophy,[2] the abstract idea of relations is very difficult. Still, this lack of familiarity with the idea of relatedness in the abstract may itself be taken as an indication that the feeling of relatedness is not as prominent a part of our consciouness as the 'feeling' of color for instance. Moreover, considering the excellent quality of the reports obtained by Gleason, it is hardly likely that, if there had been present any 'elementary feeling' of relatedness in general, even as vague as the kinaesthesis involved in the impulse to do some external act habitually reported by her observers, this elementary feeling would have escaped report, even if not recognized as that of relatedness. However, if the feeling of relatedness were as vague an impression as the kinaesthesis of intercerebral movement which occurs with certain observers in all difficult thought-transitions, or even if it were as vague as the imagery-kinaesthetic complexes into which Clark[3] analyses "mental attitudes," it is quite possible, as experiments with misleading instructions show, that it might be overlooked simply because the observer was not sure for what he was to look.

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[1] J. Gleason, this JOURNAL, 30, 1919, 1ff.

In my own experiments[4] I used for stimuli at various times:

1. Pictures of two objects in some relation: *e.g.*, a book on a table; a boy climbing a tree. (This is Gleason's method No. 1.)
2. The names of two simply related objects: *e.g.*, Baltimore-Washington; father-son. (Gleason's method No. 2.)
3. The rule of three: *e. g.*, red is to pink as violet is to lavender. (Woodworth[5].) This method was used both with and without the last term given.
4. Simple relatives: *e.g.*, ..is greater than..;....implies....
5. Partial sentences: *e.g.*, The telephone is standing on the ....My .... is my mother's sister.
6. Propositions in symbolic logic and mathematics: *e. g.*, $p \supset p \vee p$.

My instructions in almost all cases called for a reaction in the form of an appropriate sentence; *e.g.*, a judgment involving the relation. I did this, as I felt it needless to complicate the thought-processes of the observers by requiring them to report the relation *ex situ*, which is logically speaking not a relation but a term, and which to introspective analysis will be shown to be certainly different from the psychological correlate of the relations sought for.

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[2] F. R. Bichowsky, *J. of Phil.*, 14, 1919, 518; 17, 1921, 295.

[3] This JOURNAL, 22, 1911, 215.

[4] These experiments were begun in 1911 and for the major part were completed in 1916.

[5] R. S. Woodworth, in *Essays in Honor of Wm. James*, 1908, 285.

My methods, which were the usual ones, need not be described in detail here. I had no difficulty in getting very complete analyses. Yet even in the case of observers M and myself, with whom at first a strong prejudice existed favoring the discovery of a weak "feeling of relatedness", no such "feeling" was clearly observed, and in cases where it might be inferred or indeed was vaguely reported further analysis has shown that the results may be interpreted in a different manner.

V 14 Thus for observer V we find the fourth term of the proportion "red is to pink as violet is to......" reported as "lavender" immediately, 0.5 sec., without remembered intermediate processes. Yet the observer must go through an elaborate imagery: *e.g.*, Titchener color spindle, kinaesthetic placing of all four terms on the spindle, the discovery that lavender and pink are close together, that they are tints, in order to formulate the relation "pure colors to tints."

Logically it would seem that some feeling corresponding to relatedness in general is necessary to explain the right response, since the knowledge of what the relation was only came late in the process. Psychologically, however, this conclusion is by no means necessary, since many cases are known where a specific reaction occurs before it is possible to recognize the class, or name, under which the reaction falls. Moreover, repeated analyses of cases which differ from the above only in the fact that the first phase could be analyzed have failed to find any elementary feeling of relatedness in general.

If, therefore, there is an elementary feeling of relatedness it must be of extraordinary lack of clarity, since it would seem hardly likely, in view of the wide variety of methods used by Clark, Gleason and myself, that any experimental condition necessary to observe a vivid feeling of relatedness could have been overlooked[6].

An even more definite discussion can be made as to whether there is any complex of known elements, *e.g.*, any kinaesthetic or other imagery, to be found in all cases of relational experience.

My observers, like those of Clark, were able to report repeatedly imagery complexes corresponding to such "conscious attitudes" as "surprise". They were able also, in some cases at least, to report what may be taken as the imaginal complex for a particular relation; for instance, the relation "jumping" was for one observer (K No. 21) often accompanied by the kinaesthetic sensation as if the observer was flying through the air, even where the resulting conscious pattern was grotesque; *e.g.*, where the stimulus was the "cow jumped over the moon". Still, my observers could find no complex corresponding to relation in general, that is, no common "conscious attitude" in all cases of consciousness of relations, though it is difficult to see how it could have been overlooked if it existed.

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[6] My *Os* were not aware of the results of Clark and Gleason or of the work of the Cornell school in general.

Of what, then, does "relation consciousness" consist? If it is not sensation, elementary feeling, or imagery complex, it is obviously not to be found by methods of introspection which consider as significant only reports of sensations, elementary feelings and complexes of these elements. By rough analogy, the problem of describing conscious relations is similar to the logical problem of describing any objective related nexus. Logically, it is possible to attempt this by merely listing the elementary things of which the nexus is composed. Such an analytical description gives all that has objective being in the nexus, but misses entirely the relations. So classical introspection, which is after all a process of objectifying[7], is unable to discover the transitive non-substantial part of the conscious nexus. The only way in which relations appear in an introspective report is when they crop out in the form of some statement of meaning[8], interpretation or reference; *e.g.*, in such judgments as "The lines were parallel"; "This sensation inhibited the other"; "An image of the door".

Negatively, such relations are any part of the report of a conscious content not a mere statement of sensations or elementary feelings or a statement that can be analyzed completely into a complex of these units. Empirically, relations and other meanings appear quite distinct from the sensation, feeling, and imagery content of consciousness and seem to belong to a separate realm of psychic entities. Jacobson[9] has been one of the first to include such statements of meaning in the body of his introspective reports, and though I am inclined with Titchener[10] to think that it is difficult to find a sharp line of division, I shall adopt the practice.

As a matter of fact, my experience both as experimenter and as observer has shown that, while it is difficult at first for an observer who has been trained to attend only to the sensation content of consciousness to learn to report meanings, this is largely due to the setting up of inhibitions. These inhibitions can be overcome, however; and with untrained observers I have found, if anything, less difficulty in teaching them to report meaning and sensation content ("the processes"[11] in Jacobson's terminology) than to report sensation content alone. Indeed, I think all experimenters will agree that, even with the most highly sophisticated observer, it is difficult to get a pure "process" report; the instinct of the observer is towards reference of sensation.

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[7] R. Dodge, this JOURNAL, 23, 1912, 214; E. B. Titchener, *ibid.*, 23, 1912, 165, 427, 485.

[8] "Meaning": E. Jacobson, this JOURNAL, 22, 1911, 553. "Kundgabe": E. von Aster, *Z. f Psych.*, 49, 1908, 69. "Sprachlicher Ausdruck:" E. Dürr, *ibid.*, 49, 1908, 315.

[9] Jacobson, *op., cit.*

[10] E. B. Titchener, this JOURNAL, 23, 1912, 165. I am not clear whether Titchener means to depreciate the report of meanings in introspective accounts of conscious content on the ground that such reports meet the logical demands for an interconnected account rather than the psychological demands of the situation. In any event the requirement that psychology be a purely analytical science, *i.e.*, report only the units of which its phenomena are composed and not their interconnections, even though these are of a different type, appears to me as arbitrary as to require chemistry to report only what chemical elements compose its substances and omit an account of the laws that control their combination.

I did not find, however, that exact correlation of meaning with "process" that Jacobson seems to imply. The situation is somewhat more complicated than the mere existence of "two parallel accounts, in terms of process and in terms of meaning, of one and the same total experience." In the first place, I found that "meaning" was a rather vague word to describe that part of an account of consciousness which could not be considered a dscription of "process".

The parenthetical passages as they appear in Jacobson's reports are, in the majority of cases, mere cases of reference of sensation or image to an outer object; *e.g.*, "White (from the paper)", *op. cit.*, 557; "Visual image (of an apple)", 559; but there are other cases, especially where the parenthetical passage is a sentence, where we have to do not with direct reference to an outer world but with some sort of interconnection among feelings and sensations themselves; thus: "then mixed-up feeling, unpleasantness, nausea (all this meaning, I don't know what to do)."

In classifying my results, I have found that my *Os* report as meanings all the types of references given above, and also relations among sensations, feelings, images, and other meanings: *e. g.*, such relations as "this image seemed larger than that", "the image of the house sort of flowed out of that 'idea'." I was more particularly interested in this restricted class of meanings, namely, known relations; and my discussion will be restricted to this special case, though many of the facts and conclusions drawn can, I think, be immediately applied to "meanings" in general.

Typically, knowledge of relation between A and B appeared in connection with a change from image A to image B. Just when the knowledge of the relation appeared in the time-series, it is difficult to determine. Apparently, the "knowledge" did not always appear at the same place. It often occurred in the series: (image: A) (knowledge that A R B) (image: B) in the order given, but sometimes last, and rarely first.

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[11] "Process" has always seemed to me to be a most unhappy term to describe the sensation, image, feeling content of consciousness. One of the most characteristic features of these so-called "processes" is their possession of the quality of being a psychic stuff or substance, and of corresponding to, or seeming to be, the *thing* outside. I should be tempted therefore, to call them "substances", not "processes". To be sure, they are subject to change; but so are physical substances.

Thus C (visual type), a student of symbolic logic, reports on his mental process of discovering whether the relation of implication occurs between random logical propositions. "In all cases where I recognized implication immediately the image of B seemed to flow out of that of A often before I was even aware of seeing B". Again: "There seemed to be a kinaesthetic focusing on image B, though the awareness of implication did not seem to be the focusing or either image."

This is the simplest form. In other cases, more complicated steps were gone through. Thus V (visual) reports after the auditory stimulus "John jumped the creek"[12]: "I had an image (of a man in a track suit hurdling). The image (of the creek in the park) flashing up blotting out that (of the track meet). Then the image (of the man in the track suit) gradually reappeared (as jumping the park creek). (The thing was quite incongruous.)"

Very often, the transition connected with the meaning was not between images of the things (which were judged to have the relation), but between some extraneous images coming from the preliminary set, or between some modification of the images produced by the preliminary set[13].

Observer B was asked to report in the form of a sentence as fast as possible the relation between two figures to be shown. Foreperiod report: "I felt strain. Kinaesthetics of setting myself as at the start of a sprint". Stimulus: a man sitting on a standing horse. Main report: "I didn't get the meaning at first; then the image of a horse racing came to me; then I noted the old man on his back as in the picture and reported: 'The man is racing the horse'.......No. That didn't seem incongruous. Was it?"

In certain cases an actual inversion of the relation took place.

Observer B. Direction: "Give first significant sentence of the form A has some relation to B' that comes into your mind, on hearing the two words I will give you. Give an introspective report of the total thought content."

Fore-period. Report: relief of strain. Kinaesthesis of relaxing into the chair. Meaning forgotten. There seems to have been one, probably "That's easy." Wandering attention; *i.e.*, vague articulatory kinaesthesis, ending as if to say 'relation.' Sensation of warmth.

Stimulus: pot, 0.5 sec., lid. Main report: "Start; kinaesthesis of inhibited breathing; whispered "pot" and sort of automatically "is...... on stove;" I was groping. Strain present. Jerk as of pulling up in chair, associated with the meaning "That's wrong?" The question-mark seems to belong there. Nausea: articulatory kinaesthetics of saying 'pot is on?'....'on?'....No associated meaning. Then automatically I said to myself, 'lid.' There was pleasing sensation, and feeling of relaxations. "Thats it," I thought. Reaction: 'pot is on the lid.'" Time 2.4 sec.

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[12] In this report I have distinguished the "meanings" in accordance with Jacobson's convention by parenthesis. If one wishes to see the barrenness of pure "process" reports he should read the above report omitting the parenthetical passages.

[13] May, *Archives of Psych.*, 39, 1917; R. H. Wheeler and T. D. Cutsforth, this JOURNAL, 33, 1922, 361.

However, the most striking evidence for the transitive quality of known relations is the fact that my observers all recognized the transitiveness without previous suggestion. Various terms were used to express this. The images involved were said to have "led to", "carried over", "flowed into" the meaning. And *vice versa*, the meaning "was felt relative to", "caused", "was transitive to", "flowed into" the image. Sometimes the image was said to carry the meaning; but all our observers denied that the knowledge of a relation R (that is, the meaningful idea "A has the relation R to B") was the same as an image of A in the relation R to B. Thus the knowledge of the relation in the sentence "The cat is on the table" was carefully distinguished from the image "The cat on table," though this knowledge was associated with some transition of images often, though not always, involving this image in the chain. For one observer as a rule under certain types of stimulus the knowledge that the cat was on the table rarely involved the image of the cat on the table, but separate images of cat and then of table. This was almost invariably the case when the relation was not spatial.

In our best reports, the correspondence of known relations and transitions in sensation-image-content was so complete that it occurred to me that it might form a criterion for grading introspective accounts. I therefore, at periods of approximately two months, gave two of my *Os* the same material for introspection. The results for one *O* are given in Fig. 1. Crosses represent the number of relations reported, circles the

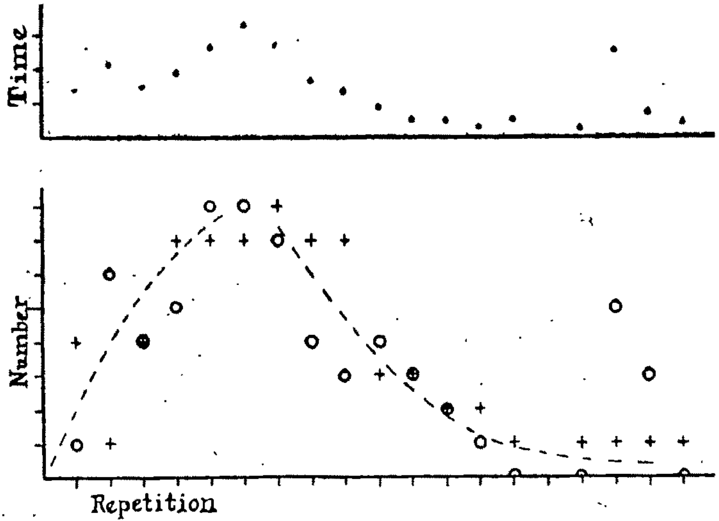

FIG. 1

Practice habit curve for repeated introspections; above: time of reaction; below: number of relations reported (+), number of images, etc. (o).

number of feelings and undifferentiated images, and images. The time of reaction is plotted in the supplementary figure. The *O* was practically untrained at the beginning of the test. There is shown here a period of growth of ability. The total number of known relations and "processes" shows a gradual increase, as shown by the dotted curve of average value; but the distribution of this number into two groups, relations and images, etc., is erratic, and seems to be due to varying focus of attention. At the maximum and during the remaining portion of the curve with the exception of two points, that is, during the period where the reaction was becoming habitual and not conscious, correlation of relation and images, etc., is very close. If anything, apparently the tendency is to lose images from consciousness before meanings. The exceptional two points near the end represent a case where special effort was put forth by the *O* to inhibit the effect of habit, slow up the reaction, and treat it as a new experiment. These results are included only because they show that it is possible in a certain measure to dissociate the effect of habit from an apparently well established habitual reaction. The correlation of time and number curve requires no comment.

The experiments with the second *O*, though not so complete, show the same general characteristics. The curve for decay shows the same form as would be expected from the general law of habit formation. However, the curve for learning shows a wide variation. The actual curve of ability in introspection may be considered as a resultant of a curve for learning (which shows much individual variation) and a curve for habitual decay (which curve is less subject to individual peculiarities). Perfect introspections, giving all the meanings and images that actually occur in a novel thought-process, could only be obtained on first trial, as after that habit-formation begins to blot out features of the original process. On the other hand, even with highly trained *Os*, the difficulty of attending and remembering is so great that practically many of the significant features of a novel thought-process are not noted or remembered on the first experiment with a new stimulus. It is plain, however, that reports which are to be used to correlate images, etc., with meanings should be taken from the maximum portion of the curve, that is, after several repetitions (at long intervals) of the stimulus.

Using such reports, I found not only that known relations as they appear in meanings show a transitive quality, *i.e.*, are always connected with some transition in the content-account, but also that they show other differences from images, sensations and feelings which are significant.

(1) The known relation has a sort of pseudo-independence from the images, sensations, etc., which it serves to connect, in that while it never occurs except in connection with some terms it is apparently independent of any particular term; for the same relation can hold between a wide variety of images, two relations may be identical, different, etc.

The exact nature of this pseudo-independence of known relations and terms, as well as the "universal" quality of known relations, which we are about to describe, can be most simply described by pointing out an analogy in the field of logic, namely, the pseudo-independence of logical relations and terms. A relation in logic is not an entity possessing separate existence; for by definition such an entity would be a term; yet it must be pseudo-ndependent of its terms in the sense that the same relation has significance when holding between different pairs of terms. Thus "A and B" represent the same relation as "C and B", namely, being taken together. Another way of saying the same thing is to say that, though relations are not terms, they must be capable of having a name. These results are accomplished in logic by treating any particular relation as a universal. Thus, a particular relation is defined by its most general connotation; it is that relation holding alike for all pairs of terms that in fact can stand in that relation. 'Jumping over' is thus the relation which holds between all pairs of terms such as the cat and the fence, the cow and the moon, which can conceivably stand in the relation jumper-jumped. Using a less technical analogy, a known relation may be conceived as a "direction" by which we go from image A to image B. "One hundred yards north-west" has no significance except from some particular point, yet it has significance when applied to almost any point on the earth's surface.

Thus our observers (who were not trained logicians) had difficulty in getting what *to them* were satisfactory accounts of the phenomena of knowing a relation. One remarked, after a long attempt to improve his introspective reports; "I don't know how it is, but the relation never seems to come without some sort of image, yet it seems to be independent of any particular image......I can't get it under attention. It just flashes out."

Titchener,[14] reexamining some of Jacobson's *Os*, asked them to give an account of the introspective differences between meanings and processes (our sensation, feeling, image content) and obtained a series of introspections which may well become classic. Both of his *Os* agree to the pseudo-independence and immediacy of consciousness of meaning, though they do not observe its transitive relation to images. I suspect this is in part due to the unfortunate connotation the word "processes" has. Thus D: "A mental complex....as it shows itself to introspection, rises, becomes more or less prominent, dies away......meanings are (in this sense) not transitory, but are perfectly definite and discrete things, distinct from one another." F: "I have never noticed, however, that meanings overlap or pass into one another; the meaning is, I think, always this or that at any particular moment."

D: "In particular (*a*) meanings are definite and arrange themselves easily in sequences, whereas it is difficult to be sure of the temporal order of processes......(*c*) Meanings go naturally into words; the phrasing taking care of itself. One has to be very careful of one's vocabulary in reporting processes....The statement of meaning is altogether independent and can be made without any thought of processes. I find, however, that even when I am trying to hold strictly to the attitude of reporting processes, I am tempted to refer to meanings in order to make the description complete and chronological. Where the statement of meaning (as in some of the new experiments) is given (reported) first, it offers itself as a skeleton on which I may reconstruct the process-account; it promises to give shape and organization to that account; and I cannot help making use of it. While therefore the introspective attitude is *analytical*, the meaning attitude is not analytical at all, but just a matter of course; the expression takes place immediately without hesitation as if automatically."

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[14] E. B. Titchener, this JOURNAL, 23, 1912, 180.

> F: "The meaning stands as a unit which cannot be analysed; it stands individual and irreducible... This "belonging together" on the side of consciousness and "single direction towards an end" on the side of meaning .... Processes move. They begin and grow and end. They have attributes; meanings are static,... they merely are. They do not possess attributes."

Another line of evidence pointing to the pseudo-independence of known relations is afforded by the failure of a series of experiments where the original purpose was to attempt to identify the knowledge of a particular relation between two things with the image of those two things in that relation. I attempted this by giving double stimuli, *e.g.* the sentence "The cat is on the table," and then, almost immediately, 0.1 to 1.0 sec. later, the picture of a cat on the table. With the comparatively untrained observers used (this particular experiment is best carried out with comparatively untrained observers), the occurrence of the first stimulus would often be completely forgotten. Yet the reaction was almost invariably a reproduction of the sentence used as the verbal stimulus. It was thought that since in these cases, as far as the observer knew, the reaction proceeded immediately from the visual stimulus, he would have no "Aufgabe" reason for distinguishing related images from the knowledge of the relation. Yet, typically, our comparatively untrained observers refused to confuse the recognition of a relation with the images in which it held. "I know the relation, I don't see it," one insisted. Again, "No, the relation wasn't the images. I just knew it."

Direct introspection of the mechanism of recognition is always difficult, and introspection on the recognition of relations (the author speaks feelingly) is more difficult than usual.

> The problem set was the recognition of a given relation between various pairs of objects, some of which did and some of which did not exhibit the relation in question; *e.g.*, for the relation 'square' the pairs of numbers 1, 1; 5, 2; 4, 2; 2, 4; etc, were given. If the relation was familiar, our *O*s, of course, had no difficulty in recognizing whether the pair exhibited the given relation or not. They must therefore in some manner carry over the idea of the relation given from the instruction period to the main period. Yet even in these cases the *O*s were emphatic in refusing to identify the meaning of the form of words often used to "carry" the meaning from fore to main period with the relation. Often the words which were to give the *O* the knowledge of the relation, *e.g.*, "is square of" were merely repeated as nonsense phrases to which meaning attached itself only when definite terms were presented. "Thus, when 3 and 4 were presented I knew immediately that 3 was not the square of 4. I recognized that 4 was the square of 2 as soon as these were presented."

More experienced observers even had difficulty here, but the results obtained are in harmony with the observations of Titchener's observers that recognition of a particular relation is immediate, in the sense that there is nothing in the consciousness of a particular relation which corresponds to the several processes involved in recognizing a conscious image as that, say, of an apple. This, of course, qualitatively, is a consequence of the fact that no new sensation elements corresponding to relatedness are to be detected in cases of consciousness that a given relation holds. However, it also appears, from what little experimental evidence we had on the actual time of recognition, that in some cases the presence of a relation between two objects actually cut down the time necessary to recognize the two objects, not presented as related.

As far as the time necessary to recognize words *versus* sentences or letters *versus* sentences is concerned, this result has been established by several other experimenters. In our cases, where the stimuli were objects presented in familar relations, the results are not so certain, but appear to be significant at least of the short time necessary to recognize relations *in situ* compared with the relatively long time necessary to go through the steps sensation, image, meaning. If the decrease of time for the recognition of A in relation R to B, in comparison with the time for the recognition of A and B separately is proved for any large number of simply related pairs, this will show not only the absence of any new physiological or psychological steps in the recognition of 144 as the square of 12, as contrasted with recognizing 144 and 12 separately, but also the actual existence in such separable perceptions of some time-consuming process (inhibitions, most likely) not present in judgment.

Our best trained observers added to the analysis of meanings given in the reports of Titchener's observers the observation that the particular relation always had the quality of a universal.

Thus: "When I try to catch the relation (north-westness) as apart from the particular places given, or try to find what I mean by north-westness, that is, when there is a question as to whether the relation between the two places given is north-west, or when I focus on north-westness, I always get an image not of any two particular places one of which is north-west of the other, but of a map of U. S. On this map I locate the places, then note their relative position. It is during this process that I become aware that one is north-west of the other". Again "North-west in the abstract, as far as it suggests an image to me, suggests a series of parallel lines drawn on a map of the United States." Another *O* (B) reports that consciousness that A is north-west of B "comes during the placing of A and B on a vector field with arrows pointing to the upper right hand corner." Another, V, remarks: "I seem to be standing in Washington (one of the places given), looking up 16th St. Baltimore is in front and to the right. It was during the development of this image that I became aware that Baltimore is north-west of Washington," Other *O*s all reported this last fact, namely, that it was during the development of the image that recognition "meant" a type of relation come. This may be taken as suggesting that the meaning of images, like the consciousness of relations, is relational in character. The image, however, is not identified with the relation which it is said to "mean", "carry", "suggest" or illustrate. In this respect the image which carries relational meaning is quantitatively, though perhaps not qualitatively, different from the sensed image of a thing which, at least to the same order of introspective *finesse*, is considered to be the thing as perceived. Thus observers will report seeing an apple, meaning thereby having the apple-image of a particular, but will not report seeing the relation north-west or having the image of a particular north-westness. What they do report is: "I have the image which, when particular points are supplied, is associated with my knowing the direction between those points. Until the points are given it is hard to focus and doesn't mean anything clearly."

Our observer trained in symbolic logic recognized the situation right away. "Why", he said "the particular relation has to me always the quality of a universal. It is carried by an image in the same way that goodness, all men, geometrical triangles are carried......When I talk in logic about all men being mortal I don't have an image of a row of men as B (one of the observers) says he does, but of a particular man. Just now it is of a tawny man, the particular image probably being suggested by the remark of Berkeley on this subject which was in my mind. The image is always of one man, not necessarily always the same one, though. The image isn't identified with the idea of men in general. It simply acts as a peg on which to hang it. So with the idea of a particular relation abstracted from any special term, as when I recognize the relation. It does seem rather strange to talk of a particular relation like north-west as a universal, doesn't it?"

This suggestion of our observer seems to give us both the most reasonable terminology in which to put our facts and also, by suggesting the identity in type between images carrying "universals" and those carrying relations, to simplify greatly psychological description. Thus, it accounts for the puzzling fact that in a large proportion of cases no images other than fugitive and incidental images connected with the fore-period[15] rather than with the stimulus used occurred, but rather a definition with the images appropriate to it.

Thus C once reported as the image for the relation of implication (indicated by Whitehead and Russell by the symbol ⊃) the image—AvB, which is Whitehead and Russell's definition of that symbol. So B: "The word 'the square of' at first had no meaning......Then came the meaning X times X, with the image X×X. The images were very difficult to recall; the meanings were, however, very vivid."

The hypothesis also accounts for the somewhat complicated introspective phenomena reported in experiments involving transposition of relations from one set of images to another, which cannot be reported in full here.

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[15] These may be identified by means of the accompanying meanings This is one of the advantages of reporting meanings along with images.

Making this hypothesis, the results of this paper may be now summed up very simply. *Consciousness of two terms in relation involves the existence of a relation between two psychic entities (images, sensations, etc.). The psychic, and presumably the physiological, correlate of the known relation is a relation.* There is no new psychic entity corresponding to relation, in the sense that there is a new psychic stuff (the image, sensation, etc.) corresponding to a thing. This position is perhaps what would be expected from our ordinary observations on relations as logical entities, but psychologically it is unexpected or at least was unexpected by the author; for by analogy it was thought that knowledge of relations would be *via* image in the same way as knowledge of things. It, however, produces some simplification in our treatment of psychic content.

THE EFFECT OF INTENSITY OF ILLUMINATION ON THE ACUITY OF THE NORMAL EYE AND EYES SLIGHTLY DEFECTIVE AS TO REFRACTION

By C. E. Ferree and Gertrude Rand, Bryn Mawr College

Considered with reference to the eye there are three factors in acuity or the power of the eye to see clearly,—the resolving power of the refracting media, the space discrimination of the retina, and the sensitivity of the retina to light. To put it in another way, there are the resolving power of the refracting media or the power to form clear images on the retina, and the resolving power of the retina or the power to discriminate detail in the physical image formed. The explanation of the effect of increase of intensity of light on the power to see clearly is to be found, of course, in terms of the resolving power of the retina, not of the refracting media. The purpose of this study has been in part to determine the effect of increase of intensity of illumination on the acuity of the normal eye and to compare this effect with that obtained with the same eyes made slightly defective as to refraction.

The Effect of Intensity of Light on the Acuity of the Normal Eye and Eyes Slightly Defective as to Refraction

In the series of experiments on the normal eye four observers were used, and the acuity was determined at 0.001, 0.005, 0.01, 0.015, 0.02, 0.05, 0.1, 0.2, 0.4, 1, 2, 3, 5, 10, and 20 foot-candles of light normal to the test object. The eye was adapted to each illumination by a 30 min. practice series with proper rest periods. The broken circle (the international test object) mounted on a rotating graduated dial was used as test object. In making the observation all that was required of the observer was to indicate the direction in which the opening pointed. The judgment on which the estimate of acuity was based was thus reduced to very simple terms, and an objective check was had on its correctness. In the final series of determinations this opening was turned in haphazard order right, left, up, down, and to the four 45° positions; and a correct judgment was required in five out of the eight positions. The breadth of the opening was measured on a micrometer comparator and the visual angle computed. The coefficient of reflection of the test surface was 85%. The preexposure and surrounding fields were made in each case as nearly as possible of the same brightness as the test surface. An exposure of 1 sec. was allowed for each judgment. The work was done under artificial illumination, the light of a frosted type-B Mazda lamp. The angle of incidence of the light on the test surface was kept constant throughout the experiments. Constancy of position of the observer's eye was secured by biting a mouthboard in which the impression of his teeth had been previously made and hardened in wax.

From 0.001 to 0.1 foot-candle the minimum visual angle changed from 7.143 to 1.213 minutes of arc, a gain of 489% in acuity; from 0.1 to 1 foot-candle it changed from 1.213 to 0.741 minutes of arc, a gain of 63.7% in acuity; from 1 to 5 foot-candles it changed from 0.741 to 0.516 minute of arc, a gain of 43.6% in acuity; and from 5 to 20 foot-candles, it changed from 0.516 to 0.477 minute of arc, a gain of 8.2% in acuity. A curve showing the average results for the four observers is given in Fig. 1. In the table of data for Fig. 1 it will be noted that an acuity of 1, 6/6 or 20/20 was attained at an illumination between 0.1 and 0.2 foot-candle; while at from 5 to 10 foot-candles, the range of intensity perhaps most frequently employed in laboratory testing and in office and clinic work, the acuity fell between 1.94 and 1.97.

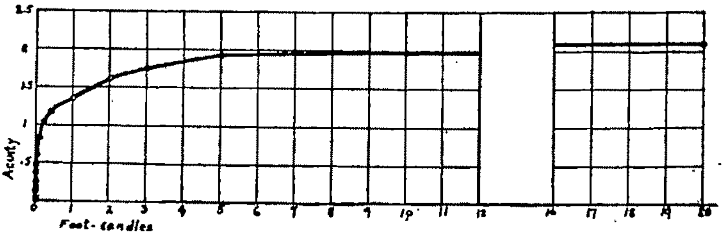

FIG. 1. Showing the effect of increase of intensity of light on acuity (4 observers). Acuity is plotted against foot-candles of light normal to the surface of the test-object.

In the experiments on eyes slightly defective as to refraction the most practised observer was selected from the former group, and a comparison was made of the effect of change of intensity on this eye normal and made slightly astigmatic in the following ways,— by a +0.12 diopter cylinder; a +0.25 diopter cylinder; a +0.25 diopter cylinder with a correcting cylinder 5° off axis; and a +0.25 diopter cylinder with a correcting cylinder 10° off axis. Artificial astigmatisms were chosen for this work in order that we might know the exact amount and location of the defect, and might have a comparison of the effect on the same eye in the normal and defective condition. They were not chosen with the belief that they are the exact functional equivalent of the natural astigmatism. We are too strongly impressed with the possibility that the astigmatic eye may progressively acquire some power to compensate for its defect to be of this opinion. The eye was pre-sensitized by 30 min. of adaptation to each illumination. The brightness of the preexposure and surrounding fields was the same as that of the test surface. The coefficient of reflection of the test surface was 85%. The intensities of illumination were 0.015, 0.05, 0.10, 0.25, 0.50, 1, 1.50, 2, 4, 6, 8, 12 and 36 foot-candles normal to the test surface. The results are shown in Fig. 2A and Fig. 2B. In Fig. 2A acuity is plotted against foot-candles and in Fig. 2B percentage-gain in acuity with increase of illumination is plotted against foot-candles. A point of particular significance to be noted in these

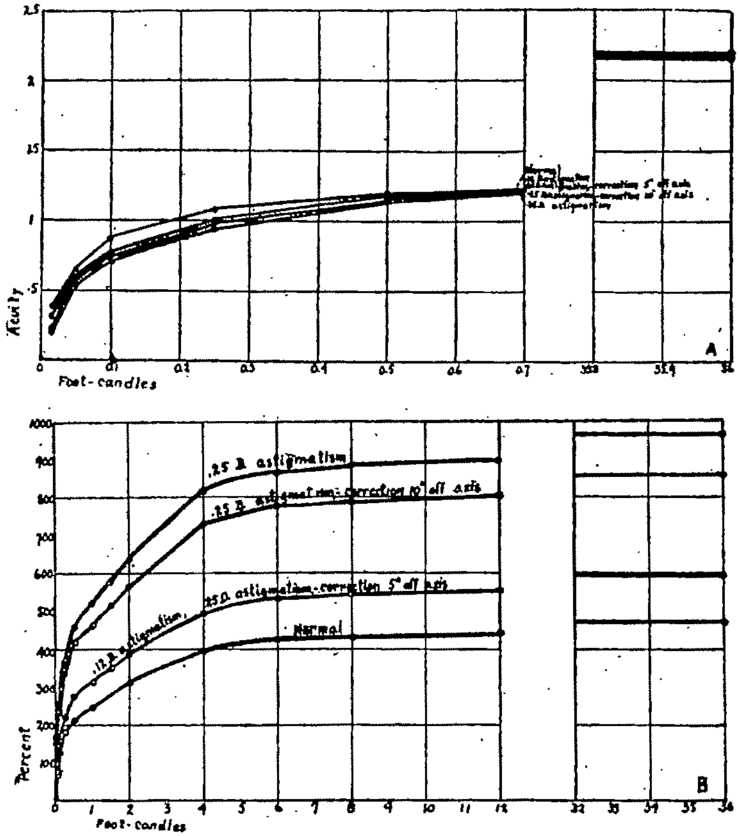

FIG. 2. Showing the effect of increase of intensity of light on acuity for eye with normal refraction and same eye made slightly astigmatic: A, acuity plotted against foot-candles; B, percentage-gain in acuity plotted against foot-candles.

data is that errors of refraction of these magnitudes give a scarcely detectable difference in acuity at the higher illuminations ordinarily used in the laboratory, office and clinic; while at the lower illuminations, around 0.1 to 0.2 foot-candle, they give differences which are readily detectable. Among other things these results indicate why eyes with uncorrected or imperfectly corrected errors of refraction require more light as a comfortable and efficient working minimum than normal eyes; and why corrections and eyes apparently good enough for the higher illuminations are not good enough for work at the lower illuminations. They also raise an important question as to what is the most favorable intensity for the illumination of test charts.

The Intensity of Illumination of Test Charts

The question frequently comes before standardizing committees: At what intensity of illumination should acuity be tested? Our answer would be that it depends for what purpose the test is to be made. There are three obvious applications of acuity testing: vocational selection, diagnosis, and hygiene or welfare of the eye.

In the rating of eyes as to fitness for vocations, the test should be made as nearly as possible at the illumination usually employed in the vocation in question. The study of even a small number of cases shows that the eye can not be given the same relative rating as to acuity at different intensities of illumination. For example, experience has shown in the Navy that only 25 to 30% of the men accepted for the service on the basis of the conventional acuity test at the higher illuminations are able to qualify for the lookout work at night on the bridge of the battleships. Further, in a test of 61 observers made by us, all under 28 years of age and rating 6/4 acuity by the conventional test with 5 foot-candles of light on the test chart, 13% rated below 6/6 at 0.55 foot-candle of light and 33% below 6/6 at 0.2 foot-candle. The acuity of the remainder was 6/6 or better at these illuminations. If speed in the use of the eye at low illumination be added to the requirement, the scatter is very much greater still. The amount of time required just to discriminate 1 min. visual angle in this group of observers, who were all put in the same class by the acuity test at the high illumination, covered a range from slowest to fastest of 1333% at 0.55 foot-candle and of 1443% at 0.2 foot-candle. It is quite obvious that any attempt to rate eyes for vocational purposes at only one or even one order of intensity of illumination is based on a lack of knowledge of the differential effect for different eyes of intensity of illumination on the power of the eye to see clearly.

All will agree, we suppose, that the object in diagnosis is to give the test under the conditions providing the maximum sensitivity for detecting errors in refraction. A glance at the curves given in Figg. 2A and 2B is sufficient to show that this maximum degree of sensitivity is not obtained at the higher illuminations. For the small uncorrected astigmatic errors represented in these charts the difference in acuity is scarcely detectable at the higher illuminations, but readily detectable at the lower, particularly around 0.1 to 0.2 foot-candle of light. The reason for this is not hard to understand. The details in the slightly blurred astigmatic image can be discriminated at high illumination, but not at low, because of the beneficial effect of increase of intensity of illumination on the resolving power of the retina. This increase in diagnostic sensitivity with decrease of intensity of illumination was further directly tested out as follows. Low artificial astigmatisms were made and corrected. Starting with the proper placement of the correcting cylinder, the axis was shifted from its position by graded changes, ascending and descending series, until the judgment of just noticeable difference in the clearness of the letter B subtending a visual angle of 5 min. was made. This was done at 15, 10, 5, 3, 0.46 and 0.25 foot-candles of light on the test card. The tests were made very carefully. Seven concordant judgments out of ten were accepted as the criterion of just noticeably different in any one set of trials. The results are shown in Table 1.

TABLE I.

Showing number of degrees at which cylinder must be placed off axis to give just noticeable difference of clearness of test-object. Results are based on 7 correct judgments out of 10.

		Degrees cylinder must be placed off axis to give just noticeable difference of clearness	
Observer	Astigmatism	High Illumination (Average of results at 5 and 10 f.c.)	Low Illumination (Average of results at 0.25 and 0.46 f.c.)
H	0.25 cyl. ax.90°	15.0	2.5
B	0.25 cyl. ax.90°	13.5	5.0
L	0.25 cyl. ax.180°	8.5	6.5
S	0.37 cyl. ax. 180°	13.5	4.0
C	0.50 cyl. ax.90°	20.3	9.5
Bs	0.75 cyl. ax.90°	9.0	5.0

In any hygiene or welfare test of the favorableness of working conditions for the eye, the tests should also be made at more than one intensity of illumination. Conditions which are apparently acceptable at the higher illuminations are often far from equally acceptable at the lower illuminations. In all cases it will be found, too, that the sensitivity of the acuity test, whether the purpose be vocational, diagnostic or hygienic, is very greatly enhanced when the procedure is made to include speed, power to sustain, and accuracy, instead of accuracy alone, as is the case in the conventional method of testing acuity.

Data for Fig. 1

Intensity (foot-candles)	Acuity
0.001	0.140
0.005	0.2075
0.010	0.2655
0.015	0.381
0.02	0.483
0.05	0.615
0.10	0.8245
0.20	1.028
0.40	1.158
1.00	1.350
2.00	1.611
3.00	1.743
5.00	1.9385
10.00	1.973
20.00	2.097

Data for Fig. 2 A
Acuity

Intensity (f.c.)	Normal	.12 D. astig.	.22 D. astig.— cor. 5° off axis	.25 D. astig.— cor. 10° off axis	.25 D. astig.
0.015	0.386	0.316	0.316	0.226	0.203
0.050	0.649	0.598	0.596	0.571	0.531
0.10	0.877	0.775	0.775	0.754	0.719
0.25	1.08	1.012	1.004	0.985	0.941
0.50	1.20	1.184	1.184	1.168	1.132
1.00	1.339	1.300	1.294	1.268	1.258
1.50	1.464	1.424	1.424	1.389	1.377
2.00	1.587	1.548	1.554	1.509	1.496
4.00	1.918	1.886	1.895	1.876	1.865
6.00	2.033	1.998	2.012	1.978	1.955
8.00	2.044	2.030	2.030	2.009	1.992
12.00	2.087	2.065	2.065	2.040	2.022
36.00	2.200	2.190	2.190	2.167	2.158

Data for Fig. 2 B
Percent

Intensity (f.c.)	Normal	.12 D. astig.	.25 D. astig.— cor. 5° off axis	.25 D. astig.— cor. 10° off axis	.25 D. astig.
0.015	......	......	......	......	......
0.050	68.13	89.24	88.61	152.65	161.58
0.10	127.20	145.25	145.25	233.63	254.18
0.25	179.79	220.25	217.72	335.84	363.55
0.50	210.88	274.68	274.68	416.81	457.64
1.00	246.89	311.39	309.49	461.06	519.70
1.50	279.27	350.63	350.63	514.60	578.33
2.00	311.14	389.87	391.14	567.70	636.95
4.00	396.89	496.84	499.68	730.09	818.72
6.00	426.68	532.28	536.71	775.22	863.05
8.00	429.53	542.41	542.41	788.94	881.28
12.00	440.67	553.48	553.48	802.66	896.06
36.00	469.95	593.04	593.04	858.85	963.05

AUDITORY AND TACTUAL PERCEPTIONS: THE RÔLE OF THE IMAGE

By A. Sophie Rogers, Ohio State University

I. *Problem and Method*

In a previous investigation[1] it became evident that the simple image plays a very important as well as a specific rôle in visual perceptions. In the present series of experiments an elaboration of this problem was carried over to auditory and tactual perceptions. The chief purpose of the experiments was to determine the particular and fundamental relations existing between imagery and perception. These interrelationships fall into three subdivisions: (1) the kinds of images present in auditory and tactual perceptions; (2) the function of the image; and (3) the relation of clearness to the image.

The procedure followed was to start with stimuli as simple and meaningless as possible, and then to proceed with more and more complicated ones. For arousing the auditory perceptions the Wundt sound-hammer was used first for a series of experiments, then a tuning-fork of 256 vs., and lastly a combination of sound-hammer and tuning-fork. The sound-hammer fell five mm. The observer was given 50 stimuli in each series, making a total of 150 reports for each person.

The Wundt sound-hammer was released through dry cells by closing one key of a three-way switch, which released either the sound-hammer alone, or the hammer and fork, or the fork alone, as desired. The striking of the fork and the timing of the duration of its sounding were controlled through the direct current. The tuning-fork was set into vibration by a felt hammer which was drawn down rather forcibly against a magnet when the proper key of the three-way switch was closed. For damping the fork, a "fall away" was constructed, which was drawn down to a magnet simultaneously with the striking hammer. This "fall away" released a timer equipped with a plunger, which lowered into an oil cup. As the plunger lowered, a contact-wire descended into a cup of mercury, thereby completing the circuit through a felt damper, which was drawn down to a magnet and thus checked the vibrations of the fork. For sounding both fork and hammer the middle key of the three-way switch was closed. The intensity of the sound of the fork was controlled by placing two Mazda bulbs, 60 and 40 watt, and a 40 watt carbon bulb in the circuit. All magnets and points of friction were covered with felt to eliminate undesired sounds. *E* sat at the front of the table, where she could operate the apparatus. *O* sat on the opposite side, behind a high upright cardboard, with his eyes closed during the time of each experiment.

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[1] A. S. Rogers, An analytic study of visual perceptions, this Journal, 28, 1917, 519.

For the tactual perceptions 25 stimuli[1] of varying degrees of resistance were employed. The same series in the same order was presented three times to each *O* by means of a gravity tachistoscope, adjusted to stimulate *O* for approximately 0.5 sec.

The tachistoscope was provided with an arm-rest and brackets so that the third finger was exposed comfortably for contact. The carriers for the stimuli were of three sizes, 1 x 0.5, 1 x 0.75, and 1 x 1 in. The constant factor of 1 in. represents the length of the stimulus, and considered with the rate of fall of the apparatus determines the duration of the exposure. The series was presented first with the smallest width, secondly with the medium, and thirdly with the greatest. This gives a total of 75 tactual exposures for each *O*.

Throughout the experiments, the following instructions were given to *O*:

"At the 'ready' signal, close your eyes. Two seconds later a sound will be heard (or: a pressure stimulus will be felt). Take the experience passively until a distraction stimulus 'now' is given.

Give a complete account of the experience in terms of:

1. Sensations, images, and affections. Indicate the temporal sequence of the processes.

2. Describe the clearness of the processes as clear, moderately clear, or obscure.

3. Give all meanings in parallel with the processes."

Three *Os*[2] served throughout the experiments. Each reported upon the same number of stimulations, which should be remembered when comparisons are made in the discussion of the results. Before starting upon the main experiments, the *Os* were given preliminary training.

II. Results

A. The kind of image present in auditory and tactual perceptions. This phase of the problem presents three aspects: viz., (1) the similarity and difference in kind of image in auditory and tactual perceptions; (2) the difference in kind and frequency with the increasing complexity of the stimulus; and (3) any variance of imagery shown by different *Os*.

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[1] Coarse crinoline; chamois; fine kid; challis; wool whip-cord; brilliantine; heavy satin; coarse ratiné; soft, silk crepe-metior; grosgrain ribbon; fine, tightly woven crinoline; silk netting; organdie; light satin; moray ribbon; chiffon velvet; heavy muslin; coarse sepia paper; soft, rough sepia paper; hard, rough sepia paper; coarse sandpaper; fine sandpaper; velours; bond paper; glazed shellac paper.

[2] The writer is indebted to Louise Ackerman (Senior), Chloie D. McGlinchay (Senior), and J. E. Svoboda (Junior), for their time and patience throughout the experiments.

TABLE I

St.		INDIRECTLY AROUSED PROCESSES					PERCENTAGES				
		Images				*Sensations*	*Images*				*Sns.*
		Vis.	*Aud.*	*Tact.*	*Total*		*Vis.*	*Aud.*	*Tact.*	*Total*	
H	No.	131	43	18	192	150	38	13	5	56	44
F		127	98	2	227	220	29	22	.4	51.4	49
H & F		156	110		266	271	29	21		50	50
H	%	10	3	2	15	11					
F		10	7		17	17					
H & F		12	8		20	20					
	Total	414	251	20	685	641					
	%	32	18	2	52	48					
0.50 in.	No.	68	2	28	98	153	27	.8	11	39	61
0.75 in.		74		32	106	168	27		12	39	61
1.00 in.		74		33	107	170	27		12	39	61
0.50 in.	%	8	.2	3	11.2	19					
0.75 in.		9		4	13	21					
1.00 in.		9		4	13	21					
	Total	216	2	93	311	491					
	%	27	.2	12	39.2	61					

Reference to Table I shows that visual, auditory, and tactual images form the imaginal constituency of both auditory and tactual perceptions. In all this series of experiments but four instances of kinaesthetic imagery were reported. The writer has not considered them for two reasons; first, because their infrequency would not materially modify results or substantiate their existence; and secondly, because there is some doubt about their imaginal character. If we compare the imaginal content of visual,[4] auditory, and tactual perceptions, we find a very decided increase in the kind of imagery corresponding with the sense department stimulated; *i.e.*, when the ear is stimulated, auditory images form 37% of all images, while the same class of images constitute but 2% of the imaginal content in visual perceptions; and when the finger is excited by tactual stimuli, there is a greater percentage (30) of tactual images than in either visual (2) or auditory (3) perceptions.

These facts call forth the old discussion of ideational type. The results of these data point to the conclusion that the kind of image depends more upon the nature of the stimulus and the direction of the attention of the *O* than upon the fact that individuals represent imaginal types. The first part bears out Meumann's point of view[5]: viz., that the type of imagery depends upon the kind of material.

The numerical relations are expressed in the following table. Table II shows the percentage that each kind of imagery constitutes of the total imaginal content within any given type of perception.

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[4] The data for the visual perceptions are compiled from the imaginal content recorded in Table I. of the article referred to above.

[5] E. Meumann, *Ueber Oekonomie u. Technik des Lernens*, 1903. *Vorlesungen z. Einführung in die experimentelle Pädagogik*, 1914. Vorlesung 14.

TABLE II.

Sense Dept. Excited	Frequency of Images Expressed in %			
	Visual	Auditory	Tactual	Kinaesthetic
Visual	76	2	2	20
Auditory	60	37	3	
Tactual	69	0.6	30	

These figures also indicate that there is but a slight increase is visual imagery (76%) in visual perceptions over the frequency of the same elements in auditory (60%) and tactual (69%) perceptions. The large percentage of visual imagery in these different kinds of perceptions may be explained by the predominance of attention to visual factors in practically all experiences. Furthermore the visual perceptions display a high frequency of kinaesthetic imagery (20%), while the auditory and tactual present less than 0.1% frequency, but 4 instances in 600 perceptions. This is probably accounted for by the fact that there would be a greater tendency to recall motor concomitants in visual experiences than in auditory and tactual, because of the greater emphasis which is placed upon the former, and the greater frequency of reactions to visual stimuli.

Referring to Table I again, we find, first, that visual images when considered from the aspect of frequency play an almost equally important rôle in auditory and tactual perceptions,—the percentage being practically the same in all series (see column 8 of Table I), with the exception of the series with the hammer, where the percentage is somewhat higher (38). The reason for this may lie in the fact that the sound-hammer represented the most meaningless stimulus employed in the experiments, and that more visual images were called forth in an attempt to satisfy the doubt existing in the minds of the *O*s with respect to the nature of the stimulus. Again, auditory images play about the same part in tactual experiences that tactual images do in the auditory. Secondly, the indirectly aroused sensations are approximately equally frequent in all auditory perceptions and in the three series of tactual experiences, but show a somewhat higher frequency (11 to 17%) in the tactual than in the auditory (see column 12 of Table I).

It is interesting to note that the indirectly aroused sensations in visual perceptions present approximately the same ratio to the imaginal components (47 to 53%) as they do in auditory perceptions (48 to 52%). While the imaginal elements show a greater frequency than the general organic processes in vision and audition, the latter have a much greater percentage (61%) than the images (39%) in tactual perceptions. This seems to indicate that experiences which are more infrequently attended to, when brought to the foreground, involve more motor factors in interpreting and orientating the object.

Table I further indicates the influence of the complexity of the stimulus upon the kind and frequency of the image. As the auditory stimulus varies from the almost "meaningless" noise of the sound-hammer to the familiar sound of a musical tone, the auditory images vary from 43 to 98, or one-third to three-fourths of the frequency of the visual images. When the stimulus is again modified by combining the noise and the tone, the ratio to the visual components is approximately the same as for the tone alone. This seems to point to the tendency of an individual to attend to the more familiar stimulus and to ignore the unfamiliar whenever he meets with the two simultaneously. This fact is confirmed by the introspections of the *O*s. When the fork and hammer were sounded together, the images pertaining to the experience were more frequently related to the tone than to the noise. The complication of the tactual stimuli by increasing the width and thereby increasing the amount of surface stimulated, and thus enhancing the possibility of accuracy of perception, did not present the results found in the case of the auditory perceptions. Tactual stimuli aroused more tactual images than did the auditory stimuli, but the widest stimulus aroused no greater number than the narrowest. In the tactual perceptions, the tactual imaginal factors display about one-third the frequency of the visual elements in each series.

The third part of this phase of the problem to be considered is the variance of imagery with different observers. Table III demonstrates the individual characteristics very plainly.

These distributions show a large percentage of visual images for all *O*s, with a predominance in the cases of M and S. A and M both reported a large frequency of auditory images with auditory stimuli and of tactual images with tactual stimuli. A is a highly trained musician, and one would expect her to experience more auditory images; but it is interesting to see that M, who is not a musician, reported as high a percentage (32%) of auditory images as A (31%). S reported practically no images other than visual. This might be explained by the fact that S attributed a rather definite meaning almost at once to the sound or pressure. There seemed to be lacking that struggling and searching for significance which the other *O*s reported. There seemed to be more "worry" about what it was or meant on the part of A and M, which was correlated with a tendency to ideate the perception.

The following conclusions may be drawn from this part of our study:

(1) In auditory and tactual perceptions, visual, auditory, and tactual images are the dominant imaginal elements. The order in which they are named also gives their order of frequency from greatest to least.

TABLE III.

Obs.	A									M									S								
Images	*Visual*			*Auditory*			*Tactual*			*Visual*			*Auditory*			*Tactual*			*Visual*			*Auditory*			*Tactual*		
Clearness*	C	M	O	C	M	O	C	M	O	C	M	O	C	M	O	C	M	O	C	M	O	C	M	O	C	M	O
H	6	26	2	9	24	1	2	9	7	39	14	3	2	1	2				20	17	4	4					
F	5	33	4	10	35	3			2	38	2		43	4	3				34	6	5						
H & F	2	38	8	3	43	2				60			58	1	3				25	20	3						
Tact. 0.50 in.		24						25		23			2			3			9	11	1						
0.75 in.		24						24		25						8			15	8	2						
1.00 in.		25						25		19	6					4	4		14	9	1						
Total	13	170	14	22	102	6	2	83	9	204	22	3	105	6	8	15	4		117	71	16	4					
Total of Clearness Values		197			130			94			229			119			19			204			4				
Per cent.		47			31			22			63			32			5			97			3				
Grand Total					421									367									208				

*C stands for clear; M, for moderately clear; and O, for obscure. These terms will be used later in the paper.

TABLE IV.

	Frequency										% for every Stimulus									
	Visual			*Auditory*			*Tactual*				*Visual*			*Auditory*			*Tactual*			
St.	I*	E	O	R	I	E	R	I	E	O	I	E	O	R	I	E	R	I	E	O
H	112	30	6	27	15	7		16	3		52	14	3	12	7	3		7	1	
F	53	82		46	25	30		2	1		22	34		19	10	13		.8	.4	
H & F	68	95		74	5	37					24	34		27	2	13				
Totals	233	207	6	147	45	74		18	4											
%	32	28	.8	20	6	10		2	.5											
% of f. in class	52	47	1	55	17	28		87	18											
Tactual																				
0.50 in.	21	50	1		2		4	1	22	1	21	50	1		2		4	1	22	1
0.75 in.	23	53					5	4	25		21	48					5	4	23	
1.00 in.	22	53					5	1	27		20	49					5	1	25	
Totals	66	156	1		2		14	6	74	1										
%	21	49	.3		.6		4	1.9	23	.3										
% of f. in class	29	70	.4		100		15	6	78	1										

*R stands for reproductive; I for interpretative; E for elaborative; and O for orientating.

(2) The kind of image depends to a large extent upon the sense department excited. When the ear is stimulated one should expect more auditory images than when the eye is excited.

(3) The kind of image depends more upon the nature of the stimulus than upon the *O*. There seems to be less ground for adhering to imaginal type than has been supposed. The facts do not indicate any such inherent characteristic.

B. The function of the image. The simple image functions very specifically in the perceptual complex. In the study upon visual perceptions the image served for interpretation, orientation, and appreciation. In this study the functions grouped themselves as reproductive, interpretative, elaborative, and orientating. By the reproductive function is meant the recall of the original sound or pressure without any enhancement of immediate meaning.[6] Thus, M reports:

> "I heard the click and tone again with the same intensity and interval as when stimulus was given. The tone seemed to be a little behind the click."

The reproductive image could necessarily be only of the same nature as the sensations aroused directly by the stimulus. Accordingly, in this study, we should find only the auditory and tactual images functioning thus. The interpretative performance of the image is identical with the usage of the term in the previous study. It implies an effort or intent on the part of the *O* to read meaning into the object. The elaborative function is the tendency on the part of the experiencing individual to enhance and enrich the meaning by filling in details and supplementing the directly aroused sensations with images carrying derived meaning. For example:

> "The click was that of a little girl striking the pavement after falling from a high window. Visually I saw the place where she landed, her clothing, etc." (S)
>
> "The tone belonged to a phrase of a musical piece which I play. The rest of the melody came in auditory and visual imagery. I could hear the melody and see the score of music. Also I could see and feel my fingers striking the keys." (A)

The orientating function is the same as that referred to in the previous article. The images serve to place the object in time or space with reference either to the *O* or to other objects.

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[6] Reproduction, as the term is used in this paper, may be either active or passive on the part of the *O*: *i.e.*, he may purposely reproduce the exact sound or pressure in order further to elaborate the meaning, or images of the original experience may repeat themselves without effort on his part. If the *O* reports that the image is a repetition of the original sound or pressure, whether active or passive, we shall call it reproductive.

With respect to the auditory and tactual perceptions, Table IV. will make clear the results.

In studying this table, it becomes evident that the following conclusions may be formulated:

(1) Since these experiences were auditory and tactual, only the auditory and tactual images can be reproductive. Considering the auditory images themselves, we find that the percentages which recalled merely the direct sensations show a steady increase from those of the "hammer" experiences to the "hammer and fork" experiences; *i.e.*, the more complicated the experience, the greater the tendency to recall the original perception. The reproductive tactual images have approximately the same percentage of frequency for all widths of the stimulus (5%). This indicates that the 1-in. width presented no greater problem to the *O* than the 0.5-in. Also when compared with the reproductive auditory elements, the tactual reproductive images constitute but 15% of all tactual images (see '% of f. in class' under Tactual), whereas the auditory reproductive furnish over half of the auditory images (55%).

(2) With respect to the interpretative function, visual images are of the greatest importance when considered from the point of view of frequency. The highest percentage (52) is in the case of the "hammer" experiences, or the most meaningless and unfamiliar. Between the "hammer" and "fork" experiences there is a difference of 30%. The tone from a fork, being very familiar, would need little interpretation. Between the "fork" and "hammer and fork" perception there is a very small difference, a 2% increase. The 2% increase may be accounted for by the slight amount of questioning which the perception of the click aroused when sounded with the tone. It has been mentioned before that, in these experiences, the *O*s ignored the unfamiliar click and elaborated upon the more familiar and pleasing tone. The percentage of visual images interpreting the tactual sensations is practically the same for the three series (21, 21, and 20%), again indicating an equality in the complexity of the stimuli.

The auditory interpretative images show a greater percentage in the case of the fork (10%) than in that of the hammer (7%). One would expect just the reverse. The result may be accounted for, however, by the fact that one *O* (M) experienced very frequently a primary memory-image of the tone as if it were a tailing-off of the original sound; she reported that there was a tendency on her part to name it. The tone was kept in the focus of attention in order to give it definiteness of meaning. Auditory images serve very little to interpret the "H and F" combination. The auditory images in these experiences were mostly involved in reproducing the sensations, and the interpretation was left to the visual images. Likewise, auditory interpretative images appeared very seldom in the tactual perceptions.

The greatest frequency of the tactual interpretative images (7%) was in the case of the "H" experiences, as was also the case with the analogous visual images. The small percentage of tactual images assisting in reading meaning into the tactual perceptions gives further evidence that the tactual stimuli were not complicated enough to furnish a problem for the *O*.

(3) All classes of images show high frequencies of the elaborative function. This service does not seem to be characteristic of any one particular kind of image. One evident conclusion may be stated, however; namely, that the elaborative function shows a high percentage when the interpretative is low, and a low when the latter is high. That is, whenever the experience is "meaningless" and unfamiliar there is more need for interpretation and not so much chance for elaboration of meaning; and when the object is familiar and easy to apprehend, then the *O* enhances and enriches the immediate meaning with derived significance.

(4) The orientating function presents a very low frequency. In order to place objects in time and place, one must have had more or less experience with analogous objects. The average individual pays comparatively little attention to auditory experiences in comparison with his visual experiences and very, very slight attention to tactual experiences; consequently we find this reflected in the attitude of the *O*s to the various stimuli. The greatest frequency, however, is in the case of the sound-hammer perceptions, just as would be expected. Here the *O* makes the most attempts to interpret and orientate, and the least to elaborate.

(5) Summing up all of these results, the conclusions may be drawn: (*a*) that the more complex the stimulus, the greater is the tendency to reproduce the direct sensations in imagery; and (*b*) that, in passing from unfamiliar to familiar objects, there is first a tendency to interpret, secondly to orientate, and lastly to elaborate upon the immediate meaning.

(6) In comparing these functions with those of the visual perceptions, the images in the auditory and tactual perceptions perform two new functions, those of reproduction and elaboration, while they did not serve to appreciate. The writer read the reports very carefully for anything which might be analogous in the two studies. The fact that the visual perceptions did not show reproductive imaginal elements is probably explicable by the fact that in the auditory and tactual experiences the *O*s found it necessary to reproduce the object in order to interpret it, while in the visual perceptions this seemed to be unnecessary.

Cases in the visual perceptions which verged upon reproduction always had other parts assimilated, and were called forth to question the probability of there being some object which the *O* felt might fit,—which would be interpretative and not reproductive. This may point, again, to a greater ease with visual experiences. Again, with respect to the elaborative function, this is the enhancement and enrichment of meaning without orientation or appreciation. This may be accounted for by past experiences. When one experiences sounds, music in particular, one usually elaborates upon their significance and appreciates them, but is less likely to orientate them in time and place than one does with visual experiences. The sounds presented in these experiments were evidently too simple and unfamiliar to arouse appreciative processes. This latter statement is probably also true of the tactual objects. On the other hand, the visual perceptions did not display any purely elaborative images; *i.e.*, there was no mere tendency to enrich the immediate meaning without interpreting, orientating, or appreciating. This means that, if one has a visual idea of a tree, one does not simply fill in the branches, leaves, and knots, without assigning it to some time or place or without appreciating it. The latter supplements are even the more dominant.

Bartlett[7] has recently conducted an experiment along these same lines, but his approach is different from that of this investigation. He attempts to determine the function of the image through images of memory after various periods of time have elapsed, varying from 15 min. to an indefinite period, not specified in his report. Whereas Bartlett depends upon images of memory entirely, in this study we have considered only those images which are closely assimilated to the perception,—the entire period reported upon being but 2 sec. The methods of procedure also differ widely, Bartlett using very complex stimuli, such as postcards and picture signs (not definitely described, except for the phrase "many of them taken directly from pictographic writing"),[8] with accompanying words to be associated with the signs; while in this experiment we used very simple stimuli, increasing their complexity very slightly as compared with Bartlett's stimuli. All of the stimuli in Bartlett's experiment were presented visually, whereas ours were presented tactually and auditorily; but Bartlett assumes that "visual imagery is typical of direct sensory imagery of all kinds."[9] Our results confirm his assumption. In our interpretations we also differ widely: Bartlett considers the function of the image in its relation to the subsequent reaction, whereas we have considered it in its relations as if looking backward toward the immediate apprehension of the stimulus. As a result of these two points of view, Bartlett finds the image functioning as a facilitation for a type of response, as an aid to a checked reaction, and as an alternative reaction, while we find it serving to reproduce, interpret, orientate, and elaborate.

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[7] F. C. Bartlett, The Functions of Images, *Brit. J. of Psychol.*, 11, 1921,, 320 ff.

[8] *Ibid.*, 321.

[9] *Ibid.*, 324.

TABLE V.

St.	Clear-ness	1. Distribution										2. % for Each Stimulus										3. No. Clear by Class			4. %			5. Total Clear All Classes			6. %	7. Total for Sense Dept.			8. %		
		Visual			Auditory			Tactual				Visual			Auditory			Tactual				Vis.	Aud.	Tact.	Vis.	Aud.	Tact.										
		I	E	O	R	I	E	R	I	E	O	I	E	O	R	I	E	R	I	E	O							C	M	O		C	M	O	C	M	O
H	C	52	13	2	6	7	2		1	1		27	7	1	3	4	1		.5	.5		67	15	2	35	8	1	84			44						
	M	40	13	4	19	2	4		8	1		21	7	2	10	1	2		4	.5		57	25	9	30	13	5		91		47						
	O	9			1				6	1		5			.5				3	.5		9	1	7	5	.5	4			17	9						
F	C	33	44		29	15	9					15	19		13	7	4					77	53		34	23		130			57						
	M	15	26		14	8	17					7	11		6	4	8					41	39		18	17			80		35						
	O	3	6		3		3		1	1		1	3		1		1		4	.4		9	6	2	4	3	.9			17	8						
H & F	C	51	37		56	3	2					19	14		21	1	.7					88	61		33	23		149			56						
	M	10	48		9	2	33					4	18		3	.7	12					58	44		22	16			102		38						
	O	5	6		3		2					2	2		1		.7					11	5		4	2				16	6	363	273	50	53	40	7
Tactual 0.50 in.	C	20	13			2		2	1			20	13			2		2	1			33	2	3	33	2	3	38			38						
	M	1	33	1				2		22	1	1	33	1				2		22	1	35		25	35		25		60		60						
	O		1										1									1			1					1	1						
0.75 in.	C	23	18					5	3			21	17					5	3			41		8	38		7	49			46						
	M		32							24			30							22		32		24	30		22		56		52						
	O		2										2									2			2					2	2						
1.00 in.	C	17	16					2		2		16	15					2		2		33		4	31		4	37			35						
	M	4	36					3	1	25		4	34					3	1	23		40		29	37		27		69		64						
	O		1										1									1			1					1	1	124	185	4	40	59	1

C. The relation of clearness to the image. This phase of the problem again divides itself into three problems: (1) the relation of clearness to the kind of image; (2) the relation of clearness to the function of the image; and (3) the relation of clearness to elaborateness of meaning. This latter problem was left a question in the last article.

A consideration of the first problem may best be considered by interpreting Table V.

By reading caption 3 of the table, it will be seen that the visual images are more frequently clear in all series of the auditory perceptions than moderately clear or obscure. The frequency of clear visual images shows a progression from the "H" through the "F" to the "H and F" experiences of 67, 77, and 88, respectively. The "H" and "H and F" series have practically the same frequency of moderately clear, 57 and 58, respectively, while the "F" series has but 41 cases of moderately clear visual images. The click of the sound-hammer must have been rather confusing and thus have tended to increase the number of unclear processes, while the tone gave no such complications. The very vagueness of meaning seems to have affected the clearness of the process. All of the series present about the same frequency of obscure visual images. Looking across to caption 4, at the corresponding percentages (figured on the basis of each series), we discover that the clear visual images present almost identical percentages (35, 34, 33), as do also the obscure visual processes. The widest discrepancy is among the moderately clear visual images, varying from 30% for the "H" to 18% for the "F" and to 22% for the "H and F" series. Thus the most unfamiliar stimuli arouse the greatest percentage of moderately clear processes. An unknown stimulus, then, has the tendency to increase the number of moderately clear rather than clear or obscure visual processes.

In the case of the auditory images, there is a greater percentage of clear processes when the stimulus possesses more familiar characteristics (23% each for "F" and "H and F") than when the stimulus is unfamiliar (8% for "H"). There is also a slight increase in the percentage of moderately clear elements for the two series having tonal qualities over that for the hammer series. The percentage for obscure auditory images is approximately the same for all three series. Whenever tactual images are present in auditory perceptions they are usually moderately clear or obscure.

Passing now to a consideration of the tactual perceptions, we find that the visual images are fairly evenly distributed in all series between the clear and moderately clear groups. Very few were obscure. This condition may be due to the simplicity of the experience. There were fewer instances of effort on the part of the *O* to read meaning into the object than in the auditory perceptions; and whenever definite, satisfactory meaning attaches to the situation, the images tend to stand out more clearly. Whatever meaning accrued to the perception came usually without searching for it. Comparing the tactual with the auditory perceptions, this even distribution of clear and moderately clear images would indicate a lack of confusion on the part of the *O*. This is also borne out by the fact that most of the visual images served the elaborative function.

The tactual images in the tactual perceptions, on the other hand, are found almost exclusively in the moderately clear group, showing about an equal distribution for the three series.

Glancing at captions 5 and 6, the frequencies and percentages show that, considering all classes of images in the auditory perceptions, the highest frequency is in the clear groups for the "F" and "H and F" series, while the moderately clear zone is the richest in the "H" series. In the tactual perceptions, the moderately clear processes show the highest percentage in each case. In conclusion, then, the auditory perceptions involved more clear than unclear images, while the tactual perceptions included more unclear than clear. This is explainable by the greater interest the *O*s displayed for the auditory presentations, especially those involving the tone.

The second phase of this part of the problem, *i.e.*, the relation of clearness to the function of the image, may be explained by reference to captions 1 and 2 of Table V. for comparisons between the various series and to Table VI. for the composite compilations.

The visual images which serve the interpretative function have higher percentages of clear than of moderately clear and obscure together, both in auditory and tactual perceptions. Thus, wherever effort is involved, the processes are pressed into the focus of attention. Only in the "H" series does the percentage of the moderately clear approach that of the clear. The auditory images performing the same function show the same relationship that the visual images do. The tactual interpretative images are mostly unclear in the auditory perceptions, but usually clear in the tactual perceptions.

In the case of the elaborative function, the visual images show no consistent distribution among clear and unclear groupings in the auditory perceptions. In the "H" series the distribution is equal (7% of each); in the "F" series, the clear outnumber the unclear (19% clear, 11% moderately clear, and 3% obscure); while in the "H and F" series the reverse is true,—the unclear surpass the clear (14% clear, 18% moderately clear, and 2% obscure). The higher percentage of clear than unclear in the "F" series is due to the high frequency of clear visual elaborative images of one *O* (M) in this series. She was particularly pleasantly affected by the tone, and elaborated it into church bells, chimes, etc. In the tactual perceptions, the distribution of the percentages is consistent throughout the three series, the highest percentage being in the moderately clear, the second highest in the clear, and the lowest in the obscure region. Each series possesses approximately the same proportion. The distribution of the first is typical: 13% clear, 33% moderately clear, and 1% obscure. The auditory and tactual images of the elaborative function in the six series show practically the same distribution as the visual do in the tactual perceptions.

The auditory images serving the reproductive function display the greatest frequency of moderately clear for the "H" series, the next greatest frequency of clear processes, and the least of the obscure. The relationship of the clear and moderately clear in the "F" and "H and F" series is the reverse of that in the "H" series. The tactual images functionating to recall the original sensations are about evenly distributed among the clear and moderately clear fields of attention.

Six out of the eight instances of the orientating function of both visual and tactual images are moderately clear.

In Table VI. a composite of each function of each kind of image has been arranged according to clearness values, so that this table shows at a glance the relationships between clearness and the functions of the various kinds of images.

Summing up all that has been said about this phase of the problem, and setting forth the implications of Table VI., the following results may be stated.

(1) The image involved in the reproductive function is more frequently clear than unclear. No additional images or indirectly aroused sensations are necessary for mere recall, so that the clearness of these images is not reduced by complication of the experience.

(2) The interpretative images are more frequently clear than unclear, with one exception, when tactual images serve to interpret auditory perceptions.

(3) The images which serve to elaborate the meaning of the perception lie without exception for the greater part in the background of attention. In only one instance are there more clear than moderately clear cases, *i.e.*, in the "H" series; but when the obscure are added to the moderately clear, the unclear outnumber the clear.

(4) The orientating function displays the greatest frequency in the moderately clear region.

TABLE VI.

Stimulus	Function	Distribution												Percentages											
		R			I			E			O			R			I			E			O		
	Clearness	C	M	O	C	M	O	C	M	O	C	M	O	C	M	O	C	M	O	C	M	O	C	M	O
	Kind of Images:																								
	Visual				136	65	17	94	87	12	2	4					20	9	2	14	13	2	.3	.6	
Auditory	Auditory	91	42	7	25	12		13	54	5				13	6	1	4	2		2	8	.7			
	Tactual				1	8	7	1	1	2							.1	1	1	.1	.1	.3			
	Visual				60	5		47	101	4		1					19	2		15	32	1		.3	
Tactual	Auditory				2												.6								
	Tactual	7	5		4	1		2	71			1		2	2		1	.3		.6	23			.3	
Total for Auditory		91	42	7	162	85	24	108	142	19	2	4													
	%	13	6	1	24	12	3	16	21	3	.3	.6													
Total for Tactual		7	5		66	6		49	172	4		2													
	%	2	2		21	2		16	55	1		.6													
Total for Aud. & Tact		98	47	7	228	91	24	157	314	23	2	6													
	%	10	5	.7	23	9	2	16	31	2	.2	.6													

The third subdivision of this phase of the problem undertakes to determine the relation of clearness to the elaborateness of meaning. After the discussion of the relation of clearness to the function of the image, it is very evident that, when the meaning is slight, unfamiliar, confusing, and difficult to grasp, the images involved in such complexes serve more for reproduction and interpretation, and by virtue of the very effort put forth become clearer. When the meaning, on the other hand, is more highly derived, familiar, explicit, and easy to comprehend, the images are more likely to serve for elaboration, arise without so much effort on the part of the *O*, possess a higher percentage of frequency, and are mostly moderately clear.

Thus the conclusion may be drawn that images are clear, but less numerous, when significance is slight; while, on the other hand, they are numerous, but moderately clear, when meaning is more elaborate.

III. Summary and Conclusions

Summarizing, we may say that auditory and tactual perceptions involve mostly visual, auditory, and tactual images, and that the percentage of frequency of the kind of image depends upon the nature of the stimulus. Images furnish approximately one-half of the indirectly aroused processes (52%) and organic sensations the other half (48%) in auditory perceptions, while in the tactual perceptions the organic processes form 61% and the imaginal 39%. An increase in the complexity of the stimulus is correlated with an increase in the kind of image corresponding to the directly aroused sensations.

These simple images render very definite services to the auditory and tactual perceptions; namely, those of reproduction, interpretation, orientation, and elaboration. Since these were auditory and tactual perceptions, the reproductive function was limited entirely to auditory and tactual images. The interpretative function is more characteristic of visual than of auditory and tactual imaginal elements. So also are the elaborative and orientating functions. The variance in frequency of these functions depends upon different factors; the function of reproduction becomes more frequent with the complexity of the stimulus, while the other three functions depend upon the degree of familiarity of the stimulus,—the less familiar involving first the interpretative, secondly the orientating, and the more familiar the elaborative function.

With respect to clearness, we find that most of the visual and auditory images tend to be clear, while the tactual tend to be moderately clear. The kind of image in relation to clearness can scarcely be considered without the function. Whenever the image serves the interpretative and reproductive functions, it is more frequently clear than unclear; while, when it functionates elaboratively or orientatively, it is more frequently unclear. Another very definite relationship between the clearness of the image and the perception is that, when the meaning of the perception is hazy and vague as in the case of the "hammer" experiences, or slight as in the tactual experiences, the images on the whole are unclear; while, in instances where the meaning is more elaborate and derived, more images are clear as in the "fork" and "hammer and fork" experiences (see captions 5 and 6 of Table V.). This general relationship is modified, however, when the function of images is taken into account, as has been indicated above; interpretative images, which are more closely attached to perceptions with vague meaning, are more frequently clear; and orientating and elaborative images, which are attached to perceptions of derived significance, are more frequently in the background of attention. These two statements are reconciled by the fact that reproductive images are more frequently clear than moderately clear, and that their greatest frequency is in perceptions of elaborate meanings and not in perceptions with slight significance. This accounts for the relation between clearness and meaning when we consider the total number of images.

In final conclusion, it may be stated that the kind, complexity and familiarity of the object arousing a perception could be fairly well predicted from the kinds of images involved, the frequency of the clear and unclear processes, and the function of the image.

OBSERVATIONS ON TAKING PEYOTE
(*ANHALONIUM LEWINII*)

By Samuel W. Fernberger, University of Pennsylvania

The following observations were made on taking Peyote (*Anhalonium Lewinii*). The drug is one of the cacti frequently and popularly called Mescal Buttons. The chemistry of the drug is imperfectlv understood, but the active principle seems to be one of the alkaloids.[1] The plant is found only in parts of the Rio Grande valley on the Mexican side. It is frequently used by the local Indians as part of their religious ceremonies. Within the last twenty-five years a rather widespread Peyote cult has grown up among the Indian tribes just west of the Mississippi basin, and this cult has reached as far north as Missouri at least. As a matter of fact, the use of the drug among the Indians has become so widespread that several of the states have taken active measures to try to prevent its use.[2]

The reason the Indians have made its use part of their religious ceremonies is that the eating of the drug is followed by the appearance of colored images. A number of cases are described in the literature, of which we mention two only as typical. Mitchell[3] gives a very interesting account of his own experiences and of those of a friend. Mitchell's own experience appears to have been as vivid as any that is recorded in the literature,—whether or not owing to the well-known ability in verbal description of the late Dr. Mitchell, we cannot say. The friend had colored visions also, but apparently not so brilliant as those of Mitchell. The other report is that of Prentiss and Morgan, mentioned above. In all they report six experiments on five subjects. One subject submitted to taking the drug twice, in spite of what Mitchell declares: "These shows are expensive. For two days I had headache and for one day a smart attack of gastric distress. This came after the first dose and was most uncomfortable. The experience, however, was worth one such headache and indigestion, but it was not worth a second."

The summary of these seven cases seems to show most clearly that visual manifestations occur after ingesting the drug. Great individual differences are to be noted in the vividness, intensity, saturation, complexity and duration of the visual phenomena. In every case they are colored, and in every case the colors seems to be in movement and constant change. The manifestations may take the form of known objects or may be of abstract geometrical shapes. All subjects agree that the visions are obtained only with the eyes closed, and that they disappear when the eyes are opened even in a darkened room. Several of the subjects note that visual after-images seem to be of longer than the usual duration. Most of the subjects also report a distortion of time, which seems very much prolonged; and in one case there was a distortion of visual space. Both Mitchell and Prentiss and Morgan believe that these visual manifestations are of cortical origin and are to be identified as visual images.

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[1] For a description of the chemical reactions and physiological effects on animals, *cf.* L. Lewin. *Anhalonium Lewinii*, *Therapeutic Gazette*, 4, 1888, 231-237.

[2] For a description of the Peyote cult ceremony, *cf.* D. W. Prentiss and F. P. Morgan. *Anhalonium Lewinii* (Mescal Buttons): A Study of the Drug with Especial Reference to its Physiological Action upon Man, with Report of Experiments, *Therapeutic Gazette*, 9, 1895, 577-585. Also M. R. Gilmore. Uses of Plants by Indians of the Missouri River Region, 33*d Annual Report of the Bureau of American Ethnology*, 1919, 104-106.

[3] S. Weir Mitchell. The Effects of *Anhalonium Lewinii* (The Mescal Button), *British Medical Journal*, 2, 1896, 1625-1629.

Thus far no reports by psychologists trained in observing mental processes have appeared in the literature. The one attempt is that of James,[4] who remarks that he ate one button (obviously too small a dose), became ill, had no visions, and discontinued the experiment. It seemed worth while to try the experiment, if only to determine whether the visions are to be identified with visual memory-images. It has been demonstrated by continued introspection, both in and out of experimental settings, that I am almost entirely lacking in visual imagery. It therefore seemed probable that, if these manifestations are imaginal in character, they would not be present in my case.

The experiment was conducted at Clark University on February 27, 1920. I wish to express my thanks to Professor Edwin G. Boring for his kind assistance and suggestions during the course of the observations. The reports were taken down in full by a stenographer. During a large part of the experience I was lying on a lounge with my eyes blindfolded with black velvet. A total dose of 39 grams, amounting to six large buttons, was ingested. The dried buttons were chewed and swallowed. This was a lengthy affair, inasmuch as they were intensively bitter and very nauseous. The first dose of 13 grams was taken at 12 noon; 13 grams more at 12.30; and the final 13 grams at 1.30 p. m. During most of the observation I was bothered with stomachic aches and pressure in the abdomen, and later with continuous frontal and occipital headache.

By 1.06 p. m. I observed that, on closing the eyes, both positive and negative visual after-images seemed to be relatively more intensive and of longer duration than usual. About this time I reclined and bandaged the eyes. At 1.43 a spot of white light of low intensity appeared about the center of the horopter. From then until 2.58 there were intermittent color effects. Colors from violet to orange appeared, but seldom red. The colors were rarely of a high degree of saturation. Two spatial forms were present,—one in the form of an arc in the upper half of the horopter, and the other in the form of lines down the center of the horopter. The colors seemed to be in constant movement. They did not seem to be affected when I opened the eyes inside the blinders, which were entirely light-proof.

At 2.58 I removed the blinders. After light-adaptation had progressed, I discovered an inability to hold fixation on any object for more than an instant. At 3.07 I made a chance discovery. While smoking a cigarette, I observed that the lighted tip, seen in indirect vision, was relatively much clearer and more intensive and saturated in color than normal. This fact was verified; and it was very noticeable that colors in the periphery of the visual field had a more than normal degree of saturation and clearness. This did not appear to be true, at this time, of colors seen in foveal vision.

At 3.22 the blinders were replaced, and the color-effects began immediately. During this period colored circles appeared, always spinning in a counter-clockwise direction.

At 3.48, over two hours after the final dose, a new series of phenomena become apparent. These phenomena were of such an unexpected sort and were so interesting that perhaps they have vitiated, to an extent, the observations. At any rate the blinders were not resumed and no further mention is made of retinal phenomena. These new phenomena had primarily to do with kinaesthetic sensations. At 3.48 I noted that dictation seemed to be very much slower than normal, although the stenographer assured me that my speech was as rapid as usual. At 3.54, when moving my shoulder, I was aware of clearer kinaesthetic sensations than I had ever known before in my experience. The muscular, and especially the articular, sensations were tremendously clear, but not more intensive than normal. This, in spite of the fact that I am of a kinaesthetic type and therefore used to attending to sensations of this modality. The blinders were then removed, and the parts of my body, seen visually, seemed to be very far away. I extended my arm and fixated the finger-tips, and then stretched the arm away from me. The hand seemed to move to a distance of at least ten feet. The stenographer moved her head and shoulders back a few inches, and she seemed to be drawing back many times that distance. This distortion of space was present for both monocular and binocular vision.

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[4] W. James. Familiar Letters of William James, *The Atlantic Monthly*, 1920, August, 171.

Experiences of this sort lasted several hours. During this period I was in what must have been very close to a state of dual personality. I was able to make perfectly rational and normal verbal judgments that were totally at variance with the immediately present sensory cues. I could say that a certain calendar was only actually four inches long although it appeared to be many feet. So far as one could analyze, the only conscious processes which would account for this state were a greater clearness of all sensations, but especially of those of a kinaesthetic sort. This would account for the distortion of visual space at least. If our judgment of depth is dependent on sensations from changes in parallax, then a great increase in the clearness of these sensations would give the illusion of magnifying visual space. Such an analysis would account for the magnification of kinaesthetic space as well.

My state, at this period, can best be described as a supernormally clear focus of attention (which, however, was very rapidly changing) with practically no background of consciousness. The increase in clearness was quite evident in sensory modalities other than the kinaesthetic. I was aware of throat-noises when a person a number of feet away from me swallowed; of color shadows in a magazine cover, and the like; in a word, of stimuli which are normally well above the sensory threshold but which usually pass unnoticed.

Accompanying this distortion of space was also a distortion of time. A given period of time seemed very much more extended than normally. Speech seemed slow. Walking became a ponderous affair. In walking I felt successively sensations from the muscles and joints involved. I became aware at each step, for example, of sensations due to the curling of the toes. If the clearness of the sensations accounts for the distortion of space, the very rapidly changing focus of attention would readily account for the distortion of time. We naturally and normally judge a period of time by the 'filling', by the number of processes that have claimed the center of attention. Under the influence of peyote this filling, for a given unit of time, is much increased, and so the period seems extended.

These reactions were of an unexpected sort and could not have been anticipated from a reading of the literature. They may have been due either to the fact that I am non-visual or to the fact that I took a larger dose of the drug than is elsewhere recorded in the literature. As a result we were quite unprepared to make sensory tests. Several tests of a rough sort were tried. Apparently my ability to judge brightnesses of greys was not enhanced. My after-images of the James waterfall and of the Mach spiral were normal. Stereograms were normal. An absolutely restful insomnia lasted throughout the night. I worked the next day without, apparently, any disastrously ill effects. It was at least forty-eight hours before the dilatation of the pupils of the eyes and other physiological effects had entirely disappeared.

The state I have just described seems more like that reported for hasheesh or *cannabis indica* than for peyote. The exception is that the illusions following the use of these two drugs are described as being accompanied by a 'feeling of superiority'. In the present experience I had a definite feeling of inferiority. Mitchell, however, reports the opposite: "Yet the sense of increased ability was so notable that, liking to test it, and with the commonsense belief in its flattery, I took up a certain paper on psychology, which a week before I laid down in despair. I grieve to say that it was less to be comprehended than ever."

The above experiment was made in an effort to determine whether or not the visual manifestations following the use of peyote are imaginal in character. In that respect the experiment was a failure. Visual colored manifestations occured in my case in spite of the fact that I am normally almost totally devoid of visual images. These phenomena were, however, much less intensive and saturated in my case than would appear from other reports in the literature. The colored phenomena appeared only on a dark field, either with the eyes closed or with the eyes opened inside a light-proof bandage. The crucial test, as Mitchell has already pointed out, can be made only on a congenitally blind subject. Up to the present time I have been unable to induce such a subject to undergo the experiment. I shall be glad to supply the drug to anyone who can procure the proper subject.

Owing possibly to the fact that I took an especially large dose of the drug, I experienced a distortion of space and time not recorded for this drug but apparently resembling that obtained with hasheesh or *cannabis indica*. All sensations appeared to be greatly enhanced in clearness but not in intensity or, in the case of colors, in saturation. The state can be described as a very clear but rapidly changing focus of attention. The increased clearness seems to account for the distortion of space; the rapidly fluctuating focus of attention seems to account for the distortion of time.

ERNST MEUMANN 1862–1915

By G. STOERRING, University of Bonn

I am glad to comply with the Editor's request to say something of the life and work of my friend, the late Ernst Meumann.

Meumann, the son of a Lutheran pastor, was born in Uerdingen bei Wesel on the Lower Rhine, August 29, 1862. After finishing his high-school course he matriculated (Easter, 1883) at the University of Tübingen, and devoted himself to the study of theology and philosophy. He later attended the Universities of Berlin, Halle, and Bonn, until Easter, 1887. He passed successfully the two theological examinations, and in 1891 obtained at Tübingen the degree of doctor of philosophy with a thesis on the Law of Association and Reproduction. He then turned to the study of experimental psychology, and entered Wundt's Institute at Leipzig. In 1894 he became *Privatdozent* in philosophy at Leipzig; his dissertation was entitled An Investigation of the Psychology and Aesthetics of Rhythm. In the years 1893 to 1896 he published his famous studies in the psychology of the temporal consciousness. In 1897 he was called to the University of Zürich as professor of philosophy, psychology and education. In 1905 he accepted a call to the University of Königsberg. He then occupied, in quick succession, chairs of philosophy at the Universities of Münster, Halle, and Leipzig, and finally settled down in the *Vorlesungswesen* at Hamburg, where he died of pneumonia, April 25, 1915.

I first met Meumann when we were students at Halle. We had kindred scientific ideals and soon formed a close friendship. At that time he was mainly interested in problems of metaphysics; he had as yet no thought of experimental psychology and education.

Meumann had been very religiously brought up, and religious beliefs and modes of thought has taken strong hold on him. He was now to fight the battle between faith and knowledge. He found himself obliged, though it cost him a serious struggle, to give up one dogma after another. He had meant to become a teacher of religion, and with that end in view to make himself a member of some philosophical faculty. Presently, however, when he realized that he had travelled too far from the ideas of orthodox Christianity to be able, with good conscience, to undertake the task of religious instruction, he reluctantly gave up his original plan. We then decided to take up together the study of medicine; for we had come to the conclusion that psychology is the necessary condition of any wholesome philosophising, and that certain medical studies are the essential precondition of successful work in psychology. Meumann, unfortunately, could not accustom himself to the odors of the dissecting room, and was therefore unable to complete his medical course. He sought accordingly to gain from books the knowledge of anatomy, physiology, and psychiatry that he judged necessary for psychology, and in fact acquired an exceptionally thorough knowledge of physiology.

Meumann had a quite extraordinary enthusiasm for scientific investigation, together with an unusual tenacity of purpose, so that he managed, despite a poor physical constitution, to get through an astonishingly large amount of work. To this intense interest in science he added an enthusiasm for social ideals, which was upheld by his strictly ethical view of human life and a truly Kantian conscientiousness. His ethical views were influenced by his hope in the moral progress of mankind.

Meumann had also a living interest in aesthetics. It was delightful to be with him on country walks and to share his keen appreciation of the beauties of nature. The service of art was for him a necessity of life. His knowledge of the history of art covered a wide range, so that he could appreciate a work of art from the artist's standpoint. He took an eager interest in the artistic furnishing of his own home.

He was strongly averse from everything mean and unworthy. His whole nature was ingenuous and sincere. He gave but little thought to the affairs of everyday life; his concern was rather with the welfare of mankind at large; and his private circumstances often suffered in consequence. It might have been better if he had paid a little more attention to money matters.

All the traits of his character and personality were drawn in large and noble lines. His manners were simple; he showed no trace of self-sufficiency; he never posed as a famous man. He bore himself modestly, despite his great achievements, and was nearly always dissatisfied with what he had done.

His scientific work lay in the fields of psychology, education and aesthetics.

Meumann's psychological investigations reveal him as a psychological experimentalist of the first rank and as a master of psychological discussion. These qualities appear even in his early articles on the temporal consciousness, which, as Wundt rightly remarks, stand among the very best that the literature of experimental psychology has to show.

We owe to Meumann and his pupils a series of admirable researches on the function of memory. Meumann drew a sharp line of distinction between immediate and permanent retention, and formulated the relations of dependence that characterise immediate retention. He also, in collaboration with his pupils, made additions of our knowledge of permanent retention and of ideational types. Further, and as a still more important contribution to this chapter of psychology, he and his pupil Ebert carried out pioneer investigations of the relation of general to special practice; discovered the fact that any special practice means the concomitant practice of related activities; and offered a competent defence of the hypothesis of a general practice, over and above the concomitant practice manifested by the part-activities, identical or similar, of a special practice.

Next, we owe to Meumann an increased knowledge of types of attention, which he first came upon in his study of immediate retention and which he later made the subject of tachistoscopic experiments. I mention here only one point, his discovery of the difference between total and discrete attention. With the one attitude of attention—so he found in his work on immediate retention—the stimuli exposed constitute in the observer's consciousness a whole, which before recitation he possesses in its totality and which, when recitation is called for, he gives forth with all speed. With the other attitude of attention the apprehension of the series takes a different course; it is now the rule that only the member of the series actually exposed finds representation in consciousness, and that before recitation the first and last syllables, but no more, are in the observer's mind; recitation proceeds relatively slowly and in characteristic fashion.

I will end this brief account of Meumann's special researches by saying a word of his work on feeling and its concomitant phenemena. The pneumographic studies, which he made in collaboration with his pupils in order to a characterisation of state of feeling by breathing-curve, are excellent of their kind. But he went further; he directed investigations that are of cardinal importance for the theory of feeling; and in his own articles on the sensitivity of the internal organs he turned to account the experiences of the surgeon and the pathologist for a theory of organic sensation.—

In his book on Intelligence and Will Meumann attacks problems of greater psychological complexity. Here he discusses intelligence and will each for itself, in its various modes and degrees, and each also in its relation to the other; nor does he fail to raise the question of the significance of intelligence and will for personality and for human life. The book abounds in keen psychological observations of the individual character, and in sober interpretation of these complex observations. We have been told, in recent years, that modern psychology is simply a branch of natural science, and that it has to do only with the simplest things of mind; and we have been told that this modern psychology must be supplemented by another, philosophically orientated psychology, which shall make no claim to carry us back to the ultimate elements of the mental life. To those of us who are repelled by such a doctrine, Meumann's contribution to the psychology of personality, which draws its materials from the whole complex round of life, is a work of outstanding value.

There can, then, be no doubt that Meumann takes his place among the leaders of contemporary psychology. In the field of experimental education, to which we now turn, he was, without question, the principal figure. It is true that studies in experimental education had been published before his day. But he did so much to further and extend this type of scientific enquiry, so much to organize and stabilize its results, that we must hail him the founder of experimental education as a special discipline. At the same time, he did not regard experimental education as the whole of education; he sought rather to relate it to the achievements of the great educators of the past. Moreover, he believed that education is concerned not only with the systematic guidance of the child in school but also with public instruction beyond the limits of the schoolroom. Its task, in this wider sphere, is to serve the cause of civilization at home and to advance international thought upon political and social problems. Public instruction is thus to be built up on a national foundation, but its national character must not render it blind to international interests. In December, 1914, during the Great War, Meumann wrote in the preface to the third volume of his Lectures on Experimental Education: "I should like to repeat here what I have said in the concluding sentences of the book,—that my occupation with questions of education has brought me again and again to realize how strong and how far-reaching are the intellectual and moral ties between the leading civilized nations of our time. In the field of education all these nations pursue the same ideal ends, and community of educational problems far outweighs national and local differences and divergences of opinion; so that there is no other sphere of human endeavor that affords such manifold opportunity for friendly exchange and mutual give-and-take. Perhaps, then, this book of mine, which has already found many friends abroad, especially (and, I think, significantly) in Austria-Hungary and the United States of America, may in some small measure help toward strengthening the intellectual ties that bind the civilized peoples together and toward the reconciliation of the nations now at war; for all alike have the same work to do in the moral education of youth."

Meumann assigned to aesthetics a fourfold task. It has to carry out a psychological investigation of artistic production and of aesthetic evaluation; it has to establish aesthetic norms; and it has to treat of art as a factor in the history of civilization. In his System of Aesthetics Meumann made a psychological study of aesthetic evaluation in closest connection with the facts of experimental aesthetics, and proposed a carefully graded scale of aesthetic judgment. At the highest level of aesthetic judgment of a work of art we have sympathetic feeling for, and sympathetic understanding of, the aim of the artist's labors. Meumann's psychological discussion of artistic production is highly original and important. Artistic production depends, first, on some experience that stirs the artist on the emotive side; secondly, on the urge to give expression to this experience; and thirdly on the endeavor to cast this expression into the form of a work of art. The motive to express and the motive to represent are discussed with rare insight. It is shown that the motive of representation sets limits to the motive of expression, limits which vary with the various modes of artistic production (laws of style in art). Painting, for example, offers many difficulties of representation that are foreign to poetry, and conversely. It is shown further, with all the force of demonstration, that the varying relation of the motive of expression to the motive of representation or of form determines the appearance of different schools of art, and that in modern art the motive to express takes precedence of the motive to form.

In his treatment of the norms of aesthetic evaluation Meumann laid down rules which are, as he said, of a quite general character and therefore —since they derive from the nature of art itself and from considerations directly suggested by that nature—escape the arbitrariness of the particular art-school or artistic authority. His use of psychological facts in the establishment of these aesthetic norms brought on him, in philosophical quarters, the reproach of psychologism. I cannot grant its justice. In the sphere of logic, where the argument deals with truths of reason and not with simple truths of fact, recourse to psychology, which is concerned only with truths of fact, is plainly impermissible. When, however, it is a question of the validity of aesthetic or (within limits) of ethical evaluations, then the introduction of psychological facts is not only harmless but actually necessary.[1]

Finally, I must not omit a reference to Meumann's executive capacity. He founded and edited the *Archiv für die gesamte Psychologie*, as well as his special educational *Zeitschrift*.

The death of Meumann removes a man who had rendered enduring service to psychology, education and aesthetics and to the practical reform of the teaching profession. With his brilliant endowment and his tremendous power of work he would, had life been spared, have added worthily even to his own great achievement.

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[1] I have discussed this point in *Sittliche Forderungen und die Frage ihrer Gültigkeit*, 106 ff.

WILLIAM HALSE RIVERS RIVERS 1864-1922

By F. C. Bartlett, Cambridge University

On June 3 last year I was walking through the grounds of St. John's College, here in Cambridge, when I met Dr. Rivers returning from a stroll. He was full of energy and enthusiasm, and began at once to talk about certain new courses of lectures which he proposed to deliver at the Psychological Laboratory during the present year. On the evening of the next day I heard that he was dangerously ill. As I approached the College on the morning of June 5 I saw the flag at halfmast. He had, in fact, died in the early afternoon of the preceding day. Never have I known so deep a gloom settle upon the College as fell upon it at that time. There was hardly a man—young or old—who did not seem to be intimately and personally affected. Rivers knew nearly everybody. As Praelector of Natural Sciences at St. John's he interviewed all the science freshmen when they came first into residence and, in an amazing number of cases, he kept in close touch with them throughout their Cambridge career. Everybody who came into contact with him was stimulated and helped to a degree which those who are acquainted only with his published works can never fully realize.

Rivers was born in 1864, and in due course went to St. Bartholomew's Hospital where, at the age of twenty-two, he qualified to practise medicine. Already his attention had turned in the direction of psychology. As House Physician at St. Bartholomew's he read several papers before the Abernethian Society on hysteria and neurasthenia. In 1891 he was appointed House Physician at the National Hospital for the Paralysed and Epileptic. Here it was that he met Dr. Henry Head, with whom he was later to collaborate in the important work on cutaneous sensibility. He had become interested in vision, and largely under the influence of Head, who had worked with Hering at Prague, he adopted with enthusiasm Hering's theories of color vision.

In 1893 Rivers was invited to Cambridge by Sir Michael Foster, to lecture on the physiology of the special senses, and in 1897 a movement, initiated many years before by Professor James Ward and Dr. Venn for the official recognition by the University of Experimental Psychology, produced its definite result, and Rivers was made University Lecturer of Physiological and Experimental Psychology. He was thus the first officially recognized teacher of experimental psychology at Cambridge. and the first person in England to plan and carry out a systematic course of practical teaching in the subject.

The year following he went with the Cambridge Anthropological Expedition to the Torres Straits. This was the first step of the second period of his career—the ethnological period, as it may be called. For some years yet, however, the greater part of his work was concerned with the psychology of the special senses, and particularly with that of vision. A glance at the bibliography of Rivers' published writings shows that, down to 1908, experimental work in psychology still occupied a large amount of his time. During this period he was carrying out his experiments on the influence of alcohol and other drugs on fatigue—published in 1908 as the Croonian Lectures to the Royal College of Physicians—and his "Human Experiment in Nerve Division", with Dr. Head. Meanwhile an expedition to Southern India in 1902 had produced a number of papers on the Todas and, finally, his book upon the same community. In 1904, with Professor James Ward and some others, Rivers founded the British Journal of Psychology of which he was at first joint editor.

From 1908 till the outbreak of the late war Dr. Rivers was mainly preoccupied with ethnological and sociological problems. Already he had relinquished his official post as Lecturer in Experimental Psychology in favour of Dr. C. S. Myers, and now held only a lectureship on the physiology of the special senses. By degrees he became more absorbed in anthropological research until, several years before his death, he resigned his lectureship, and thenceforward held no University appointment. But though he was now ethnologist rather than psychologist he always maintained that what was of value in his work was due directly to his training in the psychological laboratory. In the laboratory he had learnt the importance of exact method; in the field he now gained vigor and vitality by his constant contact with the actual daily behavior of human beings.

In 1908 Rivers made his first journey to Melanesia. The material and interests which the voyage gave him occupied practically the whole of his attention until 1914, when his great work entitled "A History of Melanesian Society" was published. In that year he made a second journey to Melanesia, returning to England in March 1915, to find that war had broken out.

Now came the third period of his life, in which all his old enthusiasm for psychology returned, intensified and strengthened by a determination to keep his knowledge in the closest possible touch with real life. He now carried out a very large amount of neurological work, first with men and officers who were suffering from various psycho-neuroses of war, and in the second place as Psychologist to the Royal Air Force. In this work he was extraordinarily successful, for throughout the whole of his life he possessed a remarkable power of attracting sympathy and confidence.

In the last five years of his life Rivers was amazingly active. He published a large number of papers, some on ethnology, some on neurology, but most on psychology. All of them were marked by great originality of ideas and a new and brilliant incisiveness of expression. It was now that he wrote "Instinct and the Unconscious" (published in 1920), and prepared for publication no less than three other books, two of which—"Conflict and Dream", and "Psychology and Politics"—have now been published, and the third of which—"Principles of Social Organisation"—is in the press. All of these were given first as lectures in Cambridge, and none of the large number of students who heard them is likely to forget the frankness with which Rivers was prepared to discuss difficulties, or the readiness with which he admitted the possibility and importance of other points of view than his own.

But his literary activities were only a small part of his crowded life during these days. He was a great influence in all the administrative life of his College; entertained constantly; regularly stimulated and directed informal discussions on any kind of subject among undergraduates; and took the keenest interest in the development of the fine Psychological Laboratory which had been founded at Cambridge by the efforts of Dr. C. S. Myers. He became more widely known than ever before, lecturing frequently upon psychological and sociological topics in many places. He often spoke to me with great appreciation of the stimulus he obtained from his visit to America in 1921.

Honours of many kinds had been showered upon him. In 1902 he was made a Fellow of St. John's College, and Praelector of Natural Sciences in 1919. He held the post of Croonian Lecturer to the Royal College of Physicians in 1906, and the Fitzpatric Lectureship in 1915-16. The Fellowship of the Royal Society was given him in 1908, and in 1915 he was awarded a Royal Medal. He was President of the Anthropological Section of the

British Association in 1911, and of the Psychological Sub-Section in 1919. In 1920-21 he was President of the English Folk Lore Society and, from 1921 to the time of his death, of the Anthropological Institute. He held an Hon. L.L.D. of St. Andrews, and an Hon. D.Sc. of Manchester Universities. Not long before the end of his life his increasing interest in affairs had led him to accept an invitation to stand as a Labor Candidate for Parliament for the University of London.

He held his honours lightly. It is not of these that his friends think chiefly now; not of his learning; not of his avidity for work, or of his splendid loyalty to the claims of exact method; not of the laborious and remarkable accuracy of his earlier researches, or even of the brilliance of many of his later speculations. These all were great, and are having their profound influence upon many lives. But it is of Rivers as a man that we think; of his eager and unconquerable optimism, and of his belief in the possible greatness of all things human. Whatever may be the verdict of the years upon his published works, the influence of his vivid personality will remain for all who knew him as one of the best things that have ever entered their lives.

CALIBRATION OF THE GALTON WHISTLE

By CHRISTIAN A. RUCKMICK, Wellesley College

It does not seem to be generally known in this country that the tables which accompany Galton whistles made in Germany before 1914 have been found unreliable. Owing to the war the account of recent investigations carried on in Germany and the announcement of the change in calibrating the whistle have not come to the notice of many American psychologists and physicists. Yet the instrument is still widely used for testing the upper region of hearing in both normal and pathological subjects, and the change in the method of determining the pitch-values of the whistle in this region involves a difference from the former values of nearly an entire octave. It therefore seems important to gather together the facts bearing on this subject, and to review briefly the historical development of the whistle; for the improvements in the form of the instrument and revisions in the method of calibrating it have naturally gone hand in hand. There are certain characteristics that have won favor for the whistle in and out of the laboratory. It is compact and easily and quickly adjustable. Besides, it has the aspects of precision: it is set by means of a micrometer mechanism; it is capable of a double adjustment; and it is accompanied by a table of vibration-rates expressed to the last integer of a five-place figure and carefully written out in longhand by the manufacturers. Fortunately for decades researches have checked and rechecked the pitch-values, and recently the makers of the instrument have taken advantage of the available data in the construction of the whistle and in its calibration.

In the original form used by Galton the whistle was made "from a brass tube whose internal diameter was less than one-tenth of an inch in diameter. A plug was fitted into the lower end of the tube, which could be pulled out or pushed in as much as desired, thereby causing the length of the bore of the whistle to be varied at will."[1] The lip of the whistle was first the common lateral slit found in our toy whistles and similar to the lips of organ pipes and fifes. This was the type of instrument that Galton describes as having been used by him to test the hearing of animals in the Zoological Gardens of London. "It consists of one of my little whistles at the end of a walking-stick—that is, in reality, a long tube; it has a bit of india-rubber pipe under the handle, a sudden squeeze upon which forces a little air into the whistle and causes it to sound."[2] The next step in Galton's time was to attach the plug to a revolving cap which screwed it in and out and permitted it to be more conveniently and evenly adjusted. The degrees of revolution of the cap could now be marked on the circumference of the cylinder and the position of the plug could be noted to 1/250 inch. The corresponding pitch of the instrument was not empirically obtained, however, but was calculated directly from the length of the chamber, according to physical formula. Galton warns us against too great faith in the table of pitch-values when the chamber becomes very small and the lengthwise direction of the adjustment no longer follows the direction of the sound-waves inside the chamber. "When the tube is reduced to a shallow pan, a note may still be produced by it, but the note has reference rather to the diameter of the whistle than to its depth, being sometimes apparently unaltered by a further decrease of depth."[3] This place appears to be reached when the depth of the whistle is less than 1½ times the diameter or, for tubes of 1/10 in. diameter, at the setting of .16 inch, the equivalent of 14,000 d.v. It is for this reason that the instrument requires a small bore. The first forms were made for Galton by the surgical instrument maker, Hawksley, and by the optician, Tisley. Koenig also made instruments that were frequently used in early researches.

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[1] Galton, F., Inquiries into Human Faculty and its Development, 1883, 38; *cf.* also 375. It was first described, apparently, in an article entitled "Whistles for Determining the Upper Limits of Audible Sound," S. Kensington Conferences (in connection with Loan Exhibition of Scientific Instruments), 1876, 61. See also *Nature*, xxvii, 1883, 491; Titchener, E. B., Experimental Psychology, 1905, II., ii., 31-46, *passim*; Sanford, E. C., A Course in Experimental Psychology, 1908, 379-381.

[2] *Inquiries*, 39.

A marked change in construction came in 1894 when the late Max Th. Edelmann paid heed to suggestions from Bezold and made the lip adjustable. At the same time the cylindrical lip (locomotive-whistle type) took place of the the slit at the side: in other words, as we now have it, the whistle consists of two parts, the cylindrical lip from the pressure bulb, and the chamber into which the stream of air passes and from which the sound arises. The length of this chamber is adjustable as before, but a new micrometer arrangement changes the distance between the lip and the chamber. The last two are connected, of course, by a heavy bridge. There was also an intermediate form, of lighter construction and without the micrometer adjustment for the lip or the accompanying scale divisions.[4] In the most recent form, but one not widely used in this country, a single turn of the cap changes both the setting of the lip and the length of the chamber. The cap tapers so that, through a series of levers resting on its side, its forward movement into the chamber also controls the opening at the lip.[5]

More significant than these improvements in the form of the whistle is the progress made in the method of calibrating it. For a long time the Kundtian dust-figures obtained with lycopodium powder were extensively in use, but it now appears that they revealed the first overtone in each series of determinations and that to get this overtone the whistle was unwittingly overblown.[6] For this reason the upper audible tonal limit of the whistle was often given as high as the tone in the neighborhood of 50,000 d.v., while a number of recent studies point to a considerable reduction in this value.[7] The Edelmann Institute, which now makes most of the whistles, has consequently adopted an entirely different method of calibration, although it is a method proposed as long ago as 1897 by Stumpf and Meyer.[8] It consists, briefly, in the step-wise determination of critical points on a curve obtained through the production of difference-tones from several whistles and tuning forks in the presence of a sensitive (Schmidt) flame.[9]

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[3] *Inquiries*. 375.

[4] Edelmann, M. Th., Die Edelmannschen Grenzpfeifen, *Mitteilung No.* 10, München, 1914. Fig. 1.

[5] *Ibid.*, Fig. 4.

[6] A review of the investigations of the reliability of the Galton whistle which appeared prior to 1903 together with bibliography is given by Titchener, *op. cit.*, 32-36. The later researches which contributed to the new method of calibration are:

Schulze, F. A. Einige neue Methoden zur Bestimmung der Schwingungszahlen höchster hörbarer und unhörbarer Töne. Anwendung auf die Töne der Galtonpfeife und die Bestimmung der oberen Hörgrenze. *Ann. d. Physik*, 24, 1907, 785-822.

Debyre, P. Eine Bemerkung zu der Arbeit von Hrn. F. A. Schultze. *Ann. d. Physik*, 25, 1908, 819-823.

Hegener, T. Ueber die Tonbildung bei der Edelmannschen Galtonpfeife und deren Verwendung zur Bestimmung der physiologischen und pathologischen oberen Hörgrenze. *Beitr. z. Anat. Physiol. Pathol. u. Therap. d. Ohres. d. Nase u. d. Halses* (Passow u. Schaefer), 1, 1908, 321.

Helmholz, W. Vergleich zwischen der Galtonpfeife und dem Schulzeschen Monochord zur Bestimmung der oberen Hörgrenze. Diss., Berlin, 1910. pp. 18.

Winkelmann, A. Handbuch der Physik, 2nd ed., II, 1909, 210. The article briefly summarizes recent developments in the field of the upper limit of hearing.

The manufacturers have also made a decided improvement by inserting a diaphragm between the pressure-bulb and the whistle to prevent 'over-blowing'. This diaphragm has an aperture so small that under repeated tests it was impossible to produce an air-pressure greater than that of 100 mm. of water. On the other hand it is large enough to allow an adequate and even stream of air to pass. Through correspondence with the Edelmann Institute the writer has learned that Galton whistles may be returned to the Institute for recalibration at a very moderate expense. The whistle in the laboratory of Wellesley College was recalibrated during the summer of 1922 for the sum of $1.20; the charge for the new pressure-bulb with inserted diaphragm was 60c; and the postage was 12c. To avoid duty, the customs-tag should bear the notation that the instrument was originally purchased in Germany, is being returned for recalibration, and is educational in character. The address is: Prof. Dr. Max. Th. Edelmann & Sohn, Munich, Germany. Mr. C. H. Stoelting, 3037 Carroll Ave., Chicago, advises the writer that if the matter is handled through his firm he will undertake to register the tables in his records, thus making it comparatively easy to procure a copy of any table that might later become lost. The writer has also been informed that the older tables can not be translated mathematically into the present lower equivalents since each whistle has its own set of independent variables and must therefore be empirically calibrated.

The outcome of this revision is naturally important. All studies of the upper limit of hearing, of pitch discrimination, and of allied auditory phenomena in this region will bear review if they were undertaken on the basis of examination with the Galton whistle. For example, the highest value given in the sample table in the Edelmann leaflet[10] for whistle No. 971 at 100 mm. of water pressure is 31,249 d.v.; above this point the whistle is declared to be unreliable. Our own whistle previously had a calibrated value of 45,000 d.v. at its highest point, but the new table gives it no setting above the value of 25,000 d.v. But in its latest improved form, with both a procedure of calibration and an actuating device that guard against 'over-blowing', the Galton whistle ought to return to service as a ready and dependable instrument in the determination of pitch sensitivity in the higher regions, of the upper limit of hearing, and of the many auditory defects and peculiarities that are presented in laboratories and clinics.

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[7] Of course this reading varied with the individual whistles. Edelmann, in Studien über die Erzeugung sehr hoher Töne vermittelst der Galtonpfeife (Grenzpfeife), *Ann. d. Phys.*, 4, 1900, 469-482, gives a setting (Tab. I) for 54,900 d.v. and one (Tab. II; Galtonpfeife Nr. 331) for 48,000. Speaking of the older form he says: "Sie erzeugt.... Töne, deren Schwingungszahl.... für die älteren Constructionen bei etwa 25000 g.s. sichere Töne zu geben aufhört. Bei meiner neuen Form der Grenzpfeife geht der Ton bedeutend weiter in die Höhe und überschreitet für ausgezeichnet Hörende mit etwa 50000 g.s. die Hörgrenze" (*ibid.*, 469).

[8] A. Schwendt described the earlier procedure in two articles: Experimentelle Bestimmungen der Wellenlänge und Schwingungszahl höchster hörbarer Töne, *Pflüger's Archiv*, lxxv, 1899, 346, and Ergänzende Untersuchungen über Tonhöhenbestimmungen sehr hoher Töne mittels der Kundtschen Staubfiguren, *Verhandl. d. deutsch. otol. Ges.*, 1900, 55. Curiously enough, the method now adopted had been proposed previously to this time by C. Stumpf and M. Meyer, in Schwingungszahlbestimmungen bei sehr hohen Tönen, *Ann. d. Physik*, lxi, 1897, 760-779.

[9] Edelmann, *op. cit.*, 5f.

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[10] Edelmann, *op. cit.*, 8.

POSITION vs. INTENSITY AS A DETERMINANT OF CLEARNESS

By KARL M. DALLENBACH, Cornell University

In all the experiments on the determination of attention that have been performed in the Cornell Laboratory,[1] spatial position has proved to be an important and specific factor. Meads, working with form *vs.* intensity, found that the intensity of the formed area (a Greek cross) varied greatly at the point of attentive equality accordingly as it was to the right or to the left of the 'formless' area.[2] Curtis and Foster, working with size *vs.* intensity, arranged their experiments so as among other things "to measure the error of space."[3] Their *Os* gave divergent results, but in one respect the results agreed: spatial position was found to be of more importance than an increase in area. Indeed, in the case of one *O*, "spatial position makes a greater difference in attention-compelling power than a fourfold increase in area."[4] In Bowman's repetition of Curtis and Foster's work the effect of position was likewise noted. Bowman found that "the left-hand position is the more favored."[5] Though the position-ratio varied greatly from *O* to *O*, the results justified the statements: "*the magnitude of the space error* proves that the effect of size was cut across and obscured by another influence,"[6] and "it seems that the left-hand position has an attentional advantage."[7]

It is the purpose of this study to compare in a quantitative way the 'attention-compelling' power of position and intensity, and to note to which of the four cardinal and four intermediate positions, if any, attention is predisposed. Two circular areas which varied only in position and intensity of illumination were compared, and the objective intensity at which the two were judged equally clear was determined for every one of the positions mentioned above.

Apparatus. The apparatus employed was that described in a recent number of this JOURNAL (34, 1923, 93 ff.) *O* sat in a light-proof booth with his head held firmly in position (in order to eliminate involuntary head-movements and to ensure constancy of position from experiment to experiment) by a biting board which was so adjusted that the fixation point extended would fall midway between his two eyes. The fixation point was 1 m. away and 1 mm. in diam.[8]

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[1] L. G. Meads, *Form vs. Intensity as a Determinant of Attention*, this JOURNAL, 26, 1915, 150-151; J. N. Curtis and W. S. Foster, *Size vs. Intensity as a Determinant of Attention*, *ibid.*, 28, 1917, 293-296; A. M. Bowman, *Size vs. Intensity as a Determinant of Attention*, *ibid.*, 31, 1920, 87-90.

[2] In Meads' published work (*op. cit.*) the effect of position is masked because he merely reported the general averages of his two spatial positions. His crude data, however, reveal that all his *Os* were predisposed toward one position.

[3] *Op. cit.*, 293.

[4] *Op. cit.*, 295.

[5] *Op. cit.*, 89.

[6] *Loc. cit.*

[7] *Op. cit.*, 90.

The comparison areas, 8 cm. in diam. and centered on opposite sides and 14 cm. away from the fixation point, could be rotated around the fixation point so that the standard and the variable would appear now to the right, now to the left, below and above fixation, and also at the four positions intermediate between these. The positions of the areas varied as follows: in position 1, the variable was at the left of the fixation and the standard at the right; in position 2, the variable was at a 45°-angle below and to the left of the fixation and the standard at the same angle above and to the right; in position 3, the variable was below, the standard above; in position 4, the variable was at a 45°-angle below and to the right of fixation, the standard at the same angle above and to the left; in position 5, the variable was at the right, the standard at the left; in position 6, the variable was at a 45°-angle above and to the right of fixation, the standard at the same angle below and to the left; in position 7, the variable was above, the standard below; and in position 8, the variable was 45° above and to the left of fixation, the standard at the same angle below and to the right.

Exposure was made by illuminating the areas from behind. The duration of the exposure was controlled by an adaptation of Wundt's gravity tachistoscope. The apparatus was set for 1/10 sec., but the actual time used was, as measured by a Hipp chronoscope, $100.3 \pm 0.7\sigma$. The light was derived from a 100-watt concentrated-filament Mazda lamp placed in a projection lantern behind the tachistoscope. The rays of light when uninterrupted by the tachistoscope passed through a light-proof funnel to the posterior frame of a diffusion chamber and thence, through circular windows, and through the connecting tubes of the chamber, to the ground-glass windows in *O*'s booth. The intensity of the illumination was controlled by varying the amount of light that was allowed to pass through the connecting tubes. The full amount of light that would pass through the window in the posterior frame, which was 8 cm. in diam., was taken as 100% or unity. As the diameter of this opening was varied by the insertion of different metal shutters the amount of light falling upon the anterior windows, those viewed by *O*, would also vary. The circular openings were placed in the center of the shutters so that difusion should be equal in all directions, and they were made of a size to decrease the amount of light allowed to pass by steps of 4% from 100%-40%, by steps of 2% from 40%-20%, by steps of 1% from 20%-1%, and by steps following the negative power of 2 from 1%-1/32%. The intensity of the standard was taken at 16%, and of the variable within the limits just stated.

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[8] Both *O*s reported that the fixation-point was not always stationary, that it at times moved about the field of vision. For the most part it appeared in its proper position, direct in line with the axis of the Cyclopean eye, but at times it seemed as if it was displaced "as much as a foot" from the true position. When the shift was observed the point usually appeared as given in the new position, but at times movement was itself perceived. The movement in such cases was described as slow and unsteady, drifting and oscillating like the bobbing of a cork upon a gently moving stream. Too few of these shifts and movements, which of course were subjective (neither the fixation-point nor *O*'s head, because of the biting-board, could move from the fixed position), were reported to justify any statement regarding a central tendency. Suffice it to note here that neither the direction nor the duration, insofar as our data warrant any statement, was constant: the direction varied to all points of the field, and the duration was at times short, at other times long.

Observers. The *Os* were Mrs. A. K. Whitchurch (*W*), a graduate student in psychology, and the author (*D*).[9] Both were highly practised in the observation of attributive clearness. *W* worked without specific knowledge of the problem. The *Os* served three times a week and always at the same hour of the day. The period of observation was approximately an hour in length. A rest period was offered whenever the *Os* demanded it, but at least one rest was taken during every hour.

Procedure. The following instructions were given to the *Os*: "At the signal 'Ready' fixate the spot of light before you; at 'Now' two areas of light will be shown; report which, if either, is the more clear. Give the position of the clearer as 'right', 'left', or 'above', below', whichever the case may be; if they are equally clear say 'same'; if you are uncertain, report 'doubtful.'" The experiments reported as 'doubtful' were immediately repeated. Though few in number such reports were given by each *O*.

A series of preliminary experiments was conducted by the method of limits to determine for each *O*, and for every one of the eight positions, the approximate intensity of the variable necessary for equality of clearness. Upon the basis of these experiments 5 stimulus-values were selected for each *O* for every position of the variable, and the main experiments were conducted on the principle of the method of constant stimulus-differences. The number of stimuli was later increased to 7 for *W* in positions 3 and 4, since it was found in both these cases that the values first selected were too low.

Twenty observations were taken at every position for every value of stimulus. At the beginning of every experimental hour a short warming up series was given. The procedure in the regular series was different for each *O*. *D* completed the experiments at one position before passing to the next. He always knew where to expect the stimuli. The positions were given him in the order 1, 2, 3, ...8. *W* on the other hand never knew what position the stimuli would occupy. The positions as well as the stimulus-values were given her in haphazard order.

Results. The results of the experiment are shown in the accompanying table, which gives for each *O* and for every position of the stimuli the per cent. of illumination necessary for the variable to be reported equal to the standard in attributive clearness.

TABLE I

Showing for Each Observer and for Every Position of the Stimuli the Per-cent. of Illumination of the Variable Necessary for the Report of Equality in Clearness.

POSITION	OBSERVER			
	D		W	
	% ILLUMINATION	PRECISION (h)	% ILLUMINATION	PRECISION (h)
1	9.9168	.0989	6.0989	.1540
2	5.0984	.1999	3.6399	.3233
3	20.5932	.0379	39.6936	.0723
4	18.8796	.0552	41.6792	.0697
5	28.0568	.0276	48.7870	.0434
6	44.6260	.0231	56.8570	.0540
7	10.1760	.0882	6.0086	.2242
8	15.6680	.0419	6.6383	.2059

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[9] My thanks are due to Miss Doris B. Dewey for serving as *E* while I acted as *O*.

These data, in spite of a large individual variation, show that position is for both *Os* a condition of clearness. The areas to the left and above the point of fixation (positions, 1, 2, 7 and 8) require less illumination to equal the standard in clearness than those below and to the right (positions 3, 4, 5 and 6). The most favorable position for both *Os* is position 2, the area to the left and 45° below fixation; and the most unfavorable is position 6, which is the opposite, to the right and 45° above fixation. In the first case the percent. of illumination necessary for the variable to equal the standard in clearness is 3.6% and 5.1% for *W* and *D* respectively, and in the second case it is 56.8% and 44.6% respectively.

The data show, furthermore, that position was more effective for *W* than for *D*. All *W*'s values are without exception more extreme than *D*'s. Whether this result is due to individual difference or to difference in the mode of procedure followed in the two cases it is impossible from the facts at hand to say. It is certain, as the results of the experiments mentioned above clearly show, that large individual differences obtain in these investigations; but the fact must not be lost sight of that *D* knew for every experiment where the stimuli would apear, and this knowledge may, in part at least, have been responsible for the difference. Whatever the effect of *D*'s knowledge may have been, we feel, in the light of the results, that the haphazard method of varying the positions is preferable.

Considering the difference of procedure followed with the two *Os*, the uniformity of their results is very marked. The data vary in the same direction and the rank-order of the positions in their 'effectiveness' is extremely close. The order for *W* is 2, 7, 1, 8, 3, 4, 5, 6; and for *D*, 2, 1, 7, 8, 4, 3, 5, 6. A correlation, as figured by Pearson's Method of Rank Differences, of .95 exists between the results.

The uniformity of the results leads us, in spite of the fact that they were obtained under special conditions and with but two *Os*, to conclude that position is a condition of clearness, and that the left-hand position and the position above have a distinct attentional advantage over the right-hand position and the position below.

Titchener, in his exposition of the conditions of clearness,[10] says that they "are of a kind to produce a powerful effect upon the nervous system," and that they have "biological significance." It is therefore perhaps worth noting that position, on both of these counts, takes its place with the other conditions of clearness. From the neurological point of view the effectiveness of position may be explained by the integration of the neural connections. The processes which lead from the rods and cones of the right retinas (those excited by stimuli to the left) center in the right hemisphere of the brain and there arouse excitations which, because right-handed people are left brained, are not interfered with by other excitations. Both of our *Os* were right-handed; so that this explanation would have added plausibility if it should prove, by extension of this experiment, that the right-hand position has an attentional advantage for left-handed people. From a biological point of view the most probable source of danger for primitive man was from above. The early reptiles and other gigantic beasts of prey struck down. This would account for the attentional advantage of the position above. Continuing this imaginative trend of thought, the advantage of the left may thus be explained: when man became man's greatest danger it was the weapon held in the antagonist's right-hand, and therefore appearing at the left, which demanded attention. Speculation, of course! But the admission of biological speculation merely places position once more in line with the other conditions of clearness.

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[10] *Textbook of Psychology*, 266 ff.

The results of this experiment corroborate those of Meads, Curtis and Foster, and Bowman, and show that position cuts across form and size because it is itself a condition of clearness. The conclusion that position is a condition of clearness also confirms that made by the author, in his study of "Attributive *vs.* Cognitive Clearness," "that certain areas are favorable for clearness".[11] Moreover, the positional disposition of clearness agrees closely with the perceptual distribution of attention, and it may well be that the latter is conditioned and determined by the former.

Summary. The results of this study may be summarised as follows: (1) position is a condition of attributive clearness; (2) the left-hand position and the position above have an attentional advantage; and (3) the most advantageous position of all is the position to the left and below the point of fixation.

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[11] *Jour. of Exp. Psych.*, 3, 1920, 183-230; esp. 205, 213, and 229.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

LXIII. THE PHENOMENOLOGICAL DESCRIPTION OF MUSICAL INTERVALS

By E. M. EDMONDS and M. E. SMITH

In this study we have sought to discover whether the experience resulting from the simultaneous presentation of two tones of different pitch can phenomenologically be distinguished from other similar experiences; and, in that event, whether a qualitative term can be found that shall adequately characterize it. Former experiments with similar processes have been directed towards their relative characterization by, *e.g.*, degree of fusion, of consonance, of pleasantness, of confluence, of smoothness, and the like.[1] In this investigation, however, we have been concerned with the possibility of an absolute characterization, a phenomenological description of the bitonal process in and for itself.

In preliminary experiments we employed as tonal material an harmonium, and we limited ourselves to the major and perfect intervals of the one octave c′—c″. The *Os* were two graduate students in psychology, *M* and *A*, both of whom were musically trained and knew intervals by their musical names; two members of the instructing staff, *H* and *Bi*, neither of whom had had musical training; and an undergraduate student in psychology, *Ba*, who played instruments by ear but had no technical knowledge of music. Our formal instructions to the *Os* were framed after preliminary experiments, in which we found that, although a phenomenological difference between intervals was immediately apparent, the *Os* had ready no words to characterize this difference, and further that the task was much easier of fulfilment if two intervals, the one of which served to 'set off' the other, were given successively. Our first instructions ran, therefore, as follows: "I shall give you in succession two tonal fusions. You are to try to find adjectives which shall characterize or describe them. You may select a word from the list given below, or you may furnish some other term which for you is more descriptive."[2] Our procedure, consequently, was to present every interval with every other as in the method of paired comparisons. In a single experiment each interval was sounded for 4 sec. with an interval between the pair of 3 sec. *O* sat where he could not see the keyboard, and upon the conclusion of the second interval characterized both intervals as immediately as possible. If *O* requested it, the two intervals were repeated.

Results of Preliminary Experiments. All *Os* had difficulty at first either in understanding the instructions or in assuming the phenomenological attitude. Eventually, however, after repeated trials, 4 of the 5 succeeded not only in finding terms that characterized every one of the 8 intervals, but also in recognizing by means of its quality any one of the intervals when presented in chance order. The fifth *O*, *H*, was at times able to find characteristic terms, but failed completely as regards recognition.

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[1] For the most recent study of this kind and for references to the earlier literature see C. C. Pratt, Some Qualitative Aspects of Bitonal Complexes, this JOURNAL, 32, 1921, 450.

[2] The list, which was typewritten and placed in the hands of the *O*, was intended merely to be suggestive; the terms were chosen by free association from words descriptive of similar patterns in other sense departments: smooth, rough (coarse), hollow (dilute), harsh, sweet, rich, astringent, pungent, gritty, sparkling, lustrous, radiant, and sombre. If necessary, the *O* was warned against the employment of aesthetic or affective terms.

The terms ultimately decided upon as best descriptive of every interval by every one of the 4 *O*s were as follows. *Octave*: *A*, "smooth, like the surface of window glass;" *Ba*, "smooth, like polished steel;" *Bi*, "smooth, a unitary experience like one note;" *M*, "smooth, like the feel of ice-cream in the mouth, or the touch of polished glass." *Major Seventh*: *A*, "astringent, like the taste of a persimmon;" *Ba*, "gritty, like the feel of small sharp granular objects;" *Bi*, "astringent, like strong vinegar or alum;" *M*, "harsh, a nippy, biting effect like a strong astringent." *Major Sixth*: *A*, "sweet, satisfying, with no sustenance;" *Ba*, "astringent, intermittent puckeriness associated with roughness;"[3] *Bi*, "rich, a lusciousness;" *H*, "luscious, like a juicy mellowness;" *M*, "sweet, like the flavor of fruit, softly rich in the sense of flavor." *Perfect Fifth*: *A*, "hollow, like a hollow tree, no body to it;" *Ba*, "hollow;" *Bi*, "hollow, empty, has no body;" *M*, "hollow, like a hollow tree or a cave," "like a sound produced by tapping on a hollow wooden box." *Perfect Fourth*: *A*, "rough, like the feel of coarse sandpaper;" *Ba*, "harsh, like the feel of a coarse, granular surface;" *Bi*, "harsh, with a little astringency, thick and heavy;" *M*, "a certain coarseness, rich, oily, hard, lumpy." *Major Third*: *A*, "mellow, like biting into a very ripe apple;" *Ba*, "thick, like bean soup;" *Bi*, "luscious, like peaches to the taste;" *M*, "mellow, like sweet fruit or a soft diffused light." *Major Second; A*, "gritty, like the feel of small pebbles in the hand or grapenuts in the mouth;" *Ba*, "rough;" *Bi*, "gritty, like sand in one's teeth;" *M*, "gritty, like sandpaper."

At this stage of the investigation it became apparent that the intervals within the octave have in fact a perceptive 'quality' which our *O*s could roughly characterize, that there was fair agreement among the *O*s as regards the nature of this 'quality', and that the interval could be instantly recognized in terms of its 'quality.' Trial experiments made with the harmonium in other octaves seemed to indicate that the character of the interval was much the same elsewhere. We thought it advisable, however, to continue the experiments with relatively pure tones, and to increase, as far as our materials would allow, the range of frequencies. We shall, therefore, postpone discussion of the results of the preliminary experiments until we have described our further experiments.

Results of Principal Experiments. In this second group of experiments we used a set of König forks mounted on resonance boxes. The frequency of the forks ranged from Ut^3 (512 vs.) to Re^5 (2304 vs.).[4] In the first octave, Ut^3–Ut^4, we had forks which gave all the harmonic intervals, and in the next octave we had Mi^4 (1280 vs.), Sol^4 (1536 vs.) and Ut^5 (2048 vs.). By combining these forks in various ways we were able, therefore, to obtain 4 intervals of the octave, 2 of the major seventh, 2 of the major sixth, 7 of the perfect fifth, 5 of the perfect fourth, 4 of the major third, and 6 of the major second. Of these 30 intervals 21 were true harmonics, 6 failed of being true by 1/10 of a vibration or less, and of the remaining 3 we discarded a sixth (Ra^3–La^3) and a second (Ra^3–Mi^3). Another interval of the second (Sol^3–La^3), which we retained, failed of being a true ratio by 10.7 vs., but no difference in 'quality' with respect to other seconds was reported.

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[3] Both the sixth and the seventh, especially when given by the harmonium, arouse in some *O*s a muscular reaction which is characterized as a "twist or contortion of the body", "a wry face." *Ba* was apparently unable to dissociate this muscular reaction from the quality of the interval.

[4] These are, of course, single vibrations.

The apparatus designed for the simultaneous actuation of a pair of forks consisted of a platform set between two iron supports upon which an axis rotated. Two felt piano-hammers were clamped to this axis in such fashion that they could be adjusted to strike the two forks by means of a quick rotation of the axis. Across each end of every resonance box on the bottom was glued a strip of saddler's felt ½ in. thick; the platform was covered with coarse sandpaper. When, therefore, the forks on their resonance boxes were placed on the platform directly in front of the two hammers, the felt strips served not only to prevent secondary resonance but also, by friction with the sandpaper, to keep the forks in position when struck by the hammers. Care was taken so to adjust the hammers that the forks were struck simultaneously, with equal intensity, and with the avoidance, so far as possible, of partial tones.

The *Os* were *A*, *Bi*, and *M*, of the previous experiment. To these was added *W*, a member of the instructing staff who had full knowledge of the investigation. The instructions were: "I shall present to you a tonal experience and I want you to characterize this experience as best you can with a single word. We give below a list of terms as suggestive. You may take any one of these if it characterizes the experience, or you may substitute one that seems to be more accurately descriptive." The procedure was as follows: after a 'ready' signal the forks were actuated, and before either tone had died away they were restruck, until *O* had given a judgment which he thought was satisfactory. The forks were then damped, *O*'s report was written, and a new interval was arranged. The stimuli were presented in 3 groups of 5 series each. In Group 1 every one of the 5 series was presented in the order second, octave, seventh, fourth, sixth, fifth, and third. In Group 2 the intervals were chosen at random, and in both groups all intervals were within the range of the octave (Ut^3—Ut^4). In Group 3 the serial order of intervals was the same as in Group 1; the intervals, however, were within the ninth (Ut^4–Re^5).

The characterization of the various intervals was, on the whole, the same as in the harmonium experiments. In general there was an increase in the number of analogies and in particular of analogies of the taste-touch order. The *octave* is still described as "smooth, like the feeling of ice-cream in the mouth;" "smooth, like molasses in the mouth;" "a smooth feel in the mouth, but I have no likeness for it." The *seventh* is, as hitherto, said to be astringent by all *Os*; *M*, however, found "no definite likeness in the mouth;" it was rather "harsh or blurred, like the feel of harsh fur;" and *A* similarly reports "dilute, sour, a little rough; a soft roughness, like a lamb's wool powder puff, with some body to it." The *sixth* is again characterized by all *Os* as "luscious, succulent, rich, like rich pudding-sauce in the mouth; sweet thick, like sugar syrup." The *fifth* proved to be the most difficult of all intervals, particularly as regards its differentiation from the fourth. It was described as either "thin, dilute, like a clear soup," or "hollow, like a hollow space," or again as "rough or coarse rough, like the feel of tweed suiting," or "hollow rough, like the sound of a husky voice." The *fourth*, on the other hand, was said to be "rich, like the richness of cloth texture, velvety;" "rich, like polished ebony, shining, but dark;" "rich, like rich, crumbly cake in the mouth;" or "lustrous, like shining metal;" or again, "harsh, like a ball of crushed paper in the hands"; "harsh rich, like the feel of cotton wool in the hands"; "rough, like coarse sandpaper to the hands." The *third* is uniformly described as "mellow, like ripe fruit;" less frequently, as "sweet, like hard sweet peppermint candy in the mouth;" it is similar in character to the sixth, but not as "thin," as "juicy," as the sixth. The *second* is in general "gritty, like the feel of wheat in the fingers, or dry corn flakes in the mouth;" or again, "pebbly, like round pebbles in the hand;" or "grating, like the feel of a nutmeg grater or the feel of sandy spinach in the mouth;" occasionally it is confused with the seventh as astringent.

At the close of the three regular groups we gave a comparison series in which the *Os* were presented first with an interval in a lower octave and then with the same interval in the higher octave, and were asked to indicate any difference, if it occurred, between the qualities of the two intervals. As a result of these experiments, no change was found in the smoothness of the *octave*; the higher interval was said, however, to be more dilute, brilliant and clear. The astringency of the *seventh* was unchanged, the upper seventh differing only in body or volume. No change was found, again, in the lusciousness of the *sixth*; it was described, however, as brighter in the higher than in the lower octave and also as having less volume. The hollowness, coarseness, harshness of the *fifth* was unchanged. The upper interval, however, had less body and warmth than the lower. The roughness of the *fourth* and of the *second* was in both cases finer in quality; the higher interval of the second, for example, was said to be a finer grit than the first, the pebbles were sharper to the feel. The mellowness of the *third* was unchanged. It was, however, said to be smoother in the upper than in the lower interval.

We may, therefore, summarize the characteristic qualities of these intervals as follows: the octave smooth; the seventh astringent, sharp-rough, hollow-astringent; the sixth luscious, juicy-mellow, succulent; the fifth dilute, hollow, harsh; the fourth rich, harsh, coarse; the third mellow, sweet (to taste); the second gritty, grating.

It will be observed that the terms employed belong to three different categories. We may, first of all, characterize the various intervals by reference to taste-touch fusions. Taking the intervals in order from octave to second they would be smooth, like smooth ice-cream or smooth molasses; astringent, like the feel of a green persimmon; luscious, like a juicy, succulent fruit; dilute, like a clear soup; rich, like whipped cream or like ripe canteloupe; mellow, like a ripe but not juicy fruit; gritty, like corn flakes or other coarse substance in the mouth. Or we may characterize them in terms of touch fusions, in which case we should have, taking the intervals in the same order, smooth, like polished glass; harsh, like fine sandpaper; no term for the sixth; coarse, like tweed suiting; harsh, like crushed paper or crash; no term for the third; gritty, pebbly, like pebbles in the fingers. Finally, the interval of the fifth was most frequently described as hollow. We believe that this term had reference to the sound of the experience. It is the only descriptive term in our series which belongs to an auditory category.

Attitude. At the close of our experiments we wished to characterize the attitude under which the above observations were made. It will be recalled that at the outset our *Os* had difficulty in fulfilling the requirements of the instructions and also that one *O*, *H*, was unable to describe the intervals with any degree of consistency. These facts point to a characteristic attitude for the observation, and we desired to know, if possible, what the attitude was. Accordingly, we presented two series of intervals to every *O* with the instructions to listen to the experience and to report the descriptive term as before, and then to state as best he could what he actually did in making the report. Typical statements of the *Os* are as follows. *A* says: "the experience came as a whole, as one bit of experience, and the term gritty came at once with a gritty feeling in my mouth;" "heard as a whole, got feeling of ripe peach in my mouth, mellow is a quick judgment;" "got two tones coming together into fusion, lost completely the feeling of mellowness." *M* reports: "the sound comes as a single experience, then I get roughness;" "unitariness of experience impressed me first, I didn't hear out the two tones until after the judgment;" "experience came as one thing, musical association came next; I repressed this to get psychological attitude, then got judgment of lusciousness;" "I got total experience, then doubt as to term to use, next I heard out the two tones, filled in a third between, and recognized the interval as the fifth, did not get judgment of characteristic quality." *Bi* reports "one piece of experience, a mass of tonal experience, lumpy, pebbly;" "came as unity, with a feeling of a sheet of smooth paper, so unified that after judgment I heard out the two tones to make sure they were both there;" "unitary experience, buzzing and harsh, like hand going over coarse cloth;" "mellowness came first with unitary experience before realizing that two tones were present; I don't get good mellowness unless the two tones are equal in intensity; if I hear out the two tones the mellowness goes; analysis robs it of its mellow quality." It will be noticed that the three *O*s agree that the sound comes as a unitary whole, that its 'quality' seems somehow to inhere in it, *i.e.*, the sound comes as mellow, as smooth, as lumpy, etc., and that any attempt to analyze the total experience into its components changes the nature of the experience in the sense that its 'quality' disappears. The reports of *H* in the harmonium experiments show that his attitude was of the analytic sort. For example: "upper note stands out, but the beating tone is very close to the upper in pitch and fuses more or less with it;" "this interval is very uniform in all its dimensions, it goes straight on, it is rather small;" "small in volume; two tones, very much alike in pitch; within the complex is a very rapid beating tone, which is rough;" "almost complete integration of the tones, although both tones are there; this integrative tone increases and decreases at a slow rate; it may be an alternation in volume, or perhaps in intensity;" "rapid beating tone that lies pretty close to the upper primary, but is not the same in pitch; the upper tends to fuse with the beating tone, giving it body." Since, therefore, in these experiments the phenomenological characterization required, not an analysis of the complex experience, but rather the taking of it as a whole, the reason for *H*'s difficulty is apparent.

Conclusions.—Our conclusions cannot, of course, be more than tentative. We have found, however, that it is fairly easy to fit to bitonal fusions characteristic 'qualitative' names, derived from taste and touch, which make recognition of the fusions possible. There is much closer agreement among *O*s than we had anticipated. The names are overtly analogical, but they perhaps represent the sort of characterising names that ordinary language would have imposed if the fusions in question were natural and common parts of our everyday perceptive experience.

LXIV. THUNBERG'S ILLUSION

By Ida Bershansky

Thunberg, in a discussion of cutaneous complexes, remarks that a cold sensation which is not accompanied by a sensation of contact is frequently apprehended as caused by a liquid. This interpretation may, he thinks, be explained by the fact that, when a part of the body is submerged in a liquid, the pressure sense remains practically unexcited, so that the temperature sensations aroused are approximately unmixed with pressure. In this connection he describes an illusion of wetness which, he says, appears when the forehead is stimulated with a very cold substance for an interval of 20 sec. After the removal of the stimulus the cold continues for some time, during which the skin of the forehead is distinctly sensed as wet.[1]

These statements are opposed to results obtained in other experiments. Tung, for instance, points out that Helmholtz and later Bentley declare that wetness is occasioned by a simultaneous stimulation of cold and pressure, and Tung himself obtained the experience of wetness by simultaneous stimulation of a cold and a pressure spot. We have thought it advisable to make a systematic study of Thunberg's illusion, in the hope that we may not only remove the discrepancy, but also describe the perception of wetness itself more definitely than Tung, under the conditions of his experiment, was able to do.

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[1] T. Thunberg, in Nagel's *Handbuch der Physiologie des Menschen*, III, 1905, 708. The original passage is quoted by S. Tung, this JOURNAL, 32, 1921, 421.

Our experiment falls into three parts. The plan of the first was in an initial series to study the course of the pressure experience under Thunberg's conditions, but with temperature reduced to a minimum, and in particular to see whether any after-sensations of pressure might appear in the period immediately following the removal of the stimulus; and then, in a second series, to admit a cold with the pressure stimulus and in the same way to study the course of the resulting experience. Since Thunberg gives no hint of the intensity of the touch stimulus, we employed stimuli of 6 different weights; these consisted of circular discs of brass of approximately 1, 2, 3, 5, 10, and 25 gr. in weight. The diameters of the first two were 1.3 cm., and of the remaining discs 1.9 cm. Since, also, we desired to use the same stimulus objects in both series of this group of experiments, we attempted in the first series to reduce the cold quality of the metal by (*a*) covering the face of every disc with felt, (*b*) warming the discs by holding them in the closed hand, and (*c*) warming them slightly in the rays of an electric heater. In the second series the discs, with felt removed, were chilled by placing them in a bath of cold water, the temperature of which ranged from 16° to 19° C. In order to make the stimuli easier of manipulation and more uniform of application, and at the same time to eliminate any arousal of warmth by the hand of *E*, the weights were lifted by means of a cord, 10 cm. in length, which was inserted and wedged in a small hole drilled in the center of every disk.

O reclined comfortably in a Morris chair with the head thrown back so that the forehead was in a horizontal position. Preliminary trials showed that, if the stimulus objects were placed near the eyebrows, eye-movements were set up which interfered with the course of the pressure experience; we therefore stimulated the upper forehead as near to the roots of the hair as was possible without exciting them. The *Os* in this group of experiments were *B* and *W*, two members of the instructing staff, and *M*, an undergraduate specializing in psychology. In order to shorten the period of report we used two instructions. The first limited the report from the time the stimulus was removed until all after-sensations disappeared, and read as follows: "I shall give you a cutaneous stimulus on the forehead, and then, during the course of the resulting experience, I shall give a signal 'now.' You are to report the qualitative and intensive course of the experience which ensues after the signal." The second instruction by appropriate signals restricted the report to the interval between the moment of stimulation and 10 sec. after the stimulus was removed. *E* sat behind *O*, and every stimulus of each series was presented 8 times over in chance order.

Results of the First Part of the Experiment. In describing the results of these experiments we shall combine the reports received under the two instructions and, further, since there was a typical difference in the course of the pressure experience resulting from stimulation with the discs of 1, 2, 3, and 5 gr. on the one hand, and of 10 and 25 on the other hand, we shall refer to these groups as the lighter and heavier stimuli. With the lighter stimuli the course of the "pressure" experience was in general described as (1) a light pressure or tickle, (2) a weak, filmy cold or "cool dampness", (3) a weak pressure which either faded out or else continued until the stimulus was removed, and (4), after the removal of the stimulus, either "nothing", or a faint contact or tickle, which gradually disappeared. With the heavier stimuli the course was (1) an intensive pressure, (2) a cold, wet pressure, or a continued pressure, (3) wet, and (4), after the removal of the stimulus, an areal pressure which either gradually died out or else fluctuated in intensity until it finally disappeared. Occasionally, however, immediately after the removal of the stimulus, there was a short interval before the appearance of the pressure when "nothing" was felt. It will be observed that with both groups of stimuli coolness, cold, dampness, and wetness were reported, despite our efforts to eliminate the temperature factor; dampness appeared only with the lighter stimuli, wetness with the heavier. It will also be noticed that after the removal of the stimulus dampness or wetness never appeared; that the after-pressure with the lighter stimuli was very weak; and that with both groups of stimuli there were instances when no after-sensation was observed during a brief interval following the removal of the stimulus.

With the cold stimuli, the course of the experience following upon the lighter stimuli was exceedingly variable. In general, however, we may say that the course was (1) pressure, (2) cold, (3) cold damp, (4), after removal of the stimulus, either nothing at all or light pressure, or again faint coolness, occasionally a faint, cool damp, and (5) cool dampness or faint pressure that gradually faded out, sometimes alternating with a faint coolness or cool dampness. Except with the 1 gr. stimulus, when no after-sensations of any character are sometimes reported, the 'empty' interval is usually followed by cool damp or by faint pressure. With the heavier stimuli the reports were less variable. The most frequent course was (1) cold pressure, (2) wet, cold wet, or cold which continued until the stimulus was removed, then (3) cold pressure or wet, which gradually became less in intensity, sometimes alternating with cool or pressure and as a rule ending with a faint pressure or tickle.

The outstanding result of the investigation thus far is the exceeding variability of the reports. Although there is abundant evidence of the presence of an after-sensation of pressure in the period following upon the removal of the stimulus, which would seem to bring the 'illusion' into line with other investigations, it is plain that we have not obtained the compulsory conditions, if there are any, for the perception of wetness; and, furthermore, we have insufficient data as regards the nature of the integration. We decided, therefore, at this point, to continue the investigation with other *Os*; and, since the lighter group of stimuli gave the most variable results, to discard them and employ in the future only the heavier stimuli.

Results of the Second Part of the Experiment. In the second part of our investigation the stimuli were three in number and were of the same material, shape, and diameter (1.9 cm.) as before; they weighed 20, 25, and 30 gr. The *Os* were *G* and *H*, instructors in psychology, and *Br*, a graduate student in psychology. The procedure was the same as in the earlier experiments, with the exception that a mechanical instead of the manual method of applying and removing the stimulus was employed. The instructions were unchanged. For the cold-pressure series the stimuli were chilled in ice water, the temperature of which ranged from 11° to 15° C. The experiments were performed in July and August, when the coolness of the metal gave us less trouble in the pressure series; temperature was practically eliminated by the felt cushion which we employed in the earlier experiments.

In the pressure series the course of the experience was practically the same for all *Os*. It began with (1) a neutral pressure which changed to (2) a sub-cutaneous quality (drag or ache), and then for two of the three *Os* was frequently tinged with (3) a slight coolness. Following the removal of the stimulus there was (4) a sudden drop in intensity of the drag or ache quality, and then (5) an uniform decrease in intensity and change in quality from a subcutaneous to a cutaneous pressure.

In the cold-pressure series the typical course was as follows: (1) fairly intensive pressure, (2) cold or cold wet, and (3), if (2) were cold, cold wet until the stimulus is removed. Immediately upon the removal of the stimulus the wetness disappears and either a pressure (2 *Os*) or a cold pressure (1 *O*) follows. Thereupon (4) the course is either (*a*) pressure, cool wet, cool; or (*b*) pressure, cold, wet, cool; or again (*c*) cool pressure, wet, cool. It will be observed that the qualitative course after the removal of the stimulus is with one exception the same as that during the period of stimulation. The difference between the two periods is a difference in intensity and in the final quality. The fact that the end-stage of these experiments is cool, whereas the final stage of the previous experiments was usually pressure, is probably conditioned upon the greater coolness of the stimulus in these experiments. It will be seen also that wetness is almost invariably preceded by a pressure which changes to cold or else by a cold pressure, and that ultimately wetness breaks up either into cool or, as in the earlier experiments, into pressure.

Results of the Third Part of the Experiment. Before discussing the results as a whole we wish to report the third part of our investigation, which was concerned with the appearance of the illusion on other parts of the body than the forehead. It will be recalled that we began by stimulating the cutaneous surface of the forehead, because that locality was one of Thunberg's conditions; but a systematic study of the 'illusion' such as we planned would be incomplete without reference to other parts of the cutaneous surface.

We employed the same stimuli, the same procedure and the same instructions as in the second part of the experiment. The *Os* were *Br*, who had served in the previous experiment, and *S*, an undergraduate student specializing in psychology. The parts selected for investigation were the forearm, the leg just above the knee, and the middle of the back.

In general it may be said that the illusion occurs on the four parts of the body investigated with approximately equal frequency. The average percentage of occurrence in these experiments for the two *Os* was: on the forearm 87.5, on the leg 94, on the back 95. The experience, however, was reported by both *Os* as more vivid on the forearm and back than on the leg; the pressure, in particular, was less intensive and adapted more quickly. As regards the general course of the experience the results of these experiments merely substantiate those of the earlier experiments, and we have therefore nothing new to report.

Discussion. We now turn to a discussion of the two problems with which we originally set out; first, an explanation of the Thunberg illusion in terms of other experiments of a similar nature, and secondly, a more definite description of the perception of wetness. As regards the former, it seems clear from our results that the illusion as described by Thunberg is an integration of after-sensations of cold and pressure. In support of this interpretation we have the following evidence. (1) In the pressure series with the heavier stimuli after-sensations of pressure are almost invariably reported immediately upon the removal of the stimulus. (2) In the cold-pressure series with the heavier stimuli either pressure or cold pressure is again almost invariably reported before wetness. (3) With weak stimuli pressure is as frequently reported immediately upon the removal of the stimulus as 'faint coolness,' and both more frequently than damp. (4) There is a decided correlation on the one hand between weak intensity of stimulus and dampness, and on the other hand between strong intensity of stimulus and wetness. In other words, the difference between dampness and wetness seems to be referable primarily to a difference in degree of intensity of pressure. The reason for Thunberg's failure to observe the after-sensation of pressure will, we think, be apparent when we have discussed the nature of the experience of wetness.

This experience may perhaps be best described in the words of *H*. "Psychologically wet is a compact cold; whenever wet becomes less compact, wetness disappears and, in general, a diffuse cold is left." It is, apparently, the pressure quality which gives the compactness to the cold, and under a psychological attitude wetness may usually be analyzed into cold and pressure. When, however, the cold is exceptionally intensive, it is exceedingly difficult to obtain a clear experience of pressure; and since, as Tung observed, the perception of wetness requires the greater clearness of cold, it is not strange that the pressure should, in Thunberg's observations, have been overlooked. Wetness itself is a meaning put upon the integration of cold and pressure; and as a rule this meaning is associated with some object; *e.g.*, "a wet which moved on my forehead," "a fleeting wet which ran down the forehead," "it is like a drop of water." Dampness, on the other hand, is psychologically a less intensive pressure and perhaps also a less intensive cold; the integration is a less compact coolness, and the object-meaning of dampness is, accordingly, "a light breath of damp air," "being touched lightly by a wet cloth," "like the brush of a slightly wet feather."

Summary. The Thunberg illusion occurs with approximately equal frequency on the forehead, forearm, leg and back.

Wetness is a perception which psychologically is an integration of pressure and cold, the latter having the greater intensity and clearness.

The 'illusion' of wetness is conditioned upon after-sensations of pressure and of cold.

SOME RECENT PSYCHOANALYTIC LITERATURE

By G. Stanley Hall

The New Psychology and the Teacher. By H. Crichton Miller. N. Y., Seltzer, 1922. pp. 225.

The purpose of this book is to "help the teacher to gain something of the analytical point of view and having done that to draw his own conclusions." The author does not restrict himself, however, to psycho-analysis, but covers a certain portion of the older psychology. He deplores the divergences of the Vienna and the Zürich school, finding that almost diametrically opposite philosophies underlie them, Freud being a thorough-going determinist while the doctrine of free-will is implicit in all of Jung's work. These differences justify a detached and critical attitude, and it is a matter of regret, though not of surprise, that this justification is not recognized by the founders of either school, and that they follow the example of most pioneers in resisting compromise and criticism alike.

Educationists above all need freedom of criticism, for their interest is focussed at a point where many paths meet: art and philosophy; body and mind; memory and imagination; science and religion,—to name only a few of these paths. There is no panacea or master-key, and this book is simply the contribution of a physician who would not be dogmatic, but is profoundly convinced that his sphere of action is and must always be of secondary importance.

The application of psychological methods to the cure of nervous disorders is to their application in education as the cure of consumption is to its prevention; and the latter is largely due to the efforts of those who understand something of the laws involved in its treatment, though it is not necessary that they should have been patients in a sanatorium. Nearly every town-dweller, though healthy, harbors the tubercle bacillus, and so every educationalist, though well adjusted, harbors repressions that are potentially harmful. Again, everyone engaged in the prevention of phthisis would profit from the hygienic measures that constitute his propaganda. So there is not a school-teacher or parent who would not profit in his or her own mental life from the principles of mental hygiene which this volume is meant to utilize. The work, too, of preventing tuberculosis is too vast to be left to sufferers from this disease alone; and so the applications of analytic psychology to the needs of the young are too urgent and too extensive to be committed to the few who have had a nervous breakdown or have undergone treatment. One anxious for the well-being of the new generation cannot prevent impatience with the obstructionists. Analytic views have, in fact, spread so rapidly in the last eighteen years that the reactionaries will soon be negligible.

But the most serious obstruction, as so often happens, comes from within, *viz.*, from the jealousy of pioneers and their followers who, while wishing to safeguard the movement, proclaim that no one can heal who has not himself been healed, that no one can initiate who has not been initiated, and that no one can preach who has not been ordained. These should heed the lesson of the New Testament: "Master, we saw one casting out devils in Thy name, and we forbade him because he followed not us." What is needed is that teachers should recognize that no creed or initiation can qualify, but that the vision which allows us to perceive the child's needs and difficulties and potentialities is what is mainly needed; and this work is designed in some measure to contribute to this clearer vision on the part of teachers who are yet content to be learners. The chapters, after the Introduction, are: authority and suggestibility, reality and phantasy, emotional development: the boy, emotional development: the girl, the unconscious motive, mental mechanisms, dream symbolism, the herd instinct and the herd ideal, and educational methods.

Percy Bysshe Shelley: *An Introduction to the Study of Characters.* By THOMAS VERNER MOORE. Edited by E. A. Pace of the Catholic University. Psychological Monographs Series.

The author is not entirely satisfied with the analysis or definition of character given by Thorndike and his pupils, although more or less in sympathy, and taking his departure from them. He recognizes that the traits generally shown have not yet been, and perhaps cannot be, analyzed into their lowest terms so as to make them universal and everywhere acceptable; and he thinks therefore it is somewhat premature to attempt classification, but believes this must wait for the data of the kind he here undertakes. He assumes that poetry is, in general, a cryptogram to which the biography of the individual supplies a key.

Shelley, he believes, has some of the traits of dementia praecox as interpreted by Kraepelin and even Jung. Perhaps every man has his characteristic trend—praecox, manic-depressive, hysterical, etc. He takes recourse to his own conception of psychotaxes, which constitute the most characteristic elements of personality, determining how a man will act in emergencies, and thinks they may be classified into those which present no solution and those that provoke the defence-reaction and the compensations and sublimations. He is in sympathy with Adler's imaginary life-goals.

Shelley's chief attitude is one of revolt, and this was determined very largely by his bitter negative attitude toward his father, who abjured him although he was his eldest son. This made him sensitive to, and ready to find tyranny in, almost all the aspects of life. But wisdom and justice warring against tyranny were not enough for his romantic heart. He had to have someone who could understand his aspirations in their details; and so he turned first to Harriet and then, when she was utterly unsatisfactory, to Mary, with whom he led an almost gypsy life. His nature was a strange blend of physical and ideal, but he could ardently hold to nothing that did not involve conflict with the world about him, and in this his praecox disposition was most apparent. He was in conflict not only with his father and the masters at Eton but also with his schoolmates, who fagged him unmercifully, while his masters always misunderstood him.

The author has evidently carefully studied each of the several biographies of Shelley and his poems, and makes a pretty clear case that the latter express sentiments and experiences which were actually lived in a very vital way. Prometheus Unbound was Shelley himself; Jupiter was the tyrant of the world against whom he was in Satanic revolt.

The positive drive in Shelley's life was to find a woman who both loved and understood him, but none of them realized his ideal. His yearning for sympathy, stimulated of course by the bitter criticism to which he was subjected, gave him his patheticism, which appears in overdrawn pictures both of his sufferings and of the abuses to which he was subjected. This appears not only in his poetry, but apparently bred delusions in his soul, as of the insult and attack in Pisa by an officer who only knew his name.

The Psychology of Medicine. By T. W. MITCHELL, M.D. London, Methuen and Co. pp. 187.

Although this little work is designed mainly for beginners, there are chapters in it which will interest adepts as well. It is characterized by all the lucidity and impartiality for which the author is widely known. He begins with the theory of dissociation in hysteria and hypnosis, and these subjects are treated historically. Then follows repression, discussed from a very general point of view, and here and in most of the rest of this work he follows, in the main, psychoanalytic lines. Psychoanalysis is both a body of doctrine and a method of treatment, and is based mainly upon the theory of the unconscious and the neuroses. He discusses fully enough the differences of the sub-, co-, and un-conscious and the subliminal in their relations to one another. Perhaps the chapter on the neuroses, hitherto one of the most obscure themes, will be, on the whole, most helpful to all readers.

The book is by no means partisan, but treats sympathetically the work of the post-Freudians,—Jung, Adler, Silberer, and Maeder. This breadth of view would be more worthy of note in an American psychoanalyst, but he well and wisely insists that Freud's work is the foundation for all subsequent analytic doctrines. It is essentially a compilation, condensed and impartial, and the author makes no attempt to add to the views discussed or even to state very definitely his own opinions.

The Homosexual Neurosis. By WILHELM STEKEL. Tr. by J. S. Van Teslaar. Boston, Badger, 1922. pp. 322.

The first portion of this work was put into English by the same translator under the title *Bisexual Love*. The third part, entitled *Autoerotism*, is soon to appear, so that this is really the middle one of three volumes. One gathers pretty well from it, however, the author's point of view. He first treats the various relations of the homosexual to the other sex with an excellent epitome of views upon the subject. The second chapter deals with the rôle of the father and other members of the family. Then follow chapters on its relations to jealousy and of the latter to paranoia, the relations to sadism, and a final chapter on the neurotic's inability to love. This volume is rich in allusions to the author's wide clinical experience, and some of these cases are given *in extenso*. Stekel is one of the most original of the Austrian savants in this field who, while working in absolute independence of Freud, has never ostensibly broken with the Master, who seems to be showing an ever more feudal spirit toward those he has inspired who have differed with him on any points he has deemed essential.

The Psychoanalytic Study of the Family. By J. C. FLÜGEL. London, International Psychoanalytic Press, 1921. pp. 259.

Starting with a quotation from Plato's Republic, Book IX, which might almost have been a summary of modern psychoanalysis, the author treats very fully the relation between parents and children, brothers and sisters, while that between husband and wife is but lightly touched upon because psychoanalysis has less that is new to contribute here. In fact sex and marriage cannot be understood by a preliminary knowledge of the nature of the bond that unites parent and child. The author does not discuss the size of families; and his book, especially in the early part, claims to be only a compilation or epitome (and it is an excellent one), while in the latter, practical part he is more original. Here he brings out very well the necessity of family love, but stresses rather more, we think, than the facts justify, although they do go a good way towards supporting his claim, the necessity of later revolt from the influence of parents in the interests of progress.

Some Applications of Psychoanalysis. By OSKAR PFISTER. London, Allen and Unwin, 1923. pp. 352.

It is the aim of this book to show what psychoanalysis is and how it may be applied to the various spheres of human activity. It does not pretend to be an exhaustive treatise, but only to introduce the reader to a method of investigation which has a great future before it, and which some think will cause a radical revolution of our whole cultural thought. After discussing psychoanalysis as a method, then as an art, and showing its sphere in peace and in war, the author treats it as a philosophy, as a method with children, and in its relations to missionary work, the last part of the book being perhaps, on the whole, the most original, for he believes that its right use would immensely accelerate the process of evangelizing the world. This is the tenth work of the author to be put into English and his fifth large book.

Psychoanalysis in the Service of Education. By OSKAR PFISTER. London, 1922. pp. 176.

This book is addressed to teachers by the man who has done more to apply Freudian principles to education and also to pastoral work than any other. He believes that the moral, intellectual, and religious life of the rising generation would be greatly improved by a judicious use of paidology. Today we can descend into the subliminal regions where the threads of destiny are spun by gray-haired fates. Our conquest here may be compared to that of the air. Even Stern, who is opposed to Freudian analysis for children, believes in an analysis of his own, which is essentially that of Freud without sex. Flournoy and Claparède and others have felt it necessary to instruct teachers in the possibilities here. The unconscious is the world of the spirits of the world. Abundant spiritual life is buried there and cries out for deliverance. Those whose eyes are open see endless miseries caused by repressions, which educators do not understand. The hearts of those who thus descend into the kingdom of the shades, and set free fettered souls, must be pure.

Die Besessenheit. By T. K. OESTERREICH. London, 1921. pp. 403.

This Tübingen professor of philosophy here gives us a monumental study of possession which may fairly be called encyclopaedic in its range. He gathers his data from classical, religious, anthropological sources with great diligence and fairness, discussing first the nature of the study of possession and then the distribution of its phenomena and their import for the psychology of religion. He finds spontaneous possession among savages, *e.g.*, the North Asiatic Shamanism, and in it all emphasizes the possibilities of autosuggestion and a need for more detailed psychological observation. He admits parapsychical and even parapsychophysical phenomena. But it is somewhat disappointing that he gives us no theory, although from some passages and from his discussion of the Piper case one would infer that he has some sympathy with the English Psychical Researchers. He contributes several very interesting observations of his own.

Introductory Lectures on Psychoanalysis. By S. FREUD. Authorized translation by Joan Riviere. With a preface by Ernest Jones. 1922. pp. 395.

Freud was evidently not satisfied with the American translation of this book, published nearly two years ago, although it was made by an American relative of his; so that here we have another rendering of it into

English. The work is interesting and covers a wider field than any of Freud's previous works, so that it is well adapted to be regarded as an introduction to the subject for beginners. It does not, however, attempt to cover the whole field of psychoanalysis, dealing only with the psychopathology of everyday life, dreams, and the neuroses. Freud begins in a way so elementary that those familiar with the system will perhaps be impatient and expect little from what follows, but, as one reviewer has remarked, he has several different styles, a condensed and a more elaborated and a purely esoteric style, and all these are found in this book, so that the latter part of it takes the reader into the very profoundest depths of his *Tiefenpsychologie*.

The Psychology of Misconduct, Vice, and Crime. By BERNARD HOLLANDER, M.D. London, Allen and Unwin, 1922. pp. 220.

The author has had twenty-five years of experience as a practitioner with nervous and mental diseases, and records the results of his reflections. There are relatively few references to literature, and it would seem a little as though he were what the Germans call an autodidact or self-taught. He treats the bases and causes of misconduct, drink and drugs, acquisitiveness, ill-temper, violence, morbid suggestion, cunning, theft, fear of consequences, sex perversions, the egoist, autocrat, the morally weak-minded, responsibility, treatment of moral failings. The author's style is vigorous, lucid, and simple to a degree, and there is a freshness about his impressions, which are first-hand, that lifts the book distinctly above the commonplace. It is throughout sane, broadminded, and should be of assistance not only to physicians but also to laymen interested in abnormalities.

The Psychology of Everyday Life. By JAMES DREVER. 3rd edition. London, Methuen, 1922. pp. 164.

This title, as a perusal of the fourteen chapters shows, is to some slight extent a misnomer, or at least may be misleading to those who assume that the author is a follower of Freud. To be sure, the attention he gives to the Freudian work that might be characterized under this name is sympathetic, but it is very slight. He does discuss appetites, instincts, emotions, moods, sentiments, play, imagination, tastes and flavors, sound and colors, perception, illusions and hallucinations, etc., in a way that on the whole does justify his title. The book is untechnical and is well suited to the general reader. The author is influenced more by Lewes' psychology of everyday life than by Freud, although his acknowledgments are to McDougall, Stout, Titchener, James, etc., as well as to the Freudians.

Taboo and Genetics. By M. M. KNIGHT, I. L. PETERS and PHYLLIS BLANCHARD. Moffat, Yard & Co., New York, 1920. pp. 301.

This book attempts to summarize our natural knowledge of the biology, zoology and psychology of the foundations of the family institution and at the same time to suggest a starting-point for a more rational system of social control in this field. Knight treats of the new biology and the sex-problem in society in five chapters. In part II Peters discusses the institutionalized sex-taboo in four chapters. In part III Blanchard discusses the sex-problem in the light of modern psychology. It is an original, stimulating, and interesting work.

Psychanalysis in the Classroom. By GEORGE H. GREEN. New York, G. P. Putnam's Sons, 1922. pp. 272.

This book, with a brief introductory note by McDougall, was made in England and seems a misnomer, for it is not psychanalysis in the classroom but rather outside the classroom; it deals with dreams, play, instinct, repression, sex, etc. The phenomena dealt with are mainly normal, and the author thinks that it is not only possible but also desirable to apply psychanalysis to all children. It is difficult to see how teachers can make much use of such studies, but parents might do so with profit to themselves and to their children. The book contains many illustrative incidents which are treated in psychoanalytic fashion. It is not entirely obvious why McDougall takes the opportunity in his preface to take a fling at behaviorism. Nor is it obvious why the writer finds it necessary to condemn so much of psychanalysis, especially sexual implications, while he repeatedly in the book seems to recognize all claims of the apostles of the libido as to its dominance.

Studies in Dreams. By MRS. MARY ARNOLD-FORSTER. With an introduction by Morton Prince. London, Allen and Unwin, 1921. pp. 188.

We have here an essentially new theory of dreams by a cultivated woman, herself a remarkable dreamer, who tells us much of the state of the faculties, reason, memory, will, imagination, during the progress of the dream. Her theory (chap. 9) certainly deserves very careful attention. She is well-versed in the literature on dreams, including the Freudian theory, which she accepts only to a small degree. It is probable that no one theory of dreams could explain them all, for dream life is far more varied than normal waking life. This author delights in her dream life and finds as enchanting recreation in it after the cares of the day as a novel-reader. In some, we find symbolisms; and in others, none. Many dreams are repeated; others, not. Especially interesting is her claim that dream life may be cultivated, and this can be done because it is not all unconscious. Although normal dreams are of value for the light they throw on abnormal psychology, it is still truer that the varied data of the latter and the larger field are subject to experimentation and give an understanding of the former, reversing thus the Freudian psychology. The author has not only forbidden unpleasant dreams but has taught herself to fly. She is her own self-analyst, another contradiction of Freud.

Psychoanalysis and Behavior. By ANDRÉ TRIDON. New York, Alfred Knopf, 1921. pp. 354.

The author first discusses the organism, *i.e.*, body and mind, and then takes up in succession the problems of childhood, progress and regression, sleep and dreams, problems of sex, psychoanalytic treatment, and finally, and most interestingly of all, the four schools of psychoanalysis, those of (1) Freud the pioneer, (2) Jung and the Zürich school, (3) Adler and individual psychology, (4) Kempf and dynamic mechanism.

The Kingdom of Evils. By E. E. SOUTHARD, and MARY C. JARRETT. With an introduction by Dr. R. C. Cabot, and a Note upon Legal Entanglement by Roscoe Pound. New York, Macmillan, 1922. pp. 708.

This is a volume dealing largely with cases that came before the Boston Psychopathic Hospital as they were reported by Southard, before his death, and by his associates. It is a storehouse of interesting information, with a breezy and characteristic introduction by R. C. Cabot. The contents is grouped in four books: (1) the three spheres of social work; (2) the five major points of evil in the *regnum malorum*; (3) eleven minor groups of mental disease; (4) epicrisis. The large page and the small print of much of the volume enable the authors in this seven-hundred-page work to pack away a vast body of interesting material.

Suggestion and Autosuggestion. By CHAS. BAUDOUIN. London, Allen & Unwin, Ltd., 1922. pp. 228.

Part I deals with spontaneous suggestion in ten chapters, part II with reflective suggestion in eight chapters, and part III with induced suggestion in seven chapters. This volume is an exposition of the results of the studies of suggestion by the School of Nancy founded by Liébault and Bernheim and now since greatly developed by Coué. Whereas a voluntary act is one of which consciousness is aware, the mechanism of a suggested act remains subconscious. There are three types of mental activity—instinct, habit, and will. Suggestion cannot be reduced to any of these but makes a fourth, quite as important as any of them. With use, it reacquires a prestige which, according to Delboeuf, was originally possessed in an earlier stage of evolution. In those days a living being was fully aware of all that went on within. Owing to the division of labor his attention was increasingly directed outwards, and the supervision of the inner world was left to the subconscious.

Suggestion reestablishes the supervision, reconquers a lost realm, but is no detriment to subsequent gains. The teleology of the will is conscious; that of suggestion, unconscious. In the affective sphere, Freud finds affective complexes of sentiments which are unconscious which we control by making conscious. In the representative sphere, Bergson founds everything upon the distinction between intelligence and intuition. The latter slumbers, but has much knowledge of the greatest import. By the study of intuition we solve problems which intellect cannot. The Nancy School deploys in the native sphere rather than toward theory. Autosuggestion is to the will what the complex is to the sentiment, what intuition is to intelligence. Psychoanalysis, intuitionism, and the new School of Nancy contribute to the same general movement. We now live on the surface of things and do not arrive at intuition. The will is the normal mode of acting on matter without. Suggestion is the normal mode of acting on ourselves. We are masters of nature externally only. Inwardly we are nature's slaves. We have become too objective in our habits of thought. We have forgotten our own personality, and now or never we must know ourselves.

BOOK REVIEWS

The Psychology of the Special Senses and their Functional Disorders. By ARTHUR F. HURST. London, Henry Frowde, Hodder and Stoughton, 1920. pp. x., 123.

This book presents the Croonian Lectures delivered before the Royal College of Physicians in June 1920. Apparently the author has gathered together the most significant of his clinical cases, largely from his war experience. The material is frankly clinical rather than experimental. In the opening chapters he reviews the standpoint of Gendrin and Charcot on the physical and mental stigmata of hysteria. The physical symptoms, we recall, are chiefly cutaneous and pharyngeal anaesthesia and retraction of the field of vision. Babinski had already thrown doubt on the alleged importance of cutaneous insensibility and the retraction of the visual field as stable symptoms. Hurst has confirmed this doubt and has added valuable material on the anaesthesia of the mucous membranes, the pharynx and allied sensibilities.

The principal theses of the author are that the sensory processes are dynamic and that hysteria is essentially subjective in nature. The sensory processes are "active" and "require an effort of the will just as much as walking;" that is, to feel, to see, or to hear one has to pay attention, one has to set up a receptive attitude. The symptoms of hysteria are, therefore, really due to "suggestion" and "autosuggestion." These now popular terms describe for the author the production of Charcot's stigmata as well as the method of removing them. "*Hysteria is thus a condition in which the symptoms are present which have resulted from suggestion and are curable by psychotherapy*" (11: author's italics).

The first chapters concern themselves with the presentation of cases and the discussion of cutaneous and pharyngeal anaesthesia. There is a short chapter on "hysterical pain", which one wishes were more complete. The later chapters concern themselves with deafness and blindness in the author's patients.

Numerous cases are given to show that anaesthesia in the manner of Charcot and Janet is not a permanent feature of the patient's condition. The hysteria which Hurst found in his so-called shell-shock individuals was invariably nothing but the persistence of the physical symptoms after the organic lesions had healed. The clumsy diagnosis of the physician set up in the patient the attitude of belief in his permanent insensibility in the sensory fields affected in the original instance. Often the soldier's autosuggestion enhanced this notion. "When a man is temporarily deafened by a loud noise or by some recoverable disease, he finds that he is unable to hear, however much he tries, and consequently after a time he gives up trying. That is to say, he ceases to listen, and when the cause of the deafness at last disappears, he has become so convinced that he cannot hear that he makes no further attempt to listen"(63). The writer shows that the hysterical deafness reaches below the level of the auditory-motor reflexes, a result which confirms the experiments of Sherrington and Forbes on decerebrate cats. "In our cases of completely bilateral deafness the auditory-motor reflex was completely absent, but returned simultaneously with the restoration of hearing as a result of psychotherapy" (66); and further, "we found that the only test upon which almost complete reliance can be placed in the diagnosis of absolute hysterical deafness from absolute organic deafness is the presence of normal vestibular reactions in the former and their loss in the latter" (72). Hurst admits that slight evidence has been brought forward to show that organic deafness may occur without involving the vestibular canals; but he thinks that by and large his tests are adequate, and that such material as is reported by Fraser does not invalidate his position. He produced some remarkable cures, by his psychotherapeutic methods, which rather confirm his theory.

Hysterical blindness in his cases is confined very largely to three forms: amblyopia, blepharospasm and ptosis. Only the first, in the proper sense, pertains to vision directly; the other two are due to muscular troubles in organs accessory to vision. The patients studied for blindness had frequently suffered severe shelling, resulting in many cases in sand and debris being thrown into their eyes. This produced irritation and temporary loss of sight. The symptoms persisted after the original cause had disappeared.

Following his clinical cases, at this point Hurst introduces the interesting historical cure of La Dame Stapart in the 18th century by a crude method of autosuggestion at the shrine of a former miracle worker.

The final section of the book gives some useful data on the unreliability of the stigma of retraction of vision in hysteria. Babinski in 1909 presented evidence to disprove the notion of Janet that retraction of the visual field is "the emblem of hysterical sensibility in general." As with cutaneous and pharyngeal anaesthesia, and with pain and hysterical deafness, the principal difficulty in vision lies in the awkward diagnoses of physicians.

After reading these data of Hurst's, one questions, however, whether all the cases he dealt with are true hystericals. Is so-called shell-shock, from which his patients were suffering, to be compared with the psychoneurosis known in ordinary psychiatry as hysteria? The author assumes, further, that by attention and suggestion the patient may reinstate the blocked synaptic connections and thus reëstablish the sensory pathways. Increased resistance at the synapses, then, becomes for him the cause for the inability to use the special senses, formerly cut off by organic lesion. He nowhere discusses the likelihood of the use of vicarious or compensatory reflex arcs. Bury and Dana, who are convinced that such paralyses as Hurst describes are not functional at all but physical, that is, biochemical, call attention to "relapses" in similar cases. At any rate, the controversy over the real basis of the symptoms goes on. We must await further results, in which the experimentalist should co-operate with the clinician. We need better control, and more careful description of the curative methods described in the broad terms: suggestion, autosuggestion, will and attention.

Clark University KIMBALL YOUNG

A Study of American Intelligence. By CARL C. BRIGHAM. With foreword by Robert M. Yerkes. Princeton, Princeton University Press, 1923. pp. xxv, 210.

The subject-matter of this book reminds one of the recent theories and contentions of Stoddard. Its main purpose is an attempted explanation of the fact that the best scores in army tests were made by the immigrants who had been here longest. Brigham's thesis is that "the average intelligence of our own immigrants is declining" (197). This condition he considers due to two factors: (*a*) the immigration to the United States of "the Alpine and Mediterranean races has increased," while that of the Nordic races has materially decreased; and (*b*) the "representatives of the Alpine and Mediterranean races in our own immigration are undoubtedly inferior to the representatives of the Nordic race" (197). In order to expound and prove the above thesis, Brigham divides his text into two parts.

In Part I. he first presents a graphic analysis and justification of the army tests. While there is nothing new in this section of the book, it has value in that it is a brief, comprehensive presentation and justification of the army tests and combination scale,—which, or course, have been considered before in more elaborate and detailed form in *Memoir XV* and the *Army Mental Tests*. Brigham arrives at the same conclusion that the authors of these publications arrived at. He points out that "after weighing all the evidence, it would seem that we are justified in ignoring most of the arm-chair criticisms of this [alpha] test and in accepting the experimental evidence tending to show that the test was a fairly good one" (31). And again: "the reader . . . will be convinced that most of the tests [eight alpha, the Stanford Binet scale, and tests 4, 5, 6 and 7, of the beta] used were satisfactory" (59). Incidentally, he points out some matters of general interest: for instance, that the statement one frequently hears "that the tests proved that the average citizen of this country has a mental age equivalent to that of a child of thirteen . . . [is] ridiculous . . . The term 'mental age' is bad scientific slang for a total score" (56). He also notes that the prevalence of the notion that the army tests were "speed" tests, handicapping the ones who were "slow but sure," is not at all warranted; when the tests were given in "double time" the "absolute score" was higher, "but the relative position of the members of the group would be about the same" (11). It is a pity that in this connection Brigham failed to point out also that in "double time" a larger number of members of the group would score a place in class "A."

Secondly, Brigham deduces from his study of the army tests that they are tests which measure *innate* intelligence. He argues, as others have done before him, not only that the tests in general give an excellent criterion which can be used and has been used with success in the selection of army officers (15) but also that they are tests of *native* intelligence (100). As to the usefulness of the tests we must agree, and pay our humble compliment to the work of the army psychologists; but for the definite conclusion that the tests measure *innate* intelligence there are not yet enough data. From the so-called crucial and objective tests we may infer only that the tests are tests of intelligence, not necessarily that they are tests of *native* intelligence. It is to be expected that men in more advanced economic positions should score higher than men who are economically subordinate; but to say that that is necessarily because of a difference in *native* intelligence is surely unwarranted.

Part II. of Brigham's book concerns itself in the main with the proof of the principal thesis concerning immigration outlined at the beginning of this review. In the first two sections, Brigham studies the army score of a representative group of white and negro army draft, noting, of course, that the negro is found to be inferior in intelligence. Beginning with the third section, and continuing to the end of the book, he analyzes the army test score of the white draft. He finds that the native white draft ranks higher than the foreign draft; that is, "of the native born, 74% exceed the average of the foreign born" (85). He finds further that the score of the foreign born draft represents a uniform upward tendency in ratio to the years of residence in the United States (89). That fact, of course, has been well known, and many explanations have been offered. Brigham attempts to ground the usual explanations. In considering the "important problem . . . of interpreting the fact of increase of intelligence test scores with increasing years of residence," he asks: "Does our curve [the score of the foreign born white drafts in ratio to residence] . . . [1] represent the growth of intelligence with length of residence, [2] does it represent an error in the method of measuring intelligence, or . . . [3] does it show the gradually decreasing intelligence of the more recent immigrants examined in the army" (100)?

The first possibility is disposed of by Brigham with the statement that the score is a measure of *native* intelligence (100). This conclusion, as we have pointed out, is uncertain.

The second possibility is disposed of at considerable length. If there is an error in the methods of measuring intelligence, it is likely to be found in the alpha test, because of the implied use of the English language; but that is not the case, Brigham thinks, because "if the increase in the average score on the combined scale from 11.41 to 13.82 were due to the language and educational factor [Brigham usually means formal education], then the gain should come from alpha and not from beta, for alpha involves language and (indirectly) education, and beta does not. We actually find that the gain from each type of examination is about the same. This indicates, then", that the real explanation lies in differences in *native* intelligence (102). It is unfortunate that Brigham did not consider seriously the possibility that the language difficulty for the non-English speaking people in the alpha tests was perhaps paralleled by a difficulty of adjustment to the methods of the beta tests; and that this might account for the uniform rise of the score in both types of tests. On this point he writes, in another connection: "If the tests used included some mysterious type of situation that was 'typically American', we are indeed fortunate, for this is America, and the purpose of our inquiry is that of obtaining a measure of the character of our immigration. Inability to respond to a 'typically American' situation is obviously an undesirable trait" (96). We may fairly ask, at this point, whether this "typically American" trait is also an *inborn* trait?

The author therefore thinks that he is forced to the last possibility, that is, that "the curve indicates a gradual deterioration in the class of immigrants examined in the army, who came to this country in each succeeding five year period since 1902" (111). With this proposition in mind, he tries to find and to explain the reason underlying this deterioration. His explanation is that the immigration has radically changed since 1886 from a predominance of the Nordic race to that of the Alpine and, especially, Mediterranean races (112). He finds further, in an analysis of the mental score obtained by the various nationalities making up the foreign draft, that the Nordic races lead, the Alpine follow, and the Mediterraneans come last. He finds the following relative standing of the various nativity groups to prevail: England, Holland, Scotland, Germany, Denmark, (United States,) Canada, Sweden, Norway, Belgium, Ireland, Austria, Turkey, Greece, Russia, Italy, Poland. From these facts he derives the final conclusion that the representatives of the Nordic races which immigrate to the United States, in decreasing numbers every year, are really of higher *native* intelligence than the representative immigrants of the other two races of Europe. From this it follows that the "future blended American [made up of four racial elements,—the Nordic, Alpine, Mediterranean, and the Negro] will be less intelligent than the present native American . . . The deterioration of American intelligence is not inevitable, however, if public action can be aroused to prevent it" (205, 210).

To the statement that immigration is declining in intelligence no one can take exception. But whether, as Brigham argues, this is due to lack of *native* intelligence in the new immigrant is a much more doubtful matter. It is true that in previous years the United States has attracted from Europe, in part at least, a higher class of people,—people who came across the Atlantic to find freedom, salvation, and adventure. It is perhaps true that the Nordic peoples have sent, and are still sending, a greater proportion of their middle class than the Mediterraneans have. But to say that the middle class possesses a higher *native* intelligence than the peasantry of Russia and Italy is quite a different thing. Is it not as reasonable to suppose, instead, that the immigrants of past years were more easily assimilated and trained, and that for two reasons: first, because there were fewer of them who came and there were greater opportunities offered them when they had arrived; and secondly, because the training and environment of most of the immigrants (Nordic) who came to us in those days were more like our own? Another reason why the Nordic immigrant as a whole scored relatively higher than the Mediterranean, irrespective of years of residence, may lie in the fact that the Nordic people in our country are better organized and established than the Mediterraneans, and thus have had more chance for development.

Whether or not, however, Brigham's warning of approaching degeneration is quite warranted, he has written a book which few psychologists will fail to find useful.

Northwestern University A. J. SNOW

Spiritism and Commonsense. By C. M. DE HEREDIA, S. J. New York, P. J. Kenedy & Sons, Publishers to the Holy Apostolic See. 1922. pp. xv, 220, and 7 half-tone cuts.

Spiritism and the Cult of the Dead in Antiquity. By LEWIS BAYLES PATON. New York, The Macmillan Co., 1921. pp. ix, 325.

The first of these works is a popular treatise written by a Jesuit father for Catholic laymen. Its material is briefly and interestingly set forth and its author writes with a wide knowledge of the current literature of the subject—*vide* the fourteen page bibliography of works in English, French, Spanish and Italian with which the book closes—and, except upon matters with reference to which the Church has already spoken definitely, his openmindedness and scientific poise are admirable.

Accepting a minute residue of the alleged "psychic" phenomena as possibly well authenticated, he shows, nevertheless, in chapters on the origin of spiritism and on the psychology of the observer, of the medium and of the séance, the nidus of fraud in which modern "spiritualism" arose and the atmosphere of deception and credulity in which it still flourishes. As to the possibilities of deception in such matters Father de Heredia speaks as an expert, for he himself is an accomplished magician (as a boy he took lessons from Herrmann) and is able at desire to produce under test conditions most of the striking phenomena of the professional mediums: the materializations, the spirit photographs, the "ectoplasm", the levitations, and the rest.

In later chapters he points out clearly the logical gulf between the acceptance of "psychic" phenomena as actualities and the proof that they are the work of discarnate spirits, and considers in succession the diabolic theory, the natural theory (telepathy), and the spiritistic theory, accepting the first as admissible in rare instances and the second as probable, and rejecting the third. Two further chapters, on Spiritism as a Religion and on Spiritism and Morals, together with an Epilogue, complete the main part of the book. Five appendices deal with special topics.

The second work is a timely contribution to the anthropology of the belief in life after death by the Professor of Old Testament Exegesis and Criticism in the Hartford Theological Seminary. The author treats in thirteen chapters of the status of this belief in primitive religion, in ancient China, among the Indo-Europeans, in Egypt, and among the early Semites, Babylonians and Assyrians, and finally of the changes that it underwent among the Hebrews from their entrance into Canaan to New Testament times through the gradual triumph of Jahvistic conceptions,—on which changes Dr. Paton speaks, of course, with especial authority. A final chapter is devoted to an exposition of the teaching of Jesus as to personal immortality and to a defense of the historicity of his resurrection: which last section, in the reviewer's judgment, is not germane to the rest of the study and might have been omitted with advantage. The book is excellently indexed.

Though Dr. Paton reveals in his preface a certain hospitality toward the results of "psychic research", going in this respect at least as far as Father de Heredia and much further than most psychologists, the scholarly and readable chapters of the body of the work, with their fully documented detail, have evidently not been affected by this extraneous opinion. These chapters demonstrate with cumulative force that the chief phenomena of spiritism have not only appeared at all times and among all peoples, but have given rise also to pretty much the same sort of response on the part of those who were credulous with regard to them. They furnish to psychology a problem of the highest interest, toward the solution of which not only anthropology and "psychic research" but also psychiatry and social psychology have in all probability significant contributions to make.

E. C. S.

A Nursery School Experiment. By HARRIET M. JOHNSON and CARMEN S. REUBEN. New York, Bureau of Education Experiments, 1922. pp. 81.

The interest in this bulletin lies in the problems it sets rather than in the results. In the introduction by Lucy Sprague Mitchell we have two answers to the question: "Why do we want such young children?" The first answer is that the experimenters wish to study the educational value of environmental factors; the second, that they wish to secure data "concerning children's growth,—growth of every sort that is measurable or observable." This bulletin returns a partial answer to the first of these questions in particular, and incidentally gives some facts relative to the second.

Cornell University L. B. HOISINGTON

Psychologie des Gefühlslebens. By GUSTAV STÖRRING. Zweite Auflage. Verlag von Friedrich Cohen in Bonn, 1922. pp. vii, 289.

In a review of Störring's "Psychopathologie," published in the "Zeitschrift für Philosophie" (Bd. 120), Meumann wrote: "In der That würde ich lebhaft wünschen dass der Verfasser in einer neuen Auflage des Werkes manche Anomalien der Gefühle......beträchtlich ausführlicher behandelte." As the dedication of the "Gefühlsleben" runs "Meinem Freunde Ernst Meumann in treuem Gedenken," the work is surely a pious response to Meumann's wish and, perhaps, to the extent to which Meumann busied himself with the psychology of feeling, a memorial.

In this work Störring brings together and systematizes previous investigations and discussions of feelings, emotions, and organic sensations, —taking them up first in their general aspect, in their relation to their bodily accompaniments, and last in their special aspect, as aesthetic, moral and religious feelings. Between these comes a section on the play of states of feeling in various "psychical functions" such as attention, will, and the flow of ideas.

It is impossible, within the limits of this notice, to do more than glance at the rich content of the book. Perhaps a detailed examination is the less called for as there is no essential departure from the conclusions set forth in the various investigations from which the book is derived. In fact, one meets many familiar paragraphs, especially from the "Psychopathologie."

Störring is a good Wundtian,—which means that, while accepting Wundt's fundamental principles of psychological investigation, he by no means accepts all of Wundt's theories and findings.

He rejects the tridimensional theory of the feelings; feelings of tension are for him vaguely localized sensations of tension. No more is excitation a feeling; supposing that it accompanies a rise in the excitability of the cortex as a result of continuous work, Störring regards it as obscure consciousness of a facilitation in psychical processes. In this respect it would seem somewhat akin to the 'Anregung' in the Kräpelin Heidelberg investigations. It is assuredly a different thing from the 'Erregung' mentioned by Störring in reference to Kelcher's experiments, which is more of the nature of irritation.

Pathological findings have brought Störring to consider fixation through an act of will as the essential mark of attention, the driving force of the fixation coming from a feeling element of will-action. In this connection *concentration* is spoken of as a form of attention which, as applied to a feeling, increases its intensity. But attention, as usually directed to a feeling, decreases its intensity. A closer analysis of this state of concentration is much to be desired. Zoneff and Meumann also mention it and also without analysis (Phil. Stud., 18).

No more than Wundt will Störring hear of states of 'doppeltes Bewusstsein' and of 'subconscious selves'; for explanation of the states bearing these names he calls into action the play of organic sensations. Naturally he is antipathetic towards the Freudian structure: the action of the suppressed idea is a matter of fact, but unconscious reflection and unconscious volition are matters of fancy. In fact Störring is that *rara avis*, a psychiatrist who believes in basing explanation of abnormal mental phenomena on established principles of psychology and in explaining the unknown by the known and not by the mysterious.

But besides being psychiatrist Störring is a philosopher and doctor of medicine and psychologist of the Leipzig laboratory. In the exercise of his several vocations he has gathered a large amount of 'material' and it is grateful and comforting to find that in evaluating this material he abides in the straight and narrow path of scientific psychology in a domain where wandering in the by-ways of extemporary psychologizing seems to be a seductive diversion.

In bringing together and arranging in logical form the outcome of his investigations in the psychology of feeling, Störring has rendered a valuable service to the science.

FRANK ANGELL

Stanford University

NOTES

VISUAL INTENSITY

I have been accustomed to treat the question of visual intensity by way of the colour pyramid. I do not think that the following account contains any new idea. Since, however, it leads to results which seem to me to be at least not inconsistent with those which Stumpf arrives at in his recent monograph[1], I judge that a brief Note may be worthy of publication.

Let us begin by removing grey from the colour pyramid. We then have a geometrical figure of the same sort as the smell prism. We have a hollow shell, every point of whose surface is occupied by some visual quality, at its representative best.

Every such quality is, at the same time, a point upon an intensive line. Let us suppose that these lines are added to the figure. We then have, as it were, a hollow pincushion stuck all over with pins, whose length represents the length of the intensive series belonging to the various qualities. The pins are not necessarily driven in to the same distance, since the best representative quality need not correspond throughout with a particular degree of intensity. Nor, in theory, need the pins tend all alike toward the center of the pincushion; the pin that contains an apical quality might, *e.g.*, lie (aside from the intensity of this one quality) altogether outside the figure.

So far, the colour pyramid may be paralleled by a geometrical construction of the qualitative-intensive data of any other department of sense. So far, visual intensity is psychologically given, just as intensities of touch or taste or smell are psychologically given. Only, in fact, we have the complication of grey,—that anomalous quality which has no qualitative pair, no degrees of intensity[2], no part of its own in adaptation or after-image or contrast.

Grey goes, then, to the center of our hollow shell. Immediately, all the pins are, so to speak, psychologically polarised. All now have their points converged to the center of the figure, and all have their heads flush with its boundary planes. For the introduction of grey means that all qualities at all intensities are (psychophysically!) mixed with grey; and this mixture means that all the intensive visual lines tail off toward grey as a common meeting point, and that the quality of maximal intensity becomes the best representative quality. Psychologically, of course, we have lost both our original intensities and our original qualities. The originally intensive lines have been changed to qualitative; and the original qualities themselves have all been clouded or diluted.

I suggest, now, that the transformed and polarised intensive lines may have a twofold psychological status. So long as they are included in the original color pyramid, they are qualitative, and can be nothing else. But let us change our attitude, and—to make the change apparent—let us reconstruct the pyramid in terms of these lines alone. We get a star or sunburst, whose nucleus is grey, and whose rays extend from the nucleus to all points on the surface of the former pyramid. I suggest that the lines of this new figure may be called pro-intensive series. They are not pure intensive series; but they represent, none the less, our nearest possible approach to visual intensities.

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[1] C. Stumpf, *Die Attribute der Gesichtsempfindungen*, 1917.

[2] G. E. Müller, Zur Psychophysik der Gesichtsempfindungen, *Zts. f. Psych. u. Physiol. d. Sinnesorg.*, x., 1896, 31; xiv., 1897, 60 ff.

It remains a question whether the pro-intensive series can be handled, in direct psychological observation and without recourse to perceptive analogy, as intensive. At any rate, it seems plausible to suppose that grey stands as the minimum of visual intensity at large (though I confess I am not quite sure how far psychophysical considerations are involved in this plausibility); that white, since the grey-white line is the longest of all the lines in the figure, stands as the maximum of visual intensity at large; and that the colours familiar in daylight vision possess under ordinary circumstances an approximately constant degree of intensity within the middle range. These are Stumpf's conclusions. It may also be, as Stumpf seems to think, that at any rate certain pro-intensive lines, over a limited portion of their extent, lend themselves to rigorously intensive observation; but on this point further experiments are wanted.

E. B. T.

WUNDT'S ADDRESS AT SPEYER, 1861

There is no more remarkable incident in the history of experimental psychology than Wundt's attempt, by way of a single crucial experiment, to overturn the whole Herbartian psychology (cf. *Beiträge z. Theorie d. Sinneswahrnehmung*, 1862, xxi., note). One of my colleagues will presently publish a full account of the episode. Meantime I can dispose of a document in the case.

In 1861 Wundt gave an address before the *Naturforscherversammlung* in Speyer "über die persönliche Differenz zwischen Gesichts- und Gehörsbeobachtungen mit Rücksicht auf die astronomischen Durchgangsbeobachtungen" (recorded in our bibliography as 1861 (6); this JOURNAL, xxi., 1910, 603). Through the courtesy of Mr. P. Anders, of the firm Gustav Fock of Leipzig, I have received a transcript of this item. The report in the meeting's *Tageblatt* is a bare summary. Since its publication may save trouble to other enquirers, I reprint it here.

"Herr Dr. *Wundt* aus Heidelberg sprach dann über [die] persönliche Differenz, welche zwischen Gesichts- und Gehörsbeobachtung eintritt. Diese Grösse hält er für absolut, aber positiv resp. negativ, weil die verschiedenen Menschen zuerst sehen und dann hören oder umgekehrt, daher erklärt sich der Redner den Unterschied zwischen *Bessels* Beobachtungen und denen anderer, welche, nicht wie *Bessel*, zuerst gehört und dann gesehen haben.

"Herr Professor *Argelander* erklärte, dass er erst sehe und dann höre. Bei der Beobachtung vor dem Faden sei aber die Aufmerksamkeit stärker als die nach dem Faden, daher man bei letzterer den Stern häufig zwei Mal zu sehen glaubt, woher 0, 1 - 0, 15 Sek. Differenz entstehen. Dann beurtheilte er die *Bessel'* sche Beobachtungsweise, welche sich gerade in dieser Beziehung als fehlerhaft herausgestellt hat, bei der Vergleichung des Längenuntershieds zwischen Königsberg und Berlin, welcher später vermittelst des Telegraphen bestimmt wurde. Er erwähnte dann einer der Zeit proportional vor sich gegangenen Vergrösserung der Differenz zwischen ihm und Struve. Zwischen diese Zeit fällt eine schwere Krankheit von Struve, welche keinen Einfluss gehabt hat, und fordert jüngere Astronomen auf, ähnliche Vergleichungen anzustellen."

E. B. T.

URBAN'S TABLES ONCE MORE

H. H. Long's correction of Urban's Tables (the value of $2p$ for $p=.73$ should be 0.7551 instead of 0.7541), which was reported in this JOURNAL (33, 1922, 303) by E. G. Boring, not only necessitates the changes in Rich's Checking Tables (this JOURNAL, 29, 1918, 120) in the values when $p=.73$ and $x=+2$ and $x=-2$, which were pointed out by Boring, but also changes in the values when $p=.27$ and $x=+2$ and $x=-2$.

The changes in Rich's Tables which have to be made because of this latest discovered error in Urban's Tables are: the value for $p=.73$ and $x=+2$ should be 7.2317 instead of 7.2307; for $p=.73$ and $x=-2$ should be 2.2363[1] instead of 2.2373; for $p=.27$ and $x=+2$ should be 2.9665 instead of 2.9675; and for $p=.27$ and $x=-2$ should be 2.9915 instead of 2.9905.

Cornell University H. F. Davies

The Scandinavian Scientific Review

We have received the first number of a new periodical, entitled "Scandinavian Scientific Review: Contributions to Philosophy, Psychology and the Science of Education by Northern Scientists." The Review, which is written throughout in English, is edited by Dr. M. L. Reymert, formerly a fellow in psychology at the University of Iowa, with the assistance (among others) of Professors A. Aall and A. Grotenfelt, and Dr. R. H. Pedersen. Of the five articles that make up the number we mention A. Aall, "Alfred Lehmann: In Memoriam;" R. H. Pedersen, "Weight of School-Children in the Summer Time;" C Spearman and J. Flügel, "The Present Status of Psychological Science in Great Britain and Ireland." The Review hopes presently to issue a monograph series, and also to extend its scope to include other sciences than those named on its title-page.

Arkiv för Psychologi och Pedagogik

This quarterly journal is a continuation and enlargement of the Swedish "Psyke" and "Svensk arkiv för pedagogik," both of which have ceased publication. It will contain contributions to psychology and education in the Scandinavian languages. The editors are S. Alrutz and B. Hammer (Upsala) and A. Herrlin (Lund). The first double number of vol. I. appeared in April, 1922.

The Japanese Journal of Psychology

This new psychological magazine, whose first number is dated January, 1923, is edited by M. Matsumoto, H. Hayami and K. Masuda, all of Tokio. The contents of the number are: M. Matsumoto, On the Methods of Psychology; K. Masuda, What is Psychology? The Study of the "Vorgesetztes Erleben" in its Subjective and Objective Aspects; K. Imada, The Relation between Thought-processes and Verbal Ideas; and T. Oguma, The Contents of Abnormal Psychology. The titles are promising; but the articles must, alas! remain unread by western psychologists.

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[1] This value was mistakenly reported by Boring. *Cf.* G. J. Rich, this Journal, 33, 1922, 450.

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ON THE ESTIMATION OF THE MIDDLE OF LINES

By H. K. WOLFE

The following study was completed and written a few weeks previous to the death of Dr. H. K. Wolfe, late professor of philosophy and psychology at the University of Nebraska. The contribution to the field of psychology, and especially to the field of judgment, seems so great that permission was secured from the wife of the author to have it published. It is with filial devotion to Dr. Wolfe that I, a former student and assistant, present the work for publication.

C. E. Benson, Acting Dean,
The School of Education, University of Oklahoma

HISTORICAL

Nearly three quarters of a century ago E. H. Weber suggested that the just perceptible difference in the length of two lines is a constant fraction of the length of one of the lines; and that generally this fraction is not greater than one twentieth, and may be as small as one hundredth.[1] Weber's experiments were made apparently with both eyes in free movement and at the most favorable distance. The lines to be compared were not shown together; but first one, then the other, was placed in the same field.

F. Hegelmayer,[2] following a similar method, also obtained results in substantial agreement with the psychophysic law, though his experiments were too few for final decision and too incompletely described for comparative use. Hegelmayer sought to determine the effect of small intervals of time upon our memory-images for length of lines (*Gedächtniss für Linearanschauungen*). He prepared on paper seven lines from 15 to 150 mm. long, as given in Table I. He then made other lines, both longer and shorter than these standards, and proceeded according to the method of right and wrong cases in its primitive form. His experiments were very few and apparently very unsystematic. The results given in Table I (H) are a summary of all his experiments. The variable time-element between seeing the standard and making the judgment (5 to 60 sec.) has been neglected in this Table. The numbers denote only the percent. of right cases for each line. Remembering that the differences between the lines to be compared were constant fractions of the length of the lines, the approximate agreement with Weber's law is recognized.

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[1] R. Wagner's *Handwörterbuch der Physiologie*, III, ii. 1848, 568.

[2] Vierordt's *Archiv für physikalische Heilkunde*, xi., 1859, 844-853.

Fechner[3] experimented according to the method of average error, by attempting to place the points of a pair of compasses just as far apart as those of another pair of compasses. Both pairs lay on the table side by side, and were covered except at the points.[4] Fechner's problem was therefore the comparison of distances between points rather than the comparison of the lengths of actual lines. Five horizontal distances were used. The results, and approximate distances, may be seen opposite F in Table I. For each length or distance 120 experiments were made with the normal distance on the left, and as many more with the normal distance on the right. On the whole there seems to be no constant difference in Fechner's results obtained in these two positions of the normal distance.[5] These results coincide remarkably well with the requirements of the psychophysic law.

A. W. Volkmann also used the method of average error. However, instead of compass points, he employed vertical white threads against a dark background.[6] Forty-eight experiments with the standard length on the right and an equal number with the standard length on the left were made for each of the lengths 10, 20, 40, 80, 120, 160, 200, 240 mm. Later the whole series, except for the shorter two distances, was repeated. The results of both series may be studied in Table I. (V 1 and V 2). These results agree very well with those obtained by Fechner, indicating both the validity of Weber's law and the indifference of the relative position of the standard length, that is, whether it lies to the right or left of the line with which it is compared. Volkmann found that the proportional increase of the error with that of the normal line apparently does not hold good for very short lines (0.2 to 1.4 mm.). Fechner thought this result might be explained by the relative size of a new constant.[7] Even if an exception, this would merely illustrate the well-known limitations of the law in other sense judgments, viz., that near the threshold and also near the upper limits of sensation the results show greater variations, or the law ceases entirely to express the relation between stimulus and sensation.

In number of experiments and in carefulness of performance these investigations of Fechner and Volkmann are doubtless all that could be desired. The agreement of these two men and Weber regarding the validity of the psychophysic law for linear measurements by the eye was almost enough to deter even the boldest antagonist. In itself the work is not interesting, and much time and patience would be necessary to open the discussion with new data. The remarkable fertility and great extent of the field now called experimental psychology made it still less desirable to work over the ground so well explored by these pioneers. Hence for many years no attempts were made to verify or to disprove their conclusions.

A. Chodin[8] of St. Petersburg finally undertook an elaborate investigation of the validity of Weber's law for eye measurements. He used the method of average error and in part, also, the method of least observable difference. The former method will be understood except where the latter is specifically mentioned. The plan of procedure was as follows: a fine black line was drawn on paper; the standard length was marked off by short cross lines near the middle of the first line; the observer marked off from the cross lines a length equal to the standard line, both on the right and on the left of the standard line or, in case of vertical measurements, above and below the standard, thus giving a standard length between two estimated

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[3] G. T. Fechner, *Elemente der Psychophysik*, i., 1860, 211.
[4] *Ibid.*, 214.
[5] *Ibid.*, 214ff.
[6] Fechner, *Psychophysik*, i., 215ff.
[7] *Ibid.*, 212, 221ff.
[8] Graefe's *Archiv für Ophthalmologie*, xxiii., 1877, 92ff.

TABLE I.

Results of Investigations on Comparing the Lengths of Horizontal Lines:
General Summary

Observer	Follow Weber's Law?	Distance of eyes, mm.	No. of judgments each line	Line mm. 2.5	5	10	20	30	40	50	60	80	90	100	120	150	160	200	250	M	V
H.	Yes		90			59		62			62		80		67	70					
F	Yes	350	240			1.61	1.60	1.68	1.61	1.55										1.61	.03
V I	Yes	800	96			1.31	.97		1.34			1.08			1.16		1.28	.96	1.15	1.16	.12
V II	Yes	800	96						.77			.97			1.04		1.00	.98	.94	.95	.06
C I	No	350	150	2.56	1.92	1.50	1.32		1.45			1.37					1.54			1.67	.33
C II	No	350	120	2.00	1.28	1.11	.89		1.07			1.14					1.41			1.27	.26
M I	Yes	600	20			2.50	1.75	1.80	1.75	1.30	1.50	1.25	1.45	1.20	1.60	1.50	1.40	2.50		1.67	.33
M II	Yes	600	20			3.00	2.00	1.70	1.40	1.40	.90	1.80	2.70	3.00	2.70	2.30	2.40	1.90		2.00	.50
M III	Yes	600	20			3.00	4.50	3.30	3.00	2.80	2.70	2.50	2.30	2.00	2.40	2.30	2.30	2.00		2.53	.42
H I	No	500	500			3.24	3.04			1.73				1.88		2.25		2.55	2.73	2.42	.47
H II	No	500	4800							82.1				80.0		78.2		75.8			
F I	Yes	200	80		.66	.59	.64	.64	.67	.90										.68	.07
F II	Yes	200	80		1.50	1.42	1.09	1.11	1.27	1.40										1.30	.14
H.W.		Free	150				.57			.46				.59			.56		.68	.57	.05
K.W.		Free	100				.44			.45				.46			.52		.57	.49	.05
25		Free	250				.89			.73				.78			.77		1.06	.85	.10
25		Free	250				.75			.79				.79			.92		1.22	.89	.14
98		Free	490				1.22			1.17				1.28			1.28		1.62	1.31	.12
88		Free	440				1.22			1.17				1.28			1.36		1.42	1.29	.08

Table I. is a summary of results from all important investigations on this subject. In spite of the varying circumstances under which the experiments were made there is too great similarity in result to warrant the contradictory conclusions drawn by the individual writers.

The first vertical column contains the names of the observers, and Roman numerals designating their several series. The second column gives the answers of the various investigators to the question: Is Weber's Law valid for eye measurements of horizontal lines? The third vertical column shows at which distance the eyes were fixed. The column headed "No. Judgments" shows how many judgments for each length of line are included in the table.

lengths. The standard lengths used were in mm. as follows: 2.5, 5, 10, 20, 40, 80, 160. The distance from the observer's eye to the line was 350 mm. Both eyes were open and were free to move.

Altogether Chodin made about 7000 experiments. He concludes that Weber's law does not hold for eye measurements. Table I. (C 1) gives the results of his first series for horizontal lines, and Table I. (C 2) gives the results of a similar series made later. Comparison of the two series shows the effect of practice. The first series consisted of 150 judgments for each length of line indicated, 75 when the standard was on the right, and 75 when the standard was on the left side of the line to be estimated. The variable error was computed separately for each 15 judgments with a given length. The average of the separate variable errors was taken. The second series contained 60 judgments for each of the two positions of the standard for each length indicated in the table.

The effects of practice are very noticeable, but must not be distorted to support a theory. Chodin says that practice shows greatest changes in the middle distances employed. It is clear from his own figures that practice makes most difference with the results in case of the shortest lines used by him, and that there is a tolerably gradual decrease as the longer lines are reached.

Experiments on horizontal lines by the method of least observable differences were made by Chodin with very similar results. The first series of 120 judgments for each of the seven lengths of lines resulted as follows: 5.88, 3.45, 2.70, 1.89, 2.27, 2.57, 2.33 (in percent. of the total length of lines). The exact repetition of this series gave 3.85, 3.12, 2.22, 1.75, 2.78, 3.12, 3.33. The effect of practice is again seen to be greater on the short lines. Another series of experiments was made by Chodin on horizontal lines according to the method of average error, but differing from the first in having the standard removed before the estimated line is drawn (successive, instead of simultaneous, comparison). In this manner 120 judgments were made for each of the seven standard lengths. The average variable error had the following value in percent. of the length of the line: 4.35, 3.23, 2.70, 2.33, 2.08, 2.00, 2.86. Chodin concludes very positively that Weber's law is not valid in the estimation of horizontal and vertical lines. The relative difference between the standard and the estimated line does not remain constant, but in horizontal lines it decreases from the standard 2.5 mm. long to the standard 20 mm. long, and in vertical lines from 2.5 mm. to 10 mm. long. In both horizontal and vertical lines the relative sensibility for lengths increases thereafter to the standards 160 mm. long. The reason for this difference he finds in the muscular movements of the eyes. When retinal images alone are compared, as in the smallest standards, the larger the image the greater the discrimination, hence the decrease in the relative difference; but when muscular action accompanies the judgment, as in the longest standards, the reverse is true. Comment on this remarkable interpretation is obviously unnecessary. In agreement with other investigators Chodin finds that discrimination for horizontal distances is finer than for vertical distances.

Before leaving the very interesting data given by Chodin I will call attention to a general summary of his results for which I alone am responsible. It is the average of all his measures of discrimination for horizontal lines, however obtained, and must not be used for any other purpose than that of deciding the bearing of his experiments upon the validity of Weber's law for eye measurements. The distances are the same as those already given. He made 630 experiments for each length of standard line, with the following average results: 3.73, 2.60, 2.05, 1.64, 1.93, 2.04, 2.29. Chodin also made 1260 judgments of vertical lines. These results, strange to say, were even more closely in accord with Weber's law than were his experiments with horizontal lines.

Münsterberg's experiments were most carefully planned and executed.[9] In the number of normal distances employed they exceed all others. Owing to the great number of problems investigated and the consequent variation in the conditions only a few experiments can be directly compared with the work of others. In all his experiments the head was fixed at a distance of 600 mm. from the line. He judged by the method of average error the horizontal distance between two sets of two points each, which lay in the same straight line. The two middle points separating the distance to be compared were 60 mm. apart for M 1 and M 2 in Table I. For each normal distance at least twenty single judgments were made, half with the normal distance on the right, and half with it on the left. The results given in Table I opposite M 1 were obtained with the use of both eyes and perfect freedom of eye movement. These may be compared directly with those marked C 1 and C 2. The only difference in method was that Münsterberg used a movable slide to change the length of the line and Chodin used a pencil to mark off his line. Many more experiments for each length of standard were made by Chodin than by Münsterberg. It will be noticed that the mean of the average variable errors is 1.67 in C 1, and also in M 1. In C 2 practice has much reduced both the average error and the variation from the mean. We need not emphasize this absolute agreement between the empirical data of Chodin and Münsterberg to lose ourselves in wonder at the opposite conclusions reached by these investigators. M 2 in Table I. contains results of a similar series of experiments with the use of the right eye only. The general tendency is strikingly similar to that shown when two eyes are used. The only essential difference is the slight increase of the variable error and a corresponding increase of the mean variation of the separate variable errors. M 3 gives results of a similar series with the use of both eyes, but from memory of the normal distance seen just before each experiment. There is again an increase in the mean of the average variable errors, but the mean variation is less than in the preceding series.

These three series representing the results of less than 2000 experiments are about the only original data that the interested reader can obtain from Münsterberg's long article (56 pages) describing 20,000 judgments. The remaining facts given us are mere summaries of numerous valuable tables. These summaries contain just four numbers each, *viz.*, the mean constant error when the standard is at the right, the same error when the standard is at the left, the mean variable error, and the mean variation of the separate variable errors from the average. The reader must accept on faith, not only Münsterberg's conclusions, but also the facts upon which his conclusions are based, and the validity of his reasoning processes. The present writer is at a loss to understand wherein such "experimental psychology" differs from "rational psychology."

Münsterberg's tables bring out a fact mentioned by others in certain particular cases;[10] that "every change in movement, position, or manner of using the eyes manifests itself in the judgments." These influences are often so complex that their specific determination and especially their explanation may be postponed until much additional material shall become accessible. However, it may be urged justly against Münsterberg, I think, that he soon forgets his modest and cautious introduction, wherein are shown the multiplicity and complexity of finely shaded influences which we are unable at present to analyze. A few lines further on he transfers the results obtained solely upon one observer (himself) to all mankind, and proceeds to offer anything but an empirical explanation of the phenomena. Münsterberg overestimates a distance at his left compared with one at his right. He then concludes that people in general have this propensity, and explains it by our acquired facility in moving the eyes from left to right in reading.[11]

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[9] H. Münsterberg, *Beiträge zur experimentellen Psychologie*, ii., 1889, 125ff.

[10] H. Messer, Poggendorff's *Annalen*, clvii., 1875, 172.

I find that about twice as many persons overestimate the distance at the right as at the left. Remembering that, in reading, every time the eyes are moved to the right they must be moved back again, and generally are moved back more rapidly and with less attention, *i.e.*, more easily, it occurs to me that this explanation will fit my facts quite as well as the contrary facts of Münsterberg. Those who are unable to accept Münsterberg's conclusions from his experiments can but regret that much of the space occupied by comment was not given to the material itself.

Seldom have so much time, care, skill, and ingenuity been given to work of this kind. Complete tables would have afforded a rich mine for other investigators. The present writer, perhaps, does not differ much from Münsterberg's opinion in regard to the importance of the muscles in space perception; but, because Münsterberg's experiments show that he judges distances more accurately with movable eyes than with fixed eyes, one need not believe "dass alle Grössenschätzung nur Intensitätschätzung der Muskelempfindungen ist." [12]

As a general result of some 20,000 experiments Münsterberg concludes that Weber's law finds one of its best witnesses in eye measurements of extension.

Higier[13] had for his object the determination of the relative value of the two psychophysical methods in eye measurements. His discussion is therefore confined chiefly to the bearing of his results on these methods, and only incidentally touches on the practical truth of Weber's law in the field of visual judgments of spatial relations. He used especially the methods of average error and of right and wrong cases. The observers remained in the dark and compared the lengths of slits, 0.5 mm. wide, in a screen through which more or less light came. Fewer problems were investigated and more experiments were made under the same conditions than was the case with any other investigator of this subject.

The observer's eye was 50 cm. from the line, and for the experiments given in Table I. only the right eye was used. The head was fixed, the eye movable and normally directed towards the inner ends of the two lines to be compared. Perhaps the greatest error that was apparently neglected in these experiments arose from the frequent change of recorders, who, of course, had also to read off the error of observation. As far as the results by the method of average error are concerned we feel even less satisfied with the meagre tables of Higier than with those of Münsterberg. In Table I. (H 1) is given about one-third of the data we are entrusted with, for 3500 experiments, 500 of each distance estimated. It would be interesting and perhaps helpful to know, not only the final average variation, but also the separate averages of each ten or twenty experiments.

During each sitting Higier secured 350 single experiments. Such a number would be impossible or nearly worthless with a less interested observer. On the whole his experiments seem to have been very carefully performed. The normal distance was varied from right to left, the comparative distance was alternately taken too large and too small, and the separate results were tabulated to show the influence of these two factors. In comparing distances with the right eye Higier finds a decided tendency to overestimate the part on the left of the median line. Münsterberg found the same tendency to exist when he used both eyes.

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[11] *Ibid.*, 166-168.

[12] *Ibid.*, 179.

[13] H. Higier, Wundt's *Philosophische Studien*, vii., 1892, 232ff.

In agreement with Higier, Kundt[14] found, in attempting to halve a line 100 mm. long, that his right eye tended to overestimate the left half of the line and his left eye to overestimate the right half, his error being about twice as great in the former case as in the latter. Aubert[15] is said to have made a similar observation.

Higier's experiments by the method of right and wrong cases give results in substantial agreement with those just described. He used only four distances, 50, 100, 150, and 200 mm., for each of which 4800 determinations were made, 480 for each of the ten gradations. Five of these gradations were larger and five smaller than the normal distance; the difference being in both cases 1/100, 2/100, 3/100, 4/100, 5/100, of the normal distance. Half of the experiments were made with the normal distance on the right, and half with it on the left. In half the cases it was larger and in half smaller. On the average about seventeen percent. more correct answers are found when the comparative distance is smaller than the normal distance. The difference is greater when the normal distance lies on the left side. The two methods give results in remarkably close agreement, as far as the sensitiveness depends upon the length of the lines compared. Both methods show that for observer Higier, and for the distances 50, 100, 150, 200 mm., the sensitiveness decreases with increase of the distances to be compared. This decrease is so regular for the distances investigated that one is tempted to look for an undiscovered constant, especially since the two methods agree in this respect. Higier concludes and very positively declares that spatial measurements by the eye do not follow Weber's law.

R. Fisher[16] (Augenartzt in Leipzig) undertook to determine the relative values of distances in four directions from a point. That is, he compared the length of the four arms of a cross whose plane was perpendicular to the line of vision. He used the general method of average error and proceeded in two ways: by attempting to halve both the vertical and the horizontal lines forming the cross, and by attempting in turn to make each of the four arms of the cross equal to a given arm. The variable error in the first case is found to be only about one-half that obtained by the second method. The cross stood at a distance of 200 mm. (?) from the eyes. Only the arms of the cross in use for a particular experiment were in sight. The arms were not marked by visible lines, but by empty spaces between metal finger points. Experiments with all four arms of the cross followed each other regularly, and successive experiments were always of unlike kinds. In the results given in Table I., F 1, both eyes were used and free movements were permitted.[17]

The problem in this case was to divide in the middle the distance between two metal points in the same horizontal line. In order to compare

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[14] A. Kundt, Poggendorff's *Annalen*, cxx., 1863, 135.

[15] H. Aubert, *Physiologie der Netzhaut*, 1865, 263ff.

[16] R. Fischer, Graefe's *Archiv für Ophthalmologie*, xxxvii, i., 1891, 97ff.

[17] The distances used by Fischer do not quite correspond to those in Table I. Below are given the exact distances and the corresponding values of the variable error in percent. of length of line. It is to be observed that the average value for distances 15.9 and 23.8 is given in Table I. under distance 20 mm. and that in the second half the averages of distances 8.7 and 11.5 is given under distance 10 mm.

I.	Length of line, mm.	6	8.7	11.5	16	21.9	31.6	45.2
	Percent V. E.	1.50	1.47	1.37	1.09	1.11	1.27	1.40
II.	Length of line, mm.	12	15.9	23.8	34.2	48.2	68.4	96.0
	Percent V. E.	.66	.58	.60	.64	.64	.67	.90

these results with those obtained by making one line equal to a given line it is evident that one-half the distance must be taken as the basis.

In attempting to make one horizontal arm equal to a given arm Fischer found the variable error about twice as large as in halving a line. The relation of variable error to length of line, however, remains about the same. Table I. (F 2) gives the relations for approximate lengths. Experiments were also made by Fischer with the use of the right eye only. His tables with this condition deal more particularly with the constant error. He states that the variable error is similar but absolutely larger than when two eyes are used, the ratio being as 4 to 3.

Fischer is more interested in the constant error and in the practical results of his experiments,—which is for several reasons unfortunate. In the first place, although he has devoted a good deal of time and care to making a large number of experiments, he has used very short and very inconvenient lines; quite sufficient, perhaps, for his own purpose, but rather unsatisfactory to others. Again, the value of his results on the constant error is probably nearly zero, on account of defects in the observer's own vision. This point, however, will be considered in a section specially devoted to the constant error in eye measurements of horizontal and vertical lines. Fischer concludes that the estimation of lengths of lines follows Weber's law.

While it is true that the results of the various investigations on this subject cannot be used in exact comparison with one another, because of differences in the conditions, yet there is enough general agreement in methods to warrant a superficial comparison in respect to the validity of Weber's law for eye measurements. The uniformity of results revealed is clearly dependent upon a principle, and is not the consequence of self-correcting errors in the various methods of investigation. Table I. gives the results of the several investigators already mentioned. The figures are only approximate, since several have been reduced to the same scale, often from incomplete data.

The variety of methods and conditions precludes any exact quantitative comparison. The wonderful agreement in facts, however, cannot but arrest our attention.

The last part of Table I. contains the results of experiments differing still more in methods and conditions, but nevertheless, as far as general conclusions are concerned, having more or less claim to be compared with those of the first half. Fortunately it does not often happen that conclusions so contradictory are drawn from facts so nearly in harmony. Münsterberg thinks his experiments indicate the validity of Weber's law. The results of Higier conform to the requirements of this law even more nearly than do those of Münsterberg, yet Higier concludes that Weber's law does not hold for such judgments. Weber, Fechner, Volkmann, and Fischer declare for the law, Chodin decides against it. Higier attempts to reconcile his conclusion with that of Wundt by showing that Wundt, Fechner and Volkmann do not recognize the validity of the law for small distances. While this is true, it does not in the least affect the results of Higier, whose shortest distance was 10 mm., which scarcely approaches the minuteness of the distances referred to by Fechner and Wundt.

From a study of these investigations I am led to conclude that the divergence of opinion results chiefly from subjective conditions. In eliminating various subjective factors, those that remain stand out all the more clearly. We need an objective method of interpreting experimental data, or else a little more willingness to wait a while for full explanation of carefully ascertained facts.

The recognition of this fact is clearly seen in the greater attention now given to the communication of detailed methods and results in long tables of figures and pages of description of conditions with short paragraphs of interpretation. This is, of course, the severest trial for the young psychologist, especially if he be a "born" psychologist. The lesson is sometimes expensive, but many are learning it. The assumption made by a few enthusiastic observers that they are "normal," and hence that any result obtained on themselves may be unconditionally generalized as a law of human nature, is not always concealed by the usual specific disavowal of such intention. It is not given to many to say the final word on any subject, and he who deals with the complex problems of human nature will wisely expect that the final word of today will receive no consideration tomorrow. The evidence apparently indicates that the psychophysic law holds good in eye measurements of distances which we are ordinarily accustomed so to compare. It does not seem to hold for any sense beyond the limits of usual experience, and it is not improbable that the law itself expresses the sum of our *knowledge about anything*, rather than the relations between parts of that sum. In other words, the psychophysic law may express the mean result of *all* our sources of information, supplemented, diminished, and corrected by many avenues of experience, without necessarily expressing the effect of the separate sources of our knowledge. In short, it may be simply the law of normal experience, rather than the "law of the relations between mind and matter." When this normal experience is interrupted by fixing the head, limiting the distance, closing one eye, and so on, the variations become greater. It is very evident that one would not normally compare lines 250 mm. long at the same distance from the eye as lines 5 mm. long. This view is supported by most of the results already attained, and will account for the tendency in later, more refined and isolated methods to produce results differing more from the law and from the earlier results obtained with simpler and more natural methods. This conclusion receives very strong confirmation from my own experiments on the question of ordinary eye measurements.

EXPERIMENTS AT THE NEBRASKA LABORATORY

The experiments which I shall now describe were not undertaken to confute or to confirm Weber's law, or even with a view to the solution of any disputed psychological question. Since 1890 my beginning classes have been accustomed to work two hours each week in the laboratory. For several years the classes were small and the laboratory work was very carefully supervised. The experiments now under consideration were all performed in my presence, not more than six students working at a time. All measurements and computations were made by myself. The facts which form the basis of this paper were obtained from several hundred adult students who tried to mark the middle of lines.

As "lines" strips of white paper 1 cm. wide were employed. The observer's estimate of the middle was indicated by making a pinhole with a long glass-headed pin while the strip lay on the table before him, for the horizontal measurements. The observer sat squarely at the table fronting the source of light, so that each end of the strips received about the same illumination. The approximate middle of the line was directly opposite the eye of the observer, whose instructions permitted free movement of the body, head and eyes. Our purpose was to make the determinations as nearly normal as possible. The instructions were to mark the apparent middle of the lines. The students did not measure the results during the progress of the investigation. They were requested to keep their attention on what was immediately before them, rather than to speculate on what they had just done. Nevertheless, it was found that many of them learned the nature of their prevailing errors before the work was concluded. In most cases, I think the request to mark the *apparent* middle prevented any conscious allowance for discovered tendencies. In no case was I able to find any change in results which could be attributed to this influence. The only restriction upon the duration of a single judgment was that after the pin approached the paper the determination should be completed within 10 sec. Generally less time was used, but as no means of marking this time was employed it occasionally may have exceeded 10 sec. The paper strips were appropriately marked after each judgment in order that the person, the relative position, and the order of the judgment could be determined by the recorder.

The judgments of vertical lines were made under the same conditions. The paper strip was held by the left hand against an upright board, so that the middle was about on a level with the eyes. The distance of the eyes from the paper was usually about the reading distance of the student. For the longer lines there was undoubtedly a tendency to increase this distance; but of course it could not be increased beyond the length of the arm, and this was, in fact, the only restriction placed upon this factor, both in vertical and in horizontal measurements. No tentative marks were permitted. The pin was held in the hand, the point placed on the paper, and moved to right or left (or up or down) until the apparent middle was found. The order of making the judgments was always the same, viz., beginning with the shortest line, one for each length placed horizontally 50, 100, 200, 350, 500 mm.; then one for each of the same lengths held vertically. I call this a double series. Five double series were completed at one sitting, usually occupying nearly an hour, since each strip had to receive the personal and serial marks of the observer, and haste was especially avoided. Five other double series were made, usually about a week after the first set.

The persons taking part in the experiments now under consideration were nearly all advanced students of the University. The others were teachers taking work in psychology. I sought the most favorable conditions for estimating normally the middle of lines, both in the horizontal and in the vertical position. No attempt was made to determine the separate factors. In this case the number of persons investigated is, of course, of more importance than the number of experiments each person makes.

Only in a few cases was there reason to suspect that another set of experiments would have differed much from the one given. At the same time the variation is so great that less than five judgments for any length would have little or no value.

All measurements of errors were made by myself, and recorded to the nearest half mm. Since the ten results of each person for any one length were measured in immediate succession, it was possible to increase the accuracy of measurements beyond that indicated by the individual judgments recorded. Though using the same means as Münsterberg, and working as carefully as possible, I cannot claim that an accuracy of one-tenth mm. was attained. Practically it was found best to fold the papers and to measure from the crease to the pinhole. The material, consisting of some 80,000 judgments, made by more than 800 persons, naturally divides itself into two parts. I shall present first the results of attempts to mark the middle of horizontal lines; but before doing so a word concerning the value to experimental psychology of the statistical method may not be out of place.

No number of unskilled observers can take the place of the trained psychologist. There are open to investigation, however, many questions concerning which structural peculiarities of the individual prevent him from arriving at normal results. In other cases no structural difference can be found, but the results show a personal idiosyncracy that forbids the thought of considering them as standards. This is especially true in our estimation of space relations, whether we consider the immediate comparison of one line with another or the reproduction of space relations from memory. It is also equally true with the relations of both small and large dimensions. As far as horizontal and vertical lines are concerned, many examples of constant errors may be found in my tables. Examples in every-day life, both in space and time, are known to every careful observer.

The facts set forth in Table I. indicate the danger of generalizing without extending the investigation to many persons. Indeed the entire history of the science of experimental psychology, as well as that of all other sciences, shows that men unwillingly learn to be cautious. Perhaps also even the modern psychologist may attach undue importance to results obtained by self-experimentation. Being a "normal" man he may not remember that *other* people are, for the most part, abnormal, and that science cares more about the rest of the world than it does about him. I believe that the sacrifice of accuracy and control by the use of unpractised observers does not render the results worthless, but on the contrary that these more or less crude results may form an important, if not a necessary, supplement to much of the work of experts. It is therefore with a view of supplementing the patient labors of several admirably equipped special investigators that the following tables are presented.

Experiments with Horizontal Lines

Table II. contains the results of 50 observers, 25 men and 25 women. Every person made ten judgments for each of the following lengths of lines: 50, 100, 200, 350, 500 mm. The observers' names were arranged alphabetically (men first and women last), and numbered as in the first vertical column. The next five vertical columns contain the total average error of ten judgments for each of the five distances, expressed in percent. of the whole length of line. The seventh vertical column contains the arithmetical mean of the five average errors. The eighth column indicates the mean variation of these five, several, average errors from their own mean. The figures in the first horizontal line of the Table indicate the lengths of lines which were to be divided in the middle. For example: the second number in the second horizontal line, 1.20, means that the sum of the errors made by Mr. B. (1 in the first vertical column) in attempting to mark the middle of a line 50 mm. long, divided by the number of judgments and by the length of the line, and this quotient multiplied by 100, is equal to 1.20; (6x100)÷(10x50) =1.20. The next four numbers of this line were obtained in a similar way.

Table II. contains the relation of the variable error to the entire length of the line, also expressed in hundredths. As the method of halving a line differs in several important respects from that of making one line equal to another, it may be well to state the manner of obtaining the variable in the present investigation. All the ten judgments of each set were made under the same conditions. A slight difference in the method of obtaining the variable error needs to be explained to render the results intelligible and useful to those who may not understand how the variable error can be obtained by my method of measurement. Only the amount and direction of deviation from the true middle are recorded. Instead, then, of having an average length of line from which to subtract each separate estimated length, we have an average error with a particular sign (*i.e.*, the constant error) which must be subtracted from, or added to, the errors of the separate judgments. It is subtracted when the particular error is on the same side as the average constant error and is greater than the latter; otherwise it is added to the constant error. This is doubtless more difficult to understand, but it is of considerable practical value in measuring the lines.

TABLE II. (Horizontal Lines)

Relation of Total Average Error to Length of Line, Expressed in Hundredths

Line	50 mm.	100	200	350	500	Mean	Average variation from mean	Relation of average variation to mean
Men								
1	1.20	1.20	1.47	1.91	2.30	1.61	.39	1/4.1
2	1.70	1.15	1.20	.84	1.00	1.18	.22	1/5.4
3	1.60	2.60	2.52	1.10	1.38	1.84	.58	1/3.2
4	.40	.70	.70	.76	1.32	.78	.22	1/3.6
5	.50	.85	1.05	.64	1.09	.83	.20	1/4.1
6	1.00	.85	1.05	1.16	2.15	1.24	.36	1/3.4
7	.60	1.00	1.20	1.86	1.55	1.24	.37	1/3.4
8	1.00	1.05	1.45	1.02	.81	1.10	.18	1/6.1
9	.80	.60	1.20	1.27	1.20	1.01	.25	1/4.0
10	.80	.90	.95	.31	1.71	.93	.32	1/2.9
11	.70	.60	.58	.68	.87	.69	.08	1/8.6
12	.70	.70	.90	.63	1.05	.80	.14	1/5.6
13	.90	.90	1.57	1.06	.96	1.08	.20	1/5.4
14	1.50	2.50	4.20	3.58	3.47	3.05	.84	1/3.6
15	1.80	1.95	1.48	1.73	1.50	1.69	.16	1/10.5
16	2.00	2.50	2.05	1.50	1.89	1.99	.23	1/8.6
17	.80	1.10	.63	.64	1.40	.95	.28	1/3.4
18	1.70	.95	.98	.87	.66	1.03	.27	1/4.0
19	.50	.65	.53	.48	1.37	.71	.27	1/2.6
20	1.80	1.65	1.97	1.96	1.50	1.78	.16	1/11.1
21	1.40	1.15	1.15	1.26	1.42	1.28	.11	1/11.7
22	.70	.55	.97	1.73	.95	.98	.30	1/3.3
23	1.20	1.05	1.10	1.07	1.20	1.12	.06	1/18.7
24	2.00	.90	1.05	1.51	1.77	1.45	.38	1/3.9
25	1.10	.75	1.67	2.24	2.20	1.59	.53	1/3.0
Av.	1.14	1.12	1.34	1.28	1.47	1.28	.11	1/11.6
Women								
1	1.30	1.75	1.95	1.20	1.21	1.48	.29	1/5.1
2	.60	.30	.65	.64	.99	.64	.14	1/4.6
3	1.30	.80	.90	.61	1.23	.97	.23	1/4.2
4	2.30	1.45	1.03	1.24	2.05	1.61	.45	1/3.6
5	.40	1.30	.70	.74	1.13	.85	.29	1/2.9
6	1.60	1.65	1.20	.98	1.62	1.41	.26	1/5.4
7	.90	.85	1.90	.53	1.63	1.16	.48	1/2.4
8	2.20	2.35	.75	.78	.75	1.37	.73	1/1.9
9	.50	1.25	.93	1.37	2.41	1.29	.48	1/2.7
10	.90	.95	.88	1.18	2.51	1.29	.49	1/2.6
11	.70	.50	1.17	1.27	1.25	.98	.30	1/3.3
12	1.00	1.55	.63	.97	1.73	1.18	.39	1/3.0
13	.70	1.05	1.13	.89	1.10	.97	.14	1/6.9
14	.80	.75	.68	1.14	2.01	1.08	.40	1/2.7
15	1.10	.80	1.08	1.18	.97	1.03	.11	1/9.3
16	1.20	1.00	1.15	1.27	1.29	1.18	.09	1/13.7
17	.90	.90	1.62	2.43	2.39	1.65	.61	1/2.7
18	2.40	1.30	.78	1.06	1.57	1.42	.45	1/3.2
19	.50	.80	1.20	.77	2.61	1.18	.58	1/2.0
20	1.30	1.15	1.87	1.02	1.98	1.46	.37	1/4.0
21	.60	.85	1.40	1.46	2.27	1.32	.47	1/2.8
22	1.30	.85	1.03	1.70	1.03	1.18	.25	1/4.7
23	.70	.60	.92	2.14	2.16	1.30	.68	1/2.0
24	2.00	1.40	2.20	2.00	1.82	1.88	.22	1/8.6
25	.30	.50	.23	.57	.73	.47	.16	1/2.9
Av.	1.10	1.07	1.12	1.17	1.62	1.22	.36	1/7.6

If one admits the possibility of using the device of halving in the method of average error, there will be little objection to this plan of dealing with the results.

Reserving the discussion of the bearing of these results upon the validity of Weber's law till all the evidence at hand has been presented, it will be worth while to examine some details of the Table. While doing so, however, it must be remembered that with two or three exceptions these results are from untrained observers and that only ten observations were taken for each length of line. *No results from these observers are omitted, and no other observers finished the required work.* There was, of course, no selection of results, but all who attended the class did this work, and both here and in the other tables of this article all acquired data are given as first obtained. In several instances students have repeated the experiments because they did so much worse than their fellows. This was permitted for their own satisfaction, and usually resulted in considerable improvement. Only in one case, however, was this new result substituted for the old. This case does not appear in Table II. or III. The substitution was made in Table IV., because the student was seriously ill at the time the test was made. The repetition showed less than one-fourth as great errors. With the exception of number 14, there is great uniformity among the men in the amount of the total error. The lowest mean for an individual is .69%, and the highest mean is 1.99%. The lowest average for any length of line is .40%, and the highest is 2.60%. The average of all mean judgments is 1.28%, and the average variation from the average is .11% (including number 14). The very large total error of number 14 is due to his great constant error, as is proved by the very moderate size of the variable error. This was the most peculiar case of the entire number of 50 students. Some attempt at a further analysis of this error will be described later. The average variation of the different observers from their own mean is less than we might have expected, it being for these 25 men only .28% of the total length of the line.

Table II. also presents the results obtained by 25 young women under precisely the same circumstances. Here the uniformity is even greater than for the men. The lowest mean for a woman is .47%, the highest 1.88%. The lowest average for a single line is .23%, and the highest 2.61%. The average of all mean judgments is 1.22%. The average variation of the means from this average is .23%. The average variation of the women from their individual means is greater than that of the men, being .36%. As far as any conclusion can be drawn from this fact, it shows that these young women are, as individuals, more variable than the men, but that they differ from each other less than do the men. These figures, however, do not differ much

TABLE III. (Horizontal Lines)
Relation of the Variable Error to the Length of Line, Expressed in Hundredths

Line, mm.	50	100	200	350	500	Mean	Average variation from mean	Relation of average variation to mean
Men								
1	1.20	.90	.94	1.03	1.50	1.11	.19	1/5.8
2	1.10	.65	.93	.84	.90	.88	.11	1/8.0
3	1.44	.94	1.17	.84	.67	1.01	.23	1/4.4
4	.48	.70	.55	.75	1.40	.78	.25	1/3.1
5	.54	.85	.97	.50	1.01	.77	.20	1/3.9
6	1.08	.82	.71	.47	1.33	.88	.26	1/3.4
7	.68	1.00	.90	.96	1.41	.99	.17	1/5.8
8	.80	.46	.48	.83	.73	.66	.15	1/4.4
9	.96	.60	.57	.63	.60	.67	.11	1/6.1
10	.80	.82	.44	.36	1.82	.85	.39	1/2.3
11	.80	.44	.48	.65	.89	.65	.15	1/4.3
12	.80	.50	.90	.61	1.03	.77	.17	1/4.5
13	.76	.40	.81	.63	.91	.70	.16	1/4.4
14	.70	.70	.56	.93	1.01	.78	.15	1/5.2
15	1.32	1.23	.91	1.10	1.28	1.17	.13	1/9.0
16	1.00	.90	.71	.99	.99	.92	.09	1/10.2
17	.48	.60	.63	.51	.54	.55	.05	1/1.1
18	.90	.87	1.03	.90	.69	.88	.08	1/1.1
19	.50	.56	.53	.41	1.39	.68	.29	1/2.4
20	.48	.68	1.13	.62	1.11	.80	.25	1/3.2
21	1.00	.60	.65	.48	.94	.73	.19	1/3.8
22	.74	.55	.73	.96	.69	.73	.09	1/8.1
23	1.28	.96	.74	.84	.82	.93	.15	1/6.2
24	1.20	.74	1.04	1.23	1.67	1.18	.23	1/5.1
25	1.10	.85	1.05	1.24	1.06	1.06	.09	1/11.8
Av.	.89	.73	.78	.77	1.06	.85	.17	1/5.1
Women								
1	.72	1.46	1.40	.94	1.19	1.14	.25	1/4.6
2	.68	.32	.65	.54	.99	.64	.16	1/4.0
3	.58	.42	.73	.61	1.13	.69	.19	1/3.6
4	1.68	1.35	1.00	1.21	1.92	1.43	.29	1/4.9
5	.48	1.10	.72	.78	1.13	.84	.22	1/3.8
6	.68	.82	.50	.90	1.10	.80	.17	1/4.7
7	.54	.60	.92	.60	1.63	.86	.33	1/2.6
8	.64	.61	.65	.79	.72	.68	.06	1/11.3
9	.56	1.16	.69	.95	1.15	.90	.22	1/4.1
10	.36	.85	.63	.99	2.13	.99	.45	1/2.2
11	.42	.42	1.08	1.27	.81	.80	.30	1/2.6
12	.84	1.55	.63	.80	2.03	1.17	.50	1/2.3
13	.70	1.00	.89	.68	1.08	.87	.14	1/6.2
14	.88	.56	.61	1.09	1.02	.83	.20	1/4.2
15	1.10	.80	.63	1.10	.93	.91	.16	1/5.7
16	.80	.50	.80	.55	.56	.64	.13	1/5.0
17	.90	.60	.68	1.27	1.39	.97	.27	1/3.6
18	1.40	.64	.58	.89	1.44	.99	.34	1/2.9
19	.54	.80	.83	.61	1.39	.83	.22	1/3.8
20	1.36	.95	1.04	.88	1.52	1.15	.23	1/5.0
21	.68	.75	.75	.53	.83	.71	.08	1/9.0
22	.78	.65	.93	.99	.59	.79	.14	1/5.6
23	.74	.52	.93	2.02	1.50	1.14	.49	1/2.3
24	.40	.75	1.11	1.39	1.46	1.02	.36	1/2.8
25	.36	.50	.26	.65	.76	.51	.16	1/3.2
Av.	.75	.79	.79	.92	1.22	.90	.24	1/6.4

from those of the first part of the Table. The small differences are generally in favor of more uniformity and keener discrimination on the part of the women.

These tables of total errors show many interesting facts not revealed as clearly by either the variable or the constant error. Why, for example, should the shortest line show a larger per-cent. of error than the average of all lines? The explanation of this point may also show why several of the women are more influenced by this than are any of the men. Besides the peculiar interest of the tables themselves to one studying this subject, it is necessary to use the total error for comparisons with later results, in which not enough experiments were made by each individual to justify working out the variable error.

Comparing the total error with the variable error, we find that the former is not without value as a measure of the relative sensibility when many persons are employed. Of course it is not accurate; but, as has been already indicated, sometimes approximate results from many persons are of more practical value than more accurate measurements of a few. These two errors are, on the whole, nearly enough parallel to enable us to interpret the tables that contain only the constant and the total errors.

The details of the Table are doubtless sufficiently clear without comment. The decrease in the size of the variable error as compared with the total error is accompanied by a greater increase in the uniformity of the results from individuals. With a mean error of .85% the men show an individual variation of only .14%. The average of the mean variations from their own average for the different lengths is only .17%. The women show an average, mean variable error of .90%, and a variation from this of .16%. The average of the mean variation from their own averages is .24%.

There are several points connected with these averages that require some notice. The small individual variation we must accept as a fact requiring no explanation. People of the same general intelligence, age and mode of life compare lines with about the same results. The results of any one individual for the various lengths of lines need a word here, though the meaning of this equality will be considered later. We observe that these individual variations from the individual average vary, for the men, from .05% to .39%, with an average of .17%; and, for the women, from .06% to .50%, with an average of .24%. At first thought, so small a variation for the average of inexperienced observers, making so few experiments, may seem strange, especially when it is remembered that Münsterberg, with four times as many lines and twice as many experiments for each line, found for himself a corresponding error nearly one-half larger, viz., .33% (.3 in his table). Again, Münsterberg's special apparatus for making these experiments ought to have enabled him to compare lines more accurately than is possible without special apparatus. It will also be noticed that Münsterberg's average variable error was 1.67% (1.7 in his table), while that of these students was but .87%. We need not seek an explanation of the difference in the effect of Münsterberg's glasses "which competely corrected all defects of his eyes." In the first place, my figures are based on the total length of the lines employed; hence, when compared with experiments in which one line is made equal to a given line, my figures should be doubled. In the second place, we should expect a greater degree of accuracy in halving a line than in making one line equal to another, even when measured on the same standard. That is, it is certainly easier to compare two parts of a line, when both parts may be varied, than to compare them when one part must remain constant. Furthermore, it is supposed that scientific method requires the systematic arrangement of the experiments so that half are made by beginning with a shorter line and half by starting from a line that is clearly longer. This also interrupts the free judgment that is necessary for the accurate comparisons. I am not now discussing the desirability of the accepted method, but am merely calling attention to the fact that the total errors must be larger under this rule than when the very unstable thing called 'judgment' has fewer, or no, restrictions. Fischer found that the error in halving a line was only about one-half the error in attempting to make one line equal to another. Both errors were based on the same length of line, in the one case on half the line to be halved, and in the other case on an equal standard line. Hence it is not strange that Münsterberg made greater errors than my untrained students.

Fischer's experiments in halving shorter lines (10 to 100 mm.) show smaller variable errors that those of Münsterberg. The average variable error by the first method was .68% of the half line. By the second method the average variable error was 1.30% of the whole standard line. Averages of other observers are as follows: Fechner 1.61%, Volkmann 1.05%, Chodin 1.45%. On the whole, the results from my untrained observers compare very favorably with the work of experienced psychologists, both in the absolute size of the variable error and in the variation from the mean.

As already stated, the gross error is not usually considered a measure of discriminative power. Nevertheless, with the data at hand, some more or less important conclusions may be drawn from a comparison of the total errors. The experiments now to be described were performed exactly as those of former classes, except that only half as many experiments were made. Table IV. contains the summaries of five judgments for each of five horizontal lines by students in psychology for the academic years 1893, '94 and '95. To save space, the Table gives the averages for five students in a single group. As before, the names were written alphabetically, and the divisions formed in order. The three classes were kept separate, and the men were arranged first in each class. At the time the experiments were made these persons had never heard of Weber's law.

Table V. contains a record of similar experiments made by children of the eighth, fourth and first grades in the Lincoln public schools. Instead of arranging the names alphabetically, they were left in the chance order in which the results were first measured amd recorded. Partly on account of lack of time, and partly on account of several sources of error not found in other experiments, only three lengths of lines were given to the children in the public schools.

The training in high school and college apparently does not greatly assist one to measure lines by the eye. Such additional sources of error as are introduced in these grades do not apply to the shortest line, and yet the average error for this line is but little greater for these pupils than for juniors and seniors in the university. The chief source of error, possibly affecting these results, was in the short desk-top on which the lines were judged. The middle of this top may have been marked by some of the pupils, though the inkstand was at one side. The error from this source was not great, as is shown by the results themselves. In the first place, it could affect only the longest line, and the averages for this line have nearly the same relation to those of the other lines as is found among both older and younger observers. In the second place, boys more frequently deface the desks than the girls, yet the boys show no better results in the long lines than do the girls.

Children of the first grade were too immature to understand general directions; hence results from them were obtained only by an individual method. Each child was told what he was to do, in the plainest language, and this was repeated and explained until he understood it. Much time was required to get the smallest children to do the work. They were also very careful and deliberate, thus again using much more time. It was not expected that results from very small children would show great regularity, or have much value. For these reasons little work was attempted with so young children. Only two lengths of lines were used and only two judgments for each line were made by each child. In the Table are given together the averages for

TABLE IV. (Horizontal Lines)

Relation of Total Average Error to Length of Line, Expressed in Hundredths (University Students in Psychology)

	Line, mm.	50	100	200	350	500	M	V	R
Men	Number of Observers								
Class of 1893	5	1.18	1.60	2.20	1.62	1.95	1.71	.29	1/5.9
	5	1.28	1.54	1.14	1.58	1.15	1.34	.18	1/7.4
	5	1.44	1.24	1.04	1.34	1.86	1.38	.21	1/6.6
	5	.96	1.08	1.50	1.78	1.81	1.43	.32	1/4.5
	4	1.55	1.08	1.01	1.26	.93	1.17	.19	1/6.2
Class of 1894	5	1.48	1.68	1.89	1.49	1.35	1.58	.17	1/9.3
	5	1.04	1.10	1.23	.97	.91	1.05	.09	1/11.7
	5	1.04	.86	1.03	1.09	.97	1.00	.07	1/14.3
	5	1.36	1.02	1.26	1.01	.96	1.12	.15	1/7.5
	5	1.20	1.26	1.33	1.57	1.18	1.31	.11	1/11.9
	5	.76	.72	.99	1.01	1.24	.94	.16	1/5.9
Class of 1895	5	1.48	1.28	1.36	1.60	1.82	1.51	.16	1/9.4
	5	.84	.60	.86	.73	.78	.76	.08	1/9.5
	5	1.56	1.54	1.75	1.59	1.93	1.67	.13	1/12.8
	5	1.40	1.14	1.19	1.18	.98	1.18	.09	1/13.1
	5	1.24	1.14	.82	.91	1.20	1.06	.16	1/6.6
	5	1.80	1.16	1.37	1.05	.85	1.25	.27	1/4.6
	5	1.08	1.04	1.03	1.22	1.81	1.24	.23	1/5.4
	5	.84	1.00	1.32	1.54	1.49	1.24	.25	1/5.0
	4	.85	1.38	1.30	1.05	1.28	1.17	.18	1/6.5
	Av.	1.22	1.17	1.28	1.28	1.32	1.25	.17	1/7.4
Women Class of 1893	5	1.16	1.10	1.29	1.10	1.29	1.19	.08	1/14.9
	5	.92	1.14	1.32	1.51	1.49	1.28	.20	1/6.1
	5	1.44	1.30	1.30	1:51	1.46	1.40	.08	1/17.5
	3	1.60	1.10	1.17	.83	1.29	1.20	.20	1/6.0
Class of 1894	5	1.60	1.70	2.11	2.52	2.70	2.13	.39	1/5.5
	5	1.36	1.30	1.30	.58	.79	1.13	.29	1/3.9
	5	.84	1.02	1.00	1.18	1.27	1.06	.13	1/8.2
	5	1.04	1.24	.65	1.18	1.63	1.15	.24	1/4.8
	5	1.12	1.10	1.56	1.70	1.16	1.33	.24	1/5.5
	4	1.08	.72	1.45	1.39	1.72	1.27	.30	1/4.2
	4	1.00	1.33	1.87	1.56	1.93	1.54	.30	1/5.1
Class of 1895	5	1.12	1.24	1.22	1.23	1.58	1.28	.12	1/10.7
	5	1.20	1.06	1.08	1.07	1.49	1.18	.13	1/9.1
	5	1.28	1.06	1.14	1.31	1.58	1.27	.14	1/9.1
	5	1.32	1.10	1.06	1.58	.93	1.20	.20	1/6.0
	5	1.44	1.38	1.72	1.86	.94	1.47	.26	1/5.7
	6	1.07	1.10	.80	1.11	1.06	1.03	.09	1/11.4
	6	1.30	.98	.95	1.25	1.27	1.15	.15	1/7.7
	Av.	1.22	1.17	1.28	1.36	1.42	1.29	.20	1/6.6

TABLE V. (Horizontal Lines)
Relation of Total Average Error to Length of Line, Expressed in Hundredths (School Children)

Line, mm.	Number of observers		50		200		500		Mean	
Eighth	Boys	Girls	Boys	Girls	Boys	Girls	Boys	Girls	Boys	Girls
Grade	5	5	1.08	1.56	.96	2.27	1.54	1.61	1.19	1.81
	5	5	1.80	1.16	.82	1.23	1.15	1.32	1.26	1.24
	5	5	.96	.80	1.11	.52	1.46	1.28	1.18	.87
	4	5	1.30	.92	.82	.89	.89	1.22	1.00	1.01
		5		1.04		1.15		1.65		1.28
		5		1.08		1.25		1.17		1.17
		5		1.80		1.86		1.25		1.64
		6		1.43		1.44		1.28		1.38
		6		1.23		1.79		1.21		1.41
Average			1.29	1.22	.93	1.35	1.26	1.33	1.16	1.31
Fourth	5	5	1.32	1.40	.91	1.09	1.02	2.02	1.08	1.50
Grade	5	5	.96	1.88	1.15	1.20	2.14	1.85	1.42	1.64
	5	5	2.24	2.36	1.35	1.39	1.94	1.78	1.84	1.84
	5	5	1.20	2.40	1.28	1.68	1.75	2.48	1.41	2.19
	5	5	2.44	1.40	1.18	1.47	1.41	1.75	1.68	1.54
	5	5	2.28	1.88	1.55	1.36	1.76	1.42	1.86	1.55
	5	5	1.68	1.16	1.52	1.06	1.91	1.39	1.70	1.20
	5	5	1.98	1.48	1.95	1.77	2.21	1.76	2.05	1.67
	5	5	2.60	2.64	2.26	2.62	2.27	2.11	2.38	2.46
		4		1.92		1.18		3.74		2.28
Average			1.86	1.85	1.46	1.48	1.82	2.03	1.71	1.79
First	5		2.75		2.84				2.80	
Grade	5		2.25		2.37				2.31	
	5		2.04		2.32				2.18	
	5		2.00		2.10				2.05	
	5		2.00		1.56				1.78	
	5		2.42		2.30				2.36	
	5		1.92		1.62				1.77	
	5		2.00		1.81				1.91	
	5		2.25		2.51				2.38	
	5		3.08		2.56				2.82	
Average			2.27		2.20				2.24	

twelve children, arranged in the chance order of original measurements. That is, each number is the average of twenty-four judgments, made by twelve different children.

The increase of the total error is certainly not greater than we should expect. The uniformity is surprisingly great. When it is remembered that no results were omitted, and that the order of arrangement was in no case changed from the chance order in which the results were originally measured and recorded, the constancy of even child-nature receives another witness.

For comparison with these few experiments on many persons I desired to have some results depending on a larger number of experiments than the general student would have time to make. Several students made additional experiments, but at the time I was most interested in this work few students were giving much attention to psychology. In recent years other problems have engaged our attention, and interested students have been directed to other work. Mrs. Wolfe and myself, however, made many further experiments. Those conducted under the same conditions as have been described are summarised in Table VI. The relation of the variable error to the length of line is presented. Ten judgments for each length of line constitute a series. The series are arranged in the order in which they were made. The effect of practice, therefore, may be easily seen. Usually a half-series was made at one sitting. About half the experiments in this Table were made before any results were measured. In later experiments, of course, we tried to be as objective as possible. Since the constant error in our cases was very small, I do not think that the knowledge gained by measuring the earlier results greatly influenced our judgments. Moreover, the variable error was not worked out till the experiments were finished. This circumstance, however, must be taken at no more than its real value. I am confident that the influence of just such factors is playing a very important part, even in more "exact" investigations than those described in this article.

Practice is seen to have considerable effect only in the case of H. K. W. It seems to manifest itself nearly as clearly in the short lines as in the long ones. The addition of the longer lines (750 and 1000 mm.) produces no perceptible disturbance either in the estimation of the shorter lines or in the final average. Subjectively there was a decided feeling of difference between judging lines 50 mm. long and lines 1000 mm. long. It is probable that the greater ease experienced when judging the shorter lines just after the long is a reason for the negative effect of practice noticed in the short lines of K. B. W. It was certainly very difficult to give the same degree of attention to the comparatively easy task of marking the middle of a line 50 mm. long, that one was unconsciously compelled to give to a line twenty times as long.

The smaller number of experiments in a series is sufficient to explain the rather large variation of the numbers in the body of the Table. However, when the series are taken as units the average is remarkably constant, especially in case of K. B. W., and the variation for the different lengths of lines is exceedingly small. Münsterberg's average variable error was almost exactly five times our average variation. For H. K. W. it is .60%, and for K. B. W. it is .49%. But if we consider the series as units, the average error is for H. K. W. .61% with an average variation of .14, which is less than one-fourth of the average. The remarkable regularity of the work of K. B. W. is seen in this manner of presenting the data. Her average is .49%; and the average variation from this number is only .10, or about one-fifth of the average variable error.

TABLE VI. (Horizontal Lines)
Relation of Variable Error to Length of Line, Expressed in Hundredths: H.K.W. (man) and K.B.W. (woman)

Line, mm.	50	100	200	350	500	750	1000	Mean		
H.K.W.										
First ten	.74	.55	.72	.96	.67			.73	.10	1/7.3
Second "	.82	.44	.54	.52	1.27			.72	.26	1/2.8
Third "	.56	.64	.78	.83	.62	.70	.57	.67	.08	1/8.4
Fourth "	.56	.38	.70	.50	.72	.61	1.02	.64	.15	1/4.3
Fifth "	.48	.42	.68	.66	.65	.98	1.07	.71	.18	1/4.0
Sixth "	.68	.58	.75	.46	.82	.58	.63	.70	.11	1/6.4
Seventh "	.48	.40	.62	.74	.80	.59	1.25	.70	.20	1/3.5
Eighth "	.48	.40	.71	.71	.50	1.01	.78	.66	.17	1/3.9
Ninth "	.36	.60	.68	.66	.65	.35	.80	.59	.13	1/4.5
Tenth "	.60	.60	.41	.34	.96	.60	1.20	.67	.23	1/2.9
Eleventh"	.62	.45	.54	.42	.61	.38	.66	.53	.09	1/5.9
Twelfth "	.40	.18	.55	.37	.45	.20	.54	.38	.11	1/3.5
Thirteenth	.70	.37	.48	.42	.63	.47	.77	.55	.13	1/4.2
Fourteenth	.54	.38	.55	.30	.48	.53	.52	.47	.08	1/5.9
Fifteenth"	.54	.50	.20	.56	.39	.46	.28	.42	.11	1/3.8
Average	.57	.46	.59	.56	.68	.57	.78	.61	.14	1/4.4
K.B.W.										
First ten	.36	.50	.26	.65	.76			.51	.16	1/3.2
Second "	.32	.40	.45	.44	.50			.42	.05	1/8.4
Third "	.20	.46	.36	.50	.82	.31	.65	.47	.16	1/2.9
Fourth "	.48	.38	.52	.61	.78	.31	.34	.49	.13	1/3.8
Fifth "	.48	.44	.52	.44	.25	.39	.41	.42	.06	1/7.0
Sixth "	.48	.40	.45	.37	.35	.44	.80	.47	.10	1/4.7
Seventh "	.68	.48	.38	.54	.45	.48	.26	.46	.09	1/5.1
Eighth "	.48	.65	.64	.71	.67	.62	.74	.64	.06	1/10.7
Ninth "	.36	.30	.61	.49	.63	.61	.44	.49	.11	1/4.5
Tenth "	.60	.51	.43	.49	.48	.47	.69	.52	.07	1/7.5
Average	.44	.45	.46	.52	.57	.45	.54	.49	.10	1/4.9

Experiments with Vertical Lines

The experiments with vertical lines were made at the same time as those with horizontal lines, and under conditions already described. Although there was another new factor introduced besides the change in position of the lines, I believe the results may have some value by direct comparison with those of the horizontal lines. Since we used paper strips for lines, a difficulty was met when we came to mark a vertical line. The strip would not remain upright without some holder. Convenient holders could easily be obtained, but they disturbed the judgment, if allowed to appear on the paper. Any holder that would not interfere with the judgment required time to adjust; hence we employed the natural though rather primitive holder that each student was accustomed to use,—the hand. For the short lines I do not believe that this method was very objectionable. For the two longest lines (750 and 1000 mm.) it was at least inconvenient. Since these longer lines were used only by persons much interested in the work, part of the defect was counterbalanced. A special stand with vertical board was necessary for such work, and this precluded investigation in the city schools, without more time than could be given to the subject. The question is one that will repay further investigation, especially upon younger children.

I shall present these Tables in the same order as those dealing with the results of horizontal measurements. Here also I shall defer treating of the theoretical bearing of these facts, and especially their relations to the results of other investigators, and to the psychophysic law.

If we compare Tables VII. and II., the most noticeable difference is the great increase in the average total error. For horizontal lines the average for all lengths, and all persons was: men, 1.28; women, 1.22. For vertical lines this rises to 1.73 for men, and to 1.74 for women. Further experiments show that far the larger part of this increase is due to the judgment of long lines. Indeed, it is true that the total error for very short lines (50 mm.) is less for vertical than for horizontal lines. The increase of error with that of length of line is fairly regular, till the longest line shows more than twice the relative error of the shortest. Such large errors are, of course, some indication of the greater difficulties in the way of these experiments. They are accompanied by very great irregularities in results. With facts of this kind it is clearly useless to take the average variations from the mean result for the separate lengths, except for the sake of showing uniformity, or the lack of uniformity, among individuals of the general standard of age and intelligence. However, even more interesting ways of comparing individuals are found in the mean of all individuals for any one length of line, and in the average variation from this mean. Comparison made in this manner shows very little difference between the results from vertical and horizontal lines; and yet the individual variations are enormous. Even the greatest relative difference is found with the longest lines.

Men and women show a strange agreement for three of the five lengths. In the shortest line the women are better than the men, and in the next to the longest line the men make a smaller error. The average for all lines and all persons is almost exactly the same for men and women (men, 1.73; women, 1.74). The greatest and also the least errors are made by men. Number

TABLE VII. (Vertical Lines)

Relation of the Total Average Error to Length of Line, Expressed in Hundredths (University Students in Psychology)

Line, mm.	50	100	200	350	500			
Men								
1	1.30	1.45	2.52	1.97	3.01	2.05	.57	1/3.6
2	.90	.85	.58	.30	1.14	.75	.25	1/3.0
3	1.60	1.55	2.30	1.40	1.86	1.74	.27	1/6.4
4	1.10	1.70	2.32	2.64	3.16	2.18	.63	1/3.5
5	.60	.60	1.05	1.16	2.39	1.16	.49	1/2.4
6	1.80	.80	2.72	3.31	2.85	2.30	.80	1/2.9
7	2.90	3.70	2.17	3.36	2.44	2.91	.49	1/5.9
8	.80	1.30	2.55	2.15	2.34	1.83	.62	1/3.0
9	1.20	2.20	2.10	2.11	1.54	1.83	.37	1/5.0
10	1.20	1.65	1.27	1.53	1.60	1.45	.17	1/8.5
11	.90	.80	1.50	1.23	1.95	1.28	.36	1/3.6
12	1.00	.90	.68	1.40	1.17	1.03	.20	1/5.1
13	.40	.80	.87	.86	1.78	.94	.33	1/2.8
14	.70	1.05	1.75	.80	1.40	1.14	.35	1/3.3
15	2.10	1.85	1.60	1.63	3.00	2.04	.41	1/5.0
16	.90	1.60	2.67	3.86	4.89	2.78	1.27	1/2.2
17	1.20	.40	1.40	.93	1.90	1.17	.40	1/2.9
18	2.60	2.40	3.12	4.30	3.63	3.21	.60	1/5.4
19	1.20	1.55	2.52	2.37	1.95	1.92	.43	1/4.5
20	.80	.50	1.05	2.27	6.53	2.23	1.74	1/1.3
21	1.00	.95	1.10	.96	1.62	1.13	.20	1/5.7
22	.30	.30	1.02	1.44	1.49	.91	.49	1/1.8
23	1.20	2.25	2.00	2.20	3.03	2.14	.43	1/5.0
24	.70	1.10	2.52	2.26	3.76	2.07	.93	1/2.2
25	.60	.45	.82	1.10	2.34	1.06	.53	1/2.0
Av.	1.16	1.31	1.77	1.90	2.51	1.73	.40	1/4.3
Women								
1	1.50	2.05	3.25	2.41	1.96	2.23	.48	1/4.6
2	1.10	2.55	4.27	2.67	2.96	2.71	.72	1/3.8
3	1.10	.95	1.72	2.26	3.57	1.92	.80	1/2.4
4	.80	1.80	1.90	1.69	2.16	1.67	.35	1/4.8
5	.40	1.05	1.15	1.23	1.16	1.00	.24	1/4.2
6	.70	1.75	2.55	2.31	2.31	1.92	.56	1/3.4
7	1.10	.70	1.20	2.43	3.30	1.75	.90	1/1.9
8	1.50	1.95	1.92	1.74	1.98	1.82	.16	1/11.4
9	1.50	1.25	2.10	3.43	3.95	2.45	1.00	1/2.5
10	.50	.90	.87	1.44	2.65	1.27	.62	1/2.0
11	.60	.40	.97	.50	1.24	.74	.29	1/2.6
12	.70	1.85	2.97	3.66	5.02	2.84	1.25	1/2.3
13	1.10	1.95	1.40	2.71	3.45	2.12	.75	1/2.8
14	.40	.80	1.10	1.47	2.89	1.33	.68	1/2.0
15	2.20	1.05	1.67	2.03	2.95	1.98	.50	1/4.0
16	.80	.85	1.85	1.56	2.37	1.49	.53	1/2.8
17	1.20	.60	1.10	2.53	1.05	1.30	.50	1/2.6
18	.90	.80	2.95	2.99	1.35	1.80	.94	1/1.9
19	1.40	2.75	2.66	3.00	3.28	2.61	.48	1/5.4
20	2.30	.90	1.67	2.29	3.87	2.21	.74	1/3.0
21	.80	.85	1.40	2.67	4.18	1.98	1.16	1/1.7
22	.90	1.70	.72	1.39	1.47	1.24	.34	1/3.6
23	2.10	1.00	1.77	1.20	1.94	1.60	.40	1/4.0
24	.80	1.10	.75	.84	.78	.85	.50	1/1.7
25	.20	.10	.62	1.73	1.13	.76	.54	1/1.4
Av.	1.06	1.27	1.78	2.09	2.52	1.74	.46	1/3.8

14, a man, who made astonishingly large errors on horizontal lines, has, in this table, an error very much below the average. His especial friend, number 19, a man, who had a rather large error with small variation in horizontal lines, has here the largest error in the whole Table, and a variation larger than the average error of number 2. I point out these details to show that one must be careful in generalizing from a few facts, even in such relatively simple processes as judgment of the middle of lines. The men compared above were perhaps more interested in psychology and in experimental work than any others in the class. Upon my advice, one continued his work in experimental psychology in another university, and the other studied medicine and became an oculist.

The variable error shows a similar trend, though the increase for the longer lines is less prominent. The averages for men and women are almost exactly the same. The immense constant error in vertical measurements leaves us a surprisingly small variable error. It is, however, about 14% larger than the corresponding error for horizontal lines; though the greater part of this increase falls to the longer lines. The comparison of judgments of different individuals for the same length of line is also, in this part of the Table, more suggestive than the variation of the same individual for different lines. The Table furnishes data for both kinds of comparison. There is most difference between individuals in the shortest lines, and least difference in the longest lines. Proportionally this relation is greatly increased, since, in absolute size, these two errors are just the reverse of this order. Here again the women are more constant than the men. Their average variation from the mean of all is not much less than that of the men; there are fewer large variations, both in case of different lengths of lines, and in case of different individuals.

This Table, in connection with Tables II. and III., indicates that it is slightly more difficult to mark the middle of short vertical lines than of short horizontal lines, and that this difference increases as the line grows longer, but not in the same proportion. It must not be forgotten, however, that the methods of holding the paper strips made it more inconvenient to mark the long lines. While this doubtless produced its chief effect upon the constant error, it probably influenced also the variable error by making the observer more or less uncomfortable.

Table VIII. contains the relation of the total average error to the length of the vertical lines as estimated by university students of the classes of 1893, '94, and '95. Though only five judgments were taken for each length of line, these results may

TABLE VIII. (Vertical Lines)

Relation of Total Average Error to Length of Line, Expressed in Hundredths (University Students)

	Line, mm.	50	100	200	350	500	M	V	R
Men Class	Number of observers								
of	5	1.20	1.52	1.75	2.26	3.25	2.00	.61	1/3.3
1893	5	1.40	1.52	2.05	1.87	2.78	1.92	.39	1/4.9
	5	1.28	1.44	1.48	2.07	2.88	1.83	.52	1/3.5
	5	1.12	1.50	1.42	2.07	3.18	1.86	.61	1/3.1
	4	1.30	1.15	1.35	1.67	2.07	1.51	.29	1/5.2
Class	5	1.28	1.42	1.88	2.61	2.78	1.99	.56	1/3.6
of	5	.96	1.56	1.28	2.10	2.48	1.68	.49	1/3.4
1894	5	1.40	1.12	1.11	1.20	1.57	1.28	.16	1/8.0
	5	.80	1.04	1.21	1.10	1.19	1.07	.12	1/8.9
	5	1.16	1.14	1.47	1.77	1.89	1.49	.28	1/5.3
	5	.92	1.06	1.18	1.21	1.29	1.13	.11	1/10.3
Class	5	1.04	1.26	1.71	1.25	1.63	1.38	.23	1/6.0
of	5	1.36	1.30	1.03	1.50	1.79	1.40	.20	1/7.0
1895	5	1.16	.86	1.63	2.02	1.86	1.51	.40	1/3.8
	5	1.12	1.22	1.47	1.71	1.90	1.48	.26	1/5.7
	5	1.32	1.42	1.80	1.38	1.99	1.58	.25	1/6.3
	5	1.64	1.40	1.53	2.13	2.16	1.77	.30	1/5.9
	5	1.08	.92	1.24	1.63	1.80	1.33	.30	1/4.4
	5	1.56	1.58	1.21	1.63	2.32	1.66	.26	1/6.4
	4	1.10	1.50	1.88	1.80	2.79	1.81	.42	1/4.3
	Average	1.21	1.30	1.48	1.75	2.18	1.58	.30	1/5.3
Women	5	1.20	.94	1.99	2.00	2.72	1.77	.56	1/3.2
Class	5	.88	1.20	1.50	1.33	2.42	1.47	.40	1/3.7
of	5	1.64	1.52	1.45	1.56	2.52	1.74	.31	1/5.6
1893	3	1.47	1.07	1.88	1.78	3.36	1.91	.58	1/3.3
Class	5	1.12	1.64	1.71	2.20	3.16	1.97	.57	1/3.5
of	5	1.04	.66	1.35	1.86	2.68	1.52	.60	1/2.5
1894	5	1.08	1.46	1.15	2.32	2.68	1.74	.61	1/2.9
	5	1.40	1.52	1.66	1.79	2.08	1.69	.20	1/8.5
	5	1.32	.90	1.45	1.05	1.30	1.20	.18	1/6.7
	4	.88	1.18	1.48	1.60	1.87	1.40	.30	1/4.7
	4	1.20	1.37	1.20	1.31	2.14	1.44	.20	1/7.2
Class	5	1.48	1.08	1.25	2.49	2.66	1.79	.63	1/2.8
of	5	1.88	1.72	1.04	1.43	1.96	1.61	.30	1/5.4
1895	5	1.20	1.28	1.73	1.20	1.66	1.41	.22	1/6.4
	5	1.20	1.34	1.96	2.94	2.80	2.05	.66	1/3.1
	5	1.52	1.20	1.49	1.89	2.04	1.63	.27	1/6.0
	6	1.60	1.37	1.99	1.65	1.96	1.71	.21	1/8.1
	6	1.27	1.33	1.55	1.52	2.05	1.54	.20	1/7.7
	Average	1.30	1.27	1.55	1.77	2.34	1.65	.33	1/5.0

TABLE IX. (Vertical Lines)

Relation of the Variable Error to Length of Line, Expressed in Hundredths (University Students in Psychology)

Line, mm.	50	100	200	350	500	M	V	R
Men								
1	.90	.95	1.19	.84	1.33	1.04	.17	1/6.1
2	.96	.85	.58	.28	.92	.72	.23	1/3.1
3	1.24	.86	1.02	1.47	1.81	1.28	.29	1/4.4
4	1.10	.98	.81	.50	1.38	.95	.24	1/4.0
5	.76	.60	.68	1.16	1.59	.96	.33	1/2.9
6	1.80	1.40	1.95	1.05	1.54	1.56	.26	1/6.0
7	1.48	.56	1.38	2.16	.78	1.27	.48	1/2.7
8	.96	1.14	.60	.51	1.17	.88	.26	1/3.4
9	.64	1.04	.72	1.29	1.01	.94	.21	1/4.5
10	.64	.92	1.10	1.13	1.68	1.09	.25	1/4.4
11	1.18	.62	.59	1.14	1.19	.94	.27	1/3.5
12	.40	.96	.68	.85	1.25	.83	.23	1/3.6
13	.48	.66	.73	.72	1.49	.82	.25	1/3.3
14	.74	.67	1.41	.63	1.40	.97	.35	1/2.8
15	.94	1.60	1.30	1.00	1.82	1.33	.30	1/4.4
16	.90	.70	1.24	1.31	2.57	1.34	.49	1/2.7
17	.88	.40	1.41	.93	1.90	1.10	.44	1/2.5
18	.80	.88	1.38	1.01	1.12	1.04	.17	1/6.1
19	.48	.47	.97	1.42	1.00	.87	.31	1/2.8
20	.88	.58	.96	.56	1.00	.80	.18	1/4.4
21	.80	.85	.71	.61	.68	.73	.08	1/9.1
22	.36	.34	.83	1.03	1.05	.72	.30	1/2.4
23	.68	.68	.60	1.10	1.24	.86	.25	1/3.4
24	.82	1.18	1.25	1.83	1.53	1.32	.29	1/4.6
25	.70	.55	.78	.70	1.02	.75	.12	1/6.3
Av.	.86	.82	.99	1.01	1.34	1.00	.27	1/3.7
Women								
1	1.68	1.95	1.30	1.01	1.02	1.39	.34	1/4.1
2	.56	.77	.53	.71	1.57	.83	.30	1/2.8
3	.56	.70	.73	.69	1.23	.78	.18	1/4.3
4	.88	1.08	1.25	.70	.76	.93	.18	1/5.2
5	.48	.95	1.09	1.24	1.20	.99	.22	1/4.5
6	.42	.60	.78	1.30	1.43	.91	.37	1/2.5
7	1.12	.62	.87	1.28	1.48	1.07	.26	1/4.1
8	.70	.90	1.26	1.40	1.94	1.24	.35	1/3.5
9	.90	.70	.65	1.69	1.42	1.07	.39	1/2.7
10	.56	.84	.78	1.24	1.27	.94	.25	1/3.8
11	.72	.40	.83	.35	1.20	.70	.26	1/2.5
12	.80	1.28	1.67	1.46	1.10	1.26	.25	1/5.0
13	.94	1.34	1.08	.79	1.04	1.04	.14	1/7.4
14	.56	.70	1.05	1.16	1.89	1.07	.36	1/3.0
15	1.04	.66	1.40	.91	1.46	1.09	.27	1/4.0
16	.80	.70	1.20	.70	1.40	.96	.27	1/3.6
17	.90	.62	.85	1.16	1.05	.92	.15	1/6.1
18	1.10	.56	1.74	1.27	1.22	1.18	.28	1/4.2
19	1.00	.95	1.15	.67	1.93	1.14	.32	1/3.6
20	1.90	.62	1.26	1.46	1.63	1.37	.35	1/3.9
21	.80	.75	.95	1.10	1.38	1.00	.20	1/5.0
22	.54	1.12	.58	.96	1.39	.92	.29	1/3.2
23	.90	.68	1.75	1.22	1.68	1.25	.38	1/3.3
24	.40	1.10	.75	.83	.75	.77	.16	1/4.8
25	.20	.16	.44	.92	.48	.44	.21	1/2.1
Av.	.82	.83	1.04	1.05	1.32	1.01	.27	1/3.74

be directly compared with the first part of Table VII. Their value is less than that of other classes only because of fewer judgments. The same time and care were given, both in making the experiments, and in measuring and tabulating the results.

In general these results resemble those already presented. There is a decided increase in the relative total error for the longer lines. Except in case of the longest line as judged by the women this increase is comparatively regular. The men rather excel the women in regularity of judgment, and also in the smaller size of the errors. The two sexes differ, however, only in the slightest degree. Comparing Table VII. and Table VIII. we observe that, while the errors of the earlier classes were smaller for the short lines, they were larger for the long lines than those made by later classes. The larger number of experiments represented by each number in Table VIII., together with the grouping of several persons, tends to make the results of these groups more uniform than the results of the individuals represented in Table VII. I do not know why the results with the longest lines should show less total error in Table VIII. than in Table VII. Two possible reasons suggest themselves, but I cannot measure the influence of either. First, students may, perhaps, have less interest in such work after a first sitting. Secondly, with later classes, a pin slightly longer and more convenient than that used by the early classes was employed.

TABLE X. (Vertical Lines)

Relation of Variable Error to Length of Line, Expressed in Hundredths (H.K.W. and K.B.W.)

Line, mm.	50	100	200	350	500	750	1000	M	V	R
H.K.W. (man)										
First ten	.36	.34	.84	1.00	1.05			.72	.29	1/2.4
Second "	.49	.82	1.00	.74	.34			.68	.21	1/3.2
Third "	.28	.58	.74	1.11	.38	.94	.60	.66	.23	1/2.9
Fourth "	.56	.38	.70	.50	.72	.87	.42	.59	.15	1/3.9
Fifth "	.48	.41	.68	.66	.63	1.73	.68	.75	.28	1/2.7
Average	.43	.51	.79	.80	.62	1.18	.57	.70	.23	1/3.04
K.B.W. (woman)										
First ten	.20	.16	.44	.76	.47			.41	.18	1/2.3
Second "	.18	.30	.58	.50	.69			.45	.17	1/2.6
Third "	.20	.24	.25	.57	.50	.27	.28	.33	.12	1/2.8
Fourth "	.48	.34	.43	.66	.43	.47	.53	.48	.07	1/6.9
Fifth "	.49	.41	.83	.44	.43	.56	.58	.54	.11	1/4.9
Average	.31	.29	.56	.59	.50	.40	.46	.44	.13	1/3.38

Table X. contains the corresponding results from the two observers, H. K. W. and K. B. W., again grouped in series of ten experiments for each length of line. Although not as many judgments were taken as for the horizontal lines, there are enough facts to form an interesting supplement to Table VI. It is also seen here more clearly than elsewhere in this paper how great is the factor of personal interest, and how small the effect of practice, in work of this kind. The last three groups for each observer were made long after the first five, and are not to be compared with the judgments of horizontal lines of the corresponding group. Itwill be noticed also in judgments of vertical lines that the improvement with practice is confined exclusively to the longer lines. Indeed, there is a decided tendency to neglect the shorter lines in later experiments.

The large error in case of H. K. W. on length 750 mm. remains without explanation. "It just happened that way." A consciousness of failure to do justice to that particular line was present and, perhaps, contributed to the unfortunate result. This state of mind did not arise from knowledge of results obtained by measurements, for these lines were not measured until the experiments were finished. Especially in the last group of ten a sense of failure to approximate the middle of that line grew ever stronger. Though the results of H. K. W. show a very much smaller average error than the corresponding work of students, the errors are, perhaps, no smaller than great interest might account for. The work of K. B. W. with vertical lines, and indeed with horizontal lines, must be considered very unusual. Nevertheless, it is less questionable than any other series in this paper.

As already stated, it was found impracticable to obtain judgments of the middle of vertical lines from school children. An attempt to do so under somewhat different conditions was made for one line. A strip of paper, similar to those already described, 100 mm. long, was placed upon the slightly sloping desk at right angles to the position occupied by the horizontal lines. That is, the paper lay with one end toward the child and with the other end extending away from him. Since the children inclined their heads over the desk while making their judgment, this position would not differ greatly from the normal relation of eyes and line with the method of actual vertical lines. Little objection could be made to this modification, perhaps, for the shorter lines, but it would be very difficult to follow this plan with lines 500 mm. long. For this reason only one length of line was used, and from this method the results at hand are not tabulated with the other experiments. They are given merely to show that approximately the same relations between horizontal and vertical judgments exist as in case of older persons. The groups, corresponding to those given in Table IV., each consisting of five children and twenty-five judgments, yield for the "vertical" line, 100 mm. long, the following percentages of total average error:

Eighth grade: Boys 1.48, 1.28, 2.36, 2.17 Mean, 1.82
Eighth grade: Girls 1.76, 1.52, 1.20, 1.14, 1.14, 1.26, 1.48 1.50, 1.80 Mean 1.42
Fourth grade: Boys 1.96, 2.36, 1.80, 1.58, 2.24, 2.40, 2.34, 2.38, 3.47 Mean 2.28
Fourth grade: Girls 1.66, 1.02, 1.48, 2.04, 1.62, 2.20 1.40, 1.84, 1.48, 1.48 Mean 1.62

Comparison with other results must be made with full recognition of the difference in methods. Conclusions from these numbers must be drawn with great caution.

The Constant Error

One other set of facts is yet to be offered. The constant error may be studied in the following Tables, which are presented in the same order as has been adopted in the preceding exposition. First is given the relation of the constant error of the length of the horizontal line, for the 50 students who made ten judgments for each line. Table XI. shows this relation for the algebraic sum of the errors made by each observer, and whether the error lies on the right or on the left. An error on the right means that in trying to mark the middle of a line the point was placed at the right of the true middle, *i.e.*, the right half was overestimated. For convenience, errors on the right are placed in the column headed R. Errors on the left are placed in the column marked L.

A careful inspection of the details and summary in this Table ought to prevent, in the future, such remarkable generalizations as we find in several monographs based on many experiments upon one individual. The Table will also explain in part the contradictory results obtained by different observers, and improve our appreciation of their facts as well as diminish our confidence in the conflicting interpretations of different sets of facts. Even the psychologist sometimes forgets that experimental data may differ without any fault of the observers, but that a given cause always produces the same effect.

If we are not very conservative, a decided conviction will now force itself upon us. The Table at once shows that the error on the left is more frequent in the short lines than in the long lines. In case of the women, it is found about twice as often in the short lines. When these women judge the longest lines this relation is just reversed. The same tendency is seen in the first half of the Table, though the men do not indicate as great difference. The summary of the first half of the Table shows an average constant error always on the right, but increasing much faster than the length of the lines. Beginning with .36%, it jumps immediately to .61%; then to .88%, near which it remains for the next line also; then falls to .72%. Per-

TABLE XI. (Horizontal Lines)

Relation of Constant Error to Length of Line, Expressed in Hundredths (University Students in Psychology)

Line, mm.	50		100		200		350		500		Mean		mv
Men	L	R	L	R	L	R	L	R	L	R	L	R	
1		.40		1.00		1.18		1.91		2.20		1.34	.57
2		1.50		1.15		1.28		.70	.48			.83	.58
3		.80		2.60		2.48		1.14		1.38		1.68	.69
4	.20		.50		.30			.08	.70		.32		.22
5		.10		.05		.40	.41			.71		.17	.31
6		.20		.85	.00	.00		1.16		1.91		.82	.58
7		.20		.10	.95		.80		.63		.42		.45
8		.10		1.05		1.45		1.14		.53		.85	.43
9		.40	.00	.00		.95		1.27		1.12		.75	.44
10		.80		.60		.95		.15		.67		.63	.21
11		.50		.40		.38		.34	.19			.29	.19
12	.50			.50		.20		.09	.09			.04	.27
13		.70		.90		1.58		.80		.24		.84	.32
14		1.50		2.50		4.20		3.59		3.47		3.05	.84
15		1.40		1.95		1.38		1.36		.92		1.40	.22
16		2.00		2.50		2.05		1.44		1.87		1.97	.25
17	.80		1.00			.23		.59		1.36		.08	.78
18	1.30		.55		.28			.27		.08	.36		.46
19	.50		.45			.38	.49		.11		.23		.30
20		1.80		1.65		1.78		1.96		.74		1.59	.34
21	1.00		1.15		1.00		1.26		1.26		1.13		.11
22		.10		.10		.98		1.73		.59		.70	.52
23		.40		.45		1.10		.81		.94		.74	.25
24		2.00		.70		.05		1.14		.59		.88	.54
25	.70		.15			1.58		2.24		2.18		1.03	.16
Av.	.71	.83	.63	1.06	63	1.23	.74	1.14	.49	1.19	.49	.98	.44
Mean		.36		.61		.88		.84		.72		.69	.16
Women													
1		1.10		1.45		1.62		1.11		.39		1.13	.32
2		.40		.10	.20			.23	.09			.09	.19
3	1.30		.60		.78		.27		.55		.70		.27
4	1.90		.75		.03		.67			.33	.60		.60
5		.40		.70		.05	.20		.07			.18	.30
6	1.60		1.65		1.05		.30		1.30		1.18		.40
7	.90		.75		1.90		.33			.37	.70		.58
8	2.20		2.35		.50		.43			.45	1.01		1.02
9	.30		.45			.58		1.23		2.01		.61	.80
10	.90		.65		.63			.71		1.01	.09		.76
11		.70		.40		.78		.44		1.21		.71	.23
12		.80		.75		.38		.94		1.09		.79	.18
13	.30			.25		1.05.	46		.24			.06	.47
14	.20		.55		.43			.20		1.97		.20	.71
15	.50		.00	.00	.93		.27			.41	.26		.37
16		1.00		1.00		1.05		1.27		.57		.98	.16
17		.90		1.00		1.63		1.73		2.35		1.54	.65
18	2.50		1.30			.58		.71	.65		.63		1.02
19	.10		.20		1.15		.77		2.41		.93		.68
20		.30	.85		1.48			.76	.90		.43		.77
21	.40		.25			1.40		1.46		2.21		.88	.97
22	1.30		.75			.63		1.36		1.03		.19	.98
23	.10		.60			.33	.63		1.52		.50		.50
24		2.00		1.55		2.20		1.80		1.70		1.85	.20
25	.10		.20		.05			.40	.25		.04		.18
Av.	.91	.84	.79	.80	.76	.94	.43	.96	.80	1.14	.59	.71	.58
Mean	.28		.19			.13		.40		.36		.09	.25

haps even this great difference, on account of its irregularity, would not excite much interest, were it not for the greater, though similar tendency seen in the second half of the Table. The women begin with a constant error on the left amounting to .28%. This decreases slightly for the next longer lines, but still is decidedly on the left, .19%. The next line shows the small error of .13% on the right. The next line increases this to .40%. It then falls to .36%. The series thus presents an extreme variation of .68%, which is not very different from the variation in case of the men. The same relation is discovered if we consider the average errors of those making R and L errors for the different lines. In this case, we divide the sum of the positive errors by the number making such errors, instead of dividing by the total number of observers. For the men the following results are obtained:

R	.76%	1.08%	1.24%	1.14%	1.19%
L	.88%	.64%	.52%	.74%	.49%

The corresponding figures for the women are as follows:

R	.84%	.80%	.94%	.96%	1.14%
L	.90%	.79%	.76%	.43%	.80%

It is, however, not safe to draw conclusions from these facts without remembering the method of conducting the experiments. We have also some further material to examine.

Tables XII. and XIII. contain the relations of the constant error for those later classes of university students who made fewer experiments, and for children of the public schools. The grouping is the same as in the Tables already given. While the irregularity of these separate results is considerable, whether we observe the differences caused by persons, or by lengths of lines, there are certain unexpected uniformities. It might well be supposed that a constant error would indicate a disposition to overestimate, or to underestimate, one half of a horizontal line without reference to the length of the line. We enjoy finding easy physiological explanations of these idiosyncrasies. Often these happy hits are more or less successful. For example, one of my students persistently made a larger constant error at the right (overestimated the right half of the line). More varied and numerous experiments with head fixed, each eye separately, different positions, etc., showed conclusively that this was caused in part by a lameness of the muscles of the right side of the neck, including perhaps to some extent the muscles of the right eye. Furthermore, few people have two eyes even approximately equal in power. The better eye will always do more than half of the work, especially when the task is particularly delicate.

TABLE XII. (Horizontal Lines)

Relation of Constant Error to Length of Line, Expressed in Hundredths (University Students in Psychology)

Line, mm.	50		100		200		350		500		Mean		
Men	L	R	L	R	L	R	L	R	L	R	L	R	
1893													
5		.64		1.30		1.26		.83		.93		.99	.23
5	.72		.06			.56		.98		.56		.26	.52
5	.64		.36		.06			.70		1.21		.17	.63
5	.24			.24		.60		1.00		1.10		.54	.43
4		.05		.13		.26		.71		.51		.35	.25
1894													
5	.28		.40			.25		.29		.67		.11	.36
5		.24		.90		.97		.75		.45		.64	.26
5		.28		.74		.93		.61	.23			.47	.35
5		.32		.66		.90		.62		.47		.59	.16
5		.88		.74		1.07		.91		.45		.81	.17
5		.28		.12		.54		.52		.74		.44	.19
1895													
5		1.00		1.12		1.24		1.45		1.65		1.29	.21
5		.12		.28		.60		.20	.01			.24	.16
5		1.24		.94		1.29		1.25		1.25		1.19	.10
5		.52		1.02		1.07		.83		.50		.79	.22
5		.12	.18			.02		.28		.76		.20	.26
5		1.24		.40		.39		.27	.05			.45	.32
5		.60	.30			.37		.74		1.33		.55	.41
5		.60		.52		1.18		1.31		.89		.90	.28
4		.05		.53		.54		.64		.70		.49	.18
Av.	.47	.51	.26	.64	.06	.74	.00	.75	.10	.83	.00	.58	.28
Mean		.32		.42		.70		.75		.69		.58	.16
Women 1893													
5	.36		.34		.15		.39		.50		.35		.08
5	.04		.30		.10		.17		.30		.18		.09
5	.80		.80		.78		.90		.82		.82		.03
3		.80		.10	.17			.27		.35		.27	.24
1894													
5	1.12		.60		.38		.35		.58		.61		.21
5		.44		.44		.04		.06	.08			.18	.21
5		.20		.58		.79		.79		.86		.64	.20
5	.24		.72		.05			.06		.42	.11		.30
5	.88			.86		1.48		1.11		.73		1.01	.23
4		.33		.07		.87		.88		.92		.61	.33
4	.04		.16			1.05		.54		1.28		.53	.51
1895													
5	.24		.00	.00	.34			.11		.61		.03	.27
5		.80		.10		.54		.59		.80		.57	.20
5		.04	.58		.04			.40		.32		.03	.27
5		.16		.02		.55		1.13		1.19		.61	.44
5		.56		.22		1.06		1.41		.71		.79	.35
6		.20		.67		.42		.71		.64		.53	.17
6	.30			.12		.43		.43		.65		.27	.28
Av.	.39	.44	.50	.32	.25	.72	.45	.61	.46	.73	.41	.47	.25
Mean		.07				.29		.37		.40		.22	.16

TABLE XIII.

Relation of Constant Error to Length of Line, Expressed in Hundredths (School Children)

Line		50		200		500		Mean	
	No.	L	R	L	R	L	R	L	R
Boys, Eighth Grade	5		.04		.38		.91		.44
	5	.04			.18		.76		.30
	5	.48			.79		.64		.32
	4	.00	.00		.06		.27		.11
Girls, Eighth Grade	5	1.16		.55		.00	.00	.57	
	5	.84		.53		.27		.55	
	5	.64		.20		.56		.47	
	5		.04	.79		.54		.43	
	5	.48		.51		.42		.47	
	5	.52			.35		.52		.12
	5	1.24		1.38		.50		1.07	
	6	.92		.50			.48	.31	
	6	.77		1.23			.07	.64	
Girls, Mean	47	.73		.59		.14		.49	
Boys, Mean	19	.12			.35		.65		.29
Boys, Fourth Grade	5	.68			.23	.38		.28	
	5		.24		.37	1.32		.24	
	5	1.12		.08			1.32		.04
	5	.00	.00	1.06			.14	.31	
	5	1.64		.50			.20	.65	
	5	.68			.63		1.40		.45
	5	1.12		.42		.54		.69	
	5	.68			.97	.02			.09
	5	.35		.29			1.27		.21
Boys, Mean	45	.67		.02			.23	.15	
Girls, Fourth Grade	5	.44		.67		.09		.40	
	5	1.00		.30			.60	.23	
	5	2.08		.95			.29	.91	
	5	1.84		.66			.61	.63	
	5	1.24		.13			1.57		.07
	5	1.32		.72			.14	.63	
	5	.28			.84	.70		.05	
	5	.76			.25		1.09		.19
	5	.36			.34		1.28		.42
	4		.10	.03			1.49		.52
Girls, Mean	49	.93		.20			.63	.17	
Boys and Girls First Grade	5	.08			.78				
	5		1.50		.16				
	5		.63		.61				
	5		.58		.57				
	5	.17		.15					
	5		1.08		1.61				
	5	.50			.75				
	5	.17		.21					
	5		.25		2.07				
	5		1.08		.13				
Boys and Girls, Mean	50		.42		.64				

This fact does not prove that eye measurements are not chiefly muscle measurements, though I think it weakens much of the evidence that has been adduced in recent times. It is comparatively a simple matter to make experiments with one eye closed, thus shutting off the influence of the retinal action in the closed eye. Results obtained in this manner differ from results obtained with the two eyes open. The muscular movements must have been the same in both cases; hence it is the retinal impression that causes the difference. We need not accept the extreme implication of this conclusion to grant that retinal impressions are necessary accompaniments of muscular sensations in the finer measurements of visual space. It is not a question of the original spatial sense. The eye is principally a *substitute* sense. The retina, so small and so delicately constructed, with sensitive elements so minute and near together, may be called the vernier or the micrometer sense. On the other hand it is also a prophetic, or even a trail-blazing sense.

Tables XIV. and XV. show the constant errors for H. K. W. and K. B. W. Little study of the Tables is necessary to show several important facts, some of which will be still further emphasized in the Tables of constant errors with vertical lines. The most valuable point for experimental psychology seems to me to be the fact, already referred to, that conclusions based on even very numerous experiments from one individual may be misleading. Table X. furnishes material that should make us more cautious about concluding universal laws from experiments upon one individual. What a host of varying conclusions could be drawn from separate individuals of that Table! In many cases a very much greater number of experiments would doubtless change the results, but in numerous cases tested by additional experiments the cause was found to be in the individual, and often the results from two individuals were directly contradictory. Compare number 14 with number 21. The one has a large constant error at the right, increasing with the length of the line; the other has a larger constant error at the left increasing with the length of the line. Compare these two with number 24, whose error is on the left in short lines, grows less, then changes to the right and increases with the length of the lines. Compare all these with number 2, whose constant error just reverses that of number 24. Similar series may be formed from the second half of this Table; indeed, the women show a greater lack of uniformity than the men in their constant errors. We have only to bring together the results of 24, 6, 19, 17, 8, 25, to see that each person is largely a law to himself.

The personal equation in work of this kind will be found to show a greater range than in those observations whose practical importance has compelled its exact determination. Many ele-

TABLE XIV. (Horizontal Lines)

Relation of Constant Error to Length of Line, Expressed in Hundredths: H.K.W. (man), and K.B.(womanW.)

Line, mm.	50		100		200		350		500		750		1000		Mean		Av. Variation from Mean
	L	R	L	R	L	R	L	R	L	R	L	R	L	R	L	R	
H.K.W.																	
First ten		.10		.05		.98		1.73		.59						.69	.53
Second "	.30		.20			.73		1.36		.61						.44	.55
Third "	.10		.45			.65		.91		.89		1.27		.94		.59	.49
Fourth "	.30		.15		.00	.00		.17		.48		.68		.09		.14	.26
Fifth "	.20		.60			.08		.26		.81		.57		.23		.16	.35
Sixth "		.20	.15		.00	.00	.54			.65		.33		.50		.14	.32
Seventh "		.40		.25	.10			.54		.87		.87		1.10		.56	.33
Eighth "		.60		.30	.65			.21	.45			.17		.99		.17	.41
Ninth "		.90		.20	.43		.03			.03	.09		.41			.02	.30
Tenth "		.50	.00	.00	.58		.37		.60		.20			.39	.12		.36
Eleventh "		.70	.15		.23		.13		.69		.05			.81		.04	.41
Twelfth "	.00	.00	.55		.20		.37		.74			.45	.28		.24		.28
Thirteenth "		.30	.45		.63		.11		.51			.09		.45	.12		.33
Fourteenth "		.10		.35	.25		.06		.90			.20		.52	.01		.34
Fifteenth "		.10	.00	.00	.60			.01	.45			.22		.17	.08		.26
Mean		.20	.10		.09			.25		.04		.35		.35		.14	.16
K.B.W.																	
First "	.10		.20		.05			.40	.25						.04		.18
Second "	.20		.20			.38	.07		.42						.10		.21
Third "	.00	.00	.45			.05		.41	.00	.00		.23		.39		.09	.22
Fourth "		.20	.15			.35	.07		.53		.04			.26		.01	.23
Fifth "		.20	.20			.20		.21	.17			.09		.08		.06	.14
Sixth "		.80		.50		.33		.09	.11		.09			.03		.22	.28
Seventh "		.40		.35		.13		.53		.39	.25		.64			.13	.33
Eighth "		.40		.75		.66		.91		.41		.34		.22		.53	.21
Ninth "	.10		.00	.00		.33		.36		.95		.07	1.41			.03	.46
Tenth "		.40		.15	.03			.60		.28		.11	.60			.13	.26
Mean		.20		.06		.24		.34		.06		.05	.17			.11	.13

TABLE XV. (Vertical Lines)

Relation of Constant Error to Length of Line, Expressed in Hundredths: H.K.W. (man), and K.B.W. (woman)

Line, mm.	50		100		200		350		500		750		1000		Mean		Av. Variation from Mean
	U	O	U	O	U	O	U	O	U	O	U	O	U	O	U	O	
H.K.W.																	
First ten	.10		.20		.83			1.07		.95						.18	.67
Second "		2.30		1.40		1.75		1.37		2.88						1.94	.52
Third "		.20		.20		.60		.99		1.38		1.70		2.66		1.10	.69
Fourth "		.50	.25			.45		.74		.90		1.13		2.96		.92	.64
Fifth "		1.00		1.10		.45		1.64		.11		3.52		1.96		1.40	.84
Average	.10	1.00	.23	.90	.83	.81	.00	1.16	.00	1.24	.00	2.12	.00	2.53		1.11	.67
Mean		.78		.45		.48		1.16		1.24		2.12		2.53		1.25	.61
K.B.W.																	
First ten	.00	.00		.10		.53		1.64		1.13						.68	.56
Second "	.10		.20		.38			.76		.88						.19	.50
Third "		.20	.10		.00	.00		.89		.42		.40		.70		.36	.28
Fourth "	.40		.60		.18			.39		.61		.75		.58		.16	.45
Fifth "	.30		.15		.23			.23		.77		.86		1.29		.35	.53
Average	.27	.20	.26	.10	.26	.53	.00	.72	.00	.76	.00	.67	.00	.86	.00	.35	.46
Mean	.12		.19		.05			.72		.76		.67		.86		.38	.43

ments constitute the cause of this variation of individuals. Some of them are dependent upon the organs of vision and their accessories; others depend upon more general asymmetry of the two sides of the body; others again upon some temporary factor whose explanation I cannot attempt to give.

In illustration of the above points some facts not shown in the Tables may be mentioned, though only a very few can be given place. Certain individuals will have an error regularly at the right for every length except one, and that one shows an error on the left every time. Here the conditions are all alike except length of line, and the different lines are taken in regular order from shortest to longest, only one judgment being made for any one line. When the error is small no one is surprised, but often the error is considerable on each side; and when the refractory line occurs in the middle of the series, as 200 mm. or 250 mm., there is more wonder than explanation. The effect of first judgments on succeeding ones is well known. This explanation seems scarcely sufficient to account for the facts as just described, though we do not understand the influence of subconscious states. A somewhat similar phenomenon was sometimes observed. Each student made five double series at one sitting. The remaining five double series were made a week later. In general these two sets agreed very well. Some individuals, however, reversed the direction of their constant errors at this second trial. When this error was large and uniform at each sitting, but on different sides of the true middle, desire for explanation arose. Sometimes only one length gave a large and uniform constant error at the first sitting. This error fell on the other side the next week, just as large, and with the same irregularity in the other results. This could be easily explained if the students knew the results of the first work, before beginning the next. Since this was not the case we must seek a solution in other ways. The facts suggest that the length of line exerts a peculiar influence upon some observers, when judging in regular order the middle of lines differing in length. What part, if any, of this result is due to the effect of the preceding judgment of the shorter line, or to the unconscious influence of the first judgment of the line having the same length, cannot now be determined. It is probable that these two factors are more efficient than another which may suggest itself, viz., that in the free movement of the head and eyes each length of line will require activity of certain muscles in definite proportions, and different lines will require the service of other muscles, or of the same muscles in different proportions. The change from one week to another would then be explained by the different physical condition of the observer. However this may be, I am chiefly interested in pointing out the complexity of the process, the difference between observers, and the personal variations of the same observer. Although any observer may show a surprising uniformity in his results for one sitting, at the next sitting he may differ more from his former work than he does from the work of some other observer. Unlike the deviations in the personal equation noticed in the simple time-relations of mental phenomena, these differences are not in proportion to the subjective estimate of relative ability, or of similar physical condition. A feeling of indisposition nearly always produces great change in the variable error; but often such a change, and other changes, are found when the observer feels equally able and alert.

The Tables do not show these details, and it must not be understood that there is no constancy in results from the same individual. Many persons manifest great uniformity in their two sittings. In a large number of sittings, however, even with trained observers, we may expect considerable variation in results obtained on different days accompanied by great uniformity in the series completed in a single short sitting. The Tables showing the relation of the constant errors may be studied for general tendency of the error, although the results, except in the first table, are not separate for the different individuals.

Before extending these results to the human race under all conditions we must remember that the middle of strips of paper was marked by a pinhole made with a pin in the hand of the observer. When a large number of persons mark the middle of strips in the manner described, there is on the average a slight tendency to overestimate the part on the left of the true middle for the short lines, and a greater tendency to overestimate the part on the right of the true middle for the long lines. Both these tendencies are much greater in case of the women than in case of the men. Whether any part of this error is due to the use of the hand and its interference with vision, the present data are insufficient to determine. It is proposed to test this point in the classes of the next few years by varying the method of work. There were too few left-handed persons in the classes examined to furnish even a hint of the proper solution. Those observed, however, did not contradict the general tendency, and those showing the greatest deviation from this tendency were not left-handed persons.

The following Tables furnished the corresponding data for the vertical lines. All averages falling above the true middle are indicated by the letter "O," and all averages falling below the true middle by the letter "U." The individual variations are less interesting than in the case of horizontal lines, though it is again clear that the personal element cannot be neglected in similar investigations. While it is true that nearly all observers

TABLE XVI. (Vertical Lines)

Relation of Constant Error to Length of Line, Expressed in Hundredths

Line, mm.	50		100		200		350		500		Mean		mv
Men	U	O	U	O	U	O	U	O	U	O	U	O	
1		1.30		1.45		2.43		1.97		2.91		2.01	.53
2	.30		.45		.13			.10		1.23		.09	.46
3		1.20		1.55		2.20		.34		.40		1.14	.61
4		.50		1.70		2.33		2.65		3.12		2.06	.77
5	.40			.20		.85	.07			1.91		.50	.71
6		1.20		1.20		1.83		3.31		2.77		2.06	.78
7		2.90		3.70		1.68		3.19		2.44		2.78	.58
8		.40		1.30		2.55		2.14		2.34		1.75	.72
9		1.20		2.20		2.10		2.03		1.44		1.79	.38
10		1.20		1.65		1.13		1.00		1.00		1.20	.18
11		.70		.60		1.40		.54		1.91		1.03	.50
12	.00	.00		.30	.18		1.20		.33		.28		.39
13		.20		.80		.58		.47		1.30		.67	.30
14	.10			.95		1.45		.66		.46		.68	.41
15		2.10		1.10		1.20		1.60		2.80		1.76	.55
16		.90		1.60		2.43		3.86		4.87		2.73	1.31
17		.80		.40		1.35		.93		1.90		1.08	.44
18		2.60		2.40		2.88		4.30		3.63		3.16	.64
19		1.20		1.55		2.53		2.37		1.95		1.92	.44
20		.40		.20		.80		2.27		4.57		1.65	.42
21		1.00		.75		.95		.79		1.60		1.02	.23
22	.10		.20		.83			1.07		.95		.18	.67
23		1.20		2.25		2.00		1.97		2.93		2.07	.42
24	.30			.90		2.53		1.86		3.76		1.75	.16
25		.60		.25		.53		.81		2.34		.91	.58
Average	.24	1.14	.33	1.26	.38	1.72	.64	1.75	.33	2.27	.28	1.50	.61
Mean		.82		1.13		1.46		1.56		2.17		1.43	.36
Women													
1		.90		1.75		3.28		2.41		1.88		2.04	.64
2		1.10		2.55		4.28		2.67		2.96		2.71	.73
3		1.10		.75		1.73		2.26		3.56		1.88	.82
4		.40		1.80		1.85		1.57		2.16		1.56	.46
5		.20		.65		.30	.13			.22		.25	.18
6		.70		1.75		2.55		1.94		2.21		1.83	.48
7	.10			.40		.90		2.34		3.30		1.37	1.16
8		1.50		1.85		1.58		1.63		1.42		1.60	.12
9		1.50		1.25		2.10		2.74		3.85		2.29	.81
10	.30			.70		.43	.41			2.65		.61	.85
11	.60			.40		.78	.47		.84		.15		.59
12		.50		1.85		2.98		3.54		5.02		2.78	1.28
13		.50		1.95		1.30		2.71		3.45		1.98	.88
14		.40		.60		.75		1.16		2.89		1.16	.69
15		2.20		1.05		1.48		2.03		2.95		1.94	.54
16		.40		.75		1.50		1.56		1.93		1.23	.52
17	1.20			.10		.75		2.47		.83		.59	.91
18	.50			.70		2.95		2.99		1.23		1.47	1.20
19		1.00		2.75		2.63		2.87		3.28		2.51	.60
20		1.90		.90		1.03		1.97		3.87		1.93	.79
21		.60		.85		1.35		2.67		4.18		1.93	1.20
22		.90		1.60	.33		.99			.71		.38	.83
23		1.90		.80		.90	.11			1.12		.93	.47
24	.80		.80		.15			.07	.16		.37		.35
25	.00	.00		.10		.53		1.64		1.13		.68	.56
Average	.58	.98	.80	1.16	.24	1.65	.42	2.16	.50	2.47	.26	1.55	.71
Mean		.57		1.08		1.50		1.65		2.23		1.41	.46

have a large constant error caused by overestimating the upper part of the vertical line, it is found that some individuals generally underestimate that part of the line. As far as my data may be taken for normal and sufficient, the proportion between the first and second classes is as ten to one for men, and as thirteen to one for women. The Tables given do not indicate quite this proportion on account of the grouping of the later classes, instead of considering each individual separately as I have done in making the above estimate. I have also taken into consideration the amount of the error as well as the number of errors. Indeed, if number of errors alone be considered, the relation of "O" to "U" judgments is about four to one.

These Tables show certain general facts that have not been brought out by the work of others, and other results that confirm or contradict the conclusions of certain investigators. Our present information is not sufficient to justify a quantitative treatment of the relations between judgments of horizontal and vertical lines. The individual, or personal, element is so powerful that it is at present impossible to determine to what extent the varying, and even contradictory, results of would-be-quantitative investigators depend on the method employed, and to what extent the physical or mental idiosyncrasy of the observer is responsible. Thus far investigators have arrived at results that sometimes agree and sometimes are contradictory. They employ different methods, and account therewith for the contradictions, while the agreements are taken for universally valid laws. It would not be difficult to lay down the theoretical conditions that are necessary for a satisfactory solution of this question. These Tables, however, add not a little to our information on this subject, and explain some of the contradictions existing between the conclusions of some specialists. They have one necessary element not possessed by those of any other observer. The individual was varied, perhaps, quite sufficiently. They lack any variation in method.

These Tables indicate a very strong tendency on the part of most observers to overestimate the upper half of a vertical line. This tendency exists for the shortest lines used, but increases very much more rapidly than the length of the line increases. This same effect of longer lines is seen in case of those who either underestimate the upper half, or have a small constant error. That is, the proportion of such errors to length of line either remains about the same or actually decreases. The women show an error of "U" about twice as large as that of the men. It will be remembered that Fischer devoted his attention very largely to the determination of the constant error. His results are so peculiar that a word concerning the matter seems called for. Fischer attempted to make the four arms of a cross equal to

TABLE XVII. (Vertical Lines)

Relation of Constant Error to Length of Line, Expressed in Hundredths (University Students in Psychology)

Line, mm	50		100		200		350		500		Mean		m.v.
Men	U	O	U	O	U	O	U	O	U	O	U	O	
1893													
5		.40		.92		1.50		1.17		3.13		1.42	.71
5		.60		.48		1.13		1.30		1.99		1.10	.45
5		.80		1.24		1.08		1.39		2.45		1.39	.42
5		.96		1.42		1.25		1.57		3.06		1.65	.56
4		.58		.32		.66		.78		1.50		.77	.30
1894													
5	.44			.74		1.60		1.66		1.74		1.06	.73
5	.24			.80		1.06		1.83		2.24		1.10	.71
5		1.24		.64		.67		.56		1.10		.84	.26
5	.24			.20		.17		.77		.30		.24	.24
5		.12		.30		1.37		1.78		1.38		1.09	.70
5		.44		.90		1.14		.90		1.04		.88	.18
1895													
5	.16			.26		.51		.72		.92		.45	.32
5		.80		.62		.47		1.06		.86		.76	.17
5		.36		.62		1.39		1.59		1.35		1.06	.46
5		.48		1.10		1.35		1.57		1.42		1.18	.32
5		.76		.78		1.54		.92		1.39		1.08	.31
5		.92		.56		1.23		1.89		1.68		1.26	.42
5	.52		.20		.42			.26		.53	.07		.37
5		1.08		1.54		.99		1.11		1.91		1.33	.32
4	.40		.10			.83		1.41		1.46		.64	.71
Av.	.33	.68	.15	.75	.42	1.05	.00	1.20	.00	1.60	.07	1.02	.43
Mean		.38		.66		.98		1.20		1.60		.96	.39
Women													
1893													
5		.48		.54		1.73		1.80		2.10		1.33	.66
5	.00	.00		.52		1.22		.97		1.72		.89	.50
5	.12			1.20		1.35		1.42		1.00		.97	.44
3		.18		.60		1.07		.99		1.92		.95	.45
1894													
5		.64		1.36		.99		1.82		3.00		1.56	.68
5		.16		.06		1.01		1.79		2.45		1.09	.82
5		.04		.82		.75		1.90		2.66		1.03	.08
5		1.16		1.28		1.54		1.50		1.86		1.47	.20
5		.52		.58		.69		.39		.86		.61	.13
4		.48		.78		1.24		.22		1.61		.87	.45
4		1.07		.83		1.03		.81		1.75		1.10	.26
1895													
5		1.08		.76		.93		1.75		2.26		1.36	.52
5		.80		1.56		.80		1.15		1.62		1.19	.32
5		.88		.76		1.35		.58		1.46		1.01	.32
5		.64		1.10		1.88		2.63		2.42		1.73	.69
5		.80		.44		.83		1.37		1.13		.91	.27
6		1.13		.93		1.31		1.51		1.26		1.23	.16
6		1.00		.45		.73		1.31		1.88		1.07	.42
Av.	.12	.69		.87		1.14		1.33		1.83		1.13	.45
Mean		.61		.87		1.14		1.33		1.83		1.16	.34

each other, and found that, in order to have the vertical arms seem equal to each other, the lower arm must be to the upper arm as 100 to 106.7 (by another method as 100 to 103.22). My Tables show conclusively that this proportion depends largely upon the length of the lines, and that instead of requiring the lower arm to be shorter most persons require it to be longer. The enormous size of Fischer's constant error for the short lines employed is inexplicable. In horizontal lines, Fischer finds very little constant error. I think that the constant error in comparing horizontal lines by means of both eyes may be explained by peculiarities in observer or in method. Fischer states finally that, in order that the four arms of a cross may appear to be equal, they should have the following relations: under arm 100, over arm 103.22, left arm 114.49, right arm 115.44.

The many attempts to express quantitatively the above relation seem to me to have failed, and for the very simple reason that one or both of the most important factors have been neglected. From the facts presented in the Tables of this article it is already clear that heretofore the person and the length of line have not been varied sufficiently; also that this is a problem requiring special conditions for its satisfactory solution.

CONCLUSIONS

The theoretical discussion of the facts presented in this paper, and in other papers of similar purpose, is of much interest to the speculative mind. The material is now so abundant that great self-control is required to keep within the limits of scientific conclusions. I have considered only one phase of the measurement of space by the eyes, namely the estimation of the middle of lines. All facts have worth, and while it is true that some facts have greater worth than others, yet it does not follow that the value is directly proportional to the plasticity of the facts. It is very probable that the wealth of material already accumulated on the subject of eye measurements of space relations may have important bearing on some of the greater questions of mind. But it is at least equally probable that this bearing will not be clearly understood until much adjacent territory shall have been explored. Speculations are not harmful in themselves. On the contrary, they are a necessary means of normal progress. There are, however, different kinds of speculation. The legitimate sort are of good family and are helpful when we need them. Another kind sometimes masquerades under various names for the sake of good fellowship, or from worse motives. It is this bastard speculation, bearing the name of exact science, that just now is threatening the welfare of Experimental Psychology.

Before attempting to summarise the results of this investigation some notice should be taken of their bearing upon the psychophysic law. It will be remembered that conflicting decisions have been rendered on this question, and these too from data in substantial agreement. The importance of the question, doubtless, has been exaggerated, but in its present condition worse than dissatisfaction may be caused. As already suggested, this disagreement is not due so much to variation in data, as to subjective conditions in interpretation. The lack of even an approximate standard is plainly manifest. The theoretical determination of such a standard will probably soon be attempted. Meantime several factors of discord may be recognised. It is plain that the results of more recent experiments deviate more from the psychophysic law than do the results of the earlier investigators. These latter clearly followed Weber's law with marvellous fidelity. Some of the later writers have deemed equal exactness necessary to the validity of the law. They have been unable to attain nearly the same degree of approximation; hence have declared against the law. The facts are that none of these investigators approached either Fechner or Volkmann in scientific training or in power of observation. We, therefore, should not expect equal uniformity in result. Furthermore, it is by no means certain that, by the methods employed, the constant errors have been eliminated. Again, the number of experiments made by some of the writers is entirely too small to justify independent conclusions contradicting the results of more extended researches. These considerations, taken in connection with the fact that only a very few individuals were experimented upon, and that several of these subjects were decidedly abnormal in eyesight (even though "the glasses completely corrected the deficiency"), may account for the not great deviation from the requirements of Weber's law.

Referring to Table I., we observe that the chief deviation from a uniform percentage of error is found in the shortest and longest lines of the separate series. I am unable to account for this fact, which is not new, except through the presence of a constant error. In my own investigation no attempt was made to eliminate the constant error that might possibly arise from the order of making the experiment. In the work of all other investigators this error was thought to have been eliminated by alternately beginning with the longest and shortest lines. It is evident that such procedure might possibly divide the error to some extent between the longest and shortest lines; or, if the error of the first judgment of a series could be shown to be opposite in form to the error of the latest judgment in the same series, this method might partially neutralize these errors. For such experiments as mine this hypothesis has no basis. Indeed,

AN EXPERIMENTAL STUDY OF MEMORY COLOR AND RELATED PHENOMENA[1]

By Grace Kinckle Adams

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I. INTRODUCTION

"The color in which we have most often seen an external object is indelibly impressed upon our memory"[2]; and, while this color may be aroused for various reasons, "it is awakened in a unique way if we see the external object again, or merely think we see it, for then it also determines what we see." This 'true' color of the object Hering calls the 'memory color.' "All things which are already known to us through experience, or whose color we think we know, we see through the spectacles of our memory color and therefore quite differently from the way in which we should see them without these,—provided always that we are not paying particular attention to the color." Katz,[3] while he adopts the term memory color, modifies Hering's definition. "Hering tacitly assumes that we see external things most frequently under normal illumination"[4]. Katz finds no grounds for this assumption, and, until it has been proved, prefers to define memory colors as "the surface colors in which familiar objects appear in normal illumination." He does not either agree that familiar objects always appear differently when seen through the spectacles of their memory color; for "if an object, for which one has a memory color, is seen under normal illumination, the momentary color sensation should be identical with the memory color."

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[1] From the Psychological Laboratory of Cornell University.
[2] E. Hering, *Grundzüge der Lehre vom Lichtsinn*, 1905-20, 6ff.
[3] D. Katz, *Die Erscheinungsweisen der Farben*, 1911, 214 ff.
[4] *Ibid.*, 91 ff., Katz' discussion of 'normal illumination'.

Hering noted the phenomenon and named it; he and Katz together have defined it. We wish to give this phenomenon a systematic setting. Hence we must, first, study the definitions of memory color offered by Hering and Katz, together with the examples furnished by them; and so, if possible, determine the conditions necessary for its occurrence.

The first point, upon which we find the definitions of the two investigators in agreement, is that the memory color attaches to a familiar object; and, from their methods of eliminating objectivity,[5] we may be certain that for them the significance of the term 'object' is the same. We may therefore assert with assurance that the primary and fundamental condition necessary for the appearance of the memory color is the object-consciousness. This first condition places the memory color in general perspective, and shows us that we are to seek it, not among the data of the observational attitude, but in the perceptions of our everyday, interpretative lives.

Of the identity of the two meanings of 'familiar' we can not be so sure. For Hering, the term seems to imply, as does his use of the word 'memory,' a recognition, or what we may call an individual identification, of a particular object. Katz nowhere challenges this implication; yet he follows Hering in giving examples in which the recognition is not of an individual object, but of a member of a familiar class of objects. Whether recognition, even in this broader sense, is necessary is a question which neither Hering nor Katz answers; we shall return to it later in this paper. About the familiarity itself, however, we must say one more thing, before proceeding. We naturally think of long acquaintance with an object as necessary to its becoming 'familiar'; but neither Hering nor Katz offers any reason to show why a single previous experience, provided it were sufficiently impressive, should not be adequate to the same result.

Two more claims, which Hering makes for the memory color, and which Katz, since he does not challenge them, apparently accepts, remain. The first is that the memory color is effective not only "when we see the external object again," but even "if we think we see it." Correctness of interpretation, in the commonsense significance, is plainly not a condition of the memory color. Several of Hering's instances bear out this fact. He offers none to exemplify the second claim, that we see objects through the spectacles of our memory color, only "provided we are not paying especial attention to the color,"—unless he means, by this "especial attention," not attention to the color at the expense of other characters of the perception, but an especial kind or mode of attending, which we might distinguish as the 'critical attitude.' There is support for this latter interpretation; and we may therefore tentatively accept a casual attitude toward the memory color itself as its third necessary condition.

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[5] See pp. 382 f.

In summary, we find the following to be the conditions essential for the appearance of a memory color; (1) the object-consciousness; (2) familiarity with the perceptive object, which —here we are anticipating the results of our own study— may amount to nothing more than the lack of any feeling of strangeness toward this object; (3) a casual attitude toward the memory color itself. It is clear that there is nothing inherent in these conditions which should of necessity limit the transforming image to visual quality alone; and a more careful consideration of them leads to the supposition that similar images may occur in any other sense modality. That they do occur in the case of tones and odors Stumpf[6] and Henning,[7] respectively, demonstrate. Moreover, Hering and Katz themselves give instances where there are transformations of texture and size as well as of color. For this generalized memory color, which may include any quality, as well as such perceptive characters of objects as form and texture, we suggest the term *imaginal overlay*. Our problem in regard to this overlay has been twofold: (1) we have studied the instances of it which we found occurring in everyday life; and (2), on the basis of these casual instances, we have devised experiments, whereby the overlay itself could be studied in more detail.

II. CASUAL INSTANCES

In our study of these everyday instances, we found their *means of discovery* (D) to be, in the main, three.

D (1) An object is perceived; at first neither the interpretation of this perception nor any of its experienced aspects excites *O*'s scepticism; yet some extraneous happening makes him realize that the perception has changed, or is changing, and that there has been an overlay in the experience.

(T)[8] A patch of cement flooring near the shower bath was being resurfaced. As I went into the room I saw that the workmen had relaid the rug beside the shower bath in such a way that a triangular corner of it extended over the wet patch of new cement. This corner of the rug was soaked through with wet; the pattern is a mixture of dull brown and dull red; and I very clearly saw how the wet had quickened and livened the two colors. I stepped forward—it was only two steps—to pick up the rug and throw it back, and I saw at once that the triangular patch I had been observing did not belong to the rug at all, but was a shadow cast by the angle of the base of the shower bath upon the dry cement floor; the shadow, as I now saw it, was bluish grey.

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[6] C. Stumpf, *IV. Kongress für Psychologie in Innsbruch*, 1910, 260.

[7] H. Henning, *Der Geruch*, 1916, 27 ff.

[8] The initial before every instance indicates the *O* who reported it: (T) Dr. E. B. Titchener, (Ho) Dr. L. B. Hoisington, (D) Dr. K. M. Dallenbach, (Bi) Dr. H. G. Bishop, members of the instructing staff, and (Br) Miss C. C. Braddock, (Fe) Mr. S. Feldman, (N) Mr. J. P. Nafe, (Re) Mr. A. C. Reid and (A) the writer, graduate students in the department of psychology. The unmarked instances were reported by undergraduates.

This is one of the most absolute observations of the sort that I have made. The contrast between the wet and the dry colors in the pattern was as clear as daylight. In point of fact, however, the triangle could not possibly have belonged to the rug. If you think of the shower bath as a square, then the rug is a long rectangle lying just beneath the base of the square and barely flush at its two ends with the sides of the square; the shadow lay along the right hand vertical of the square, beginning from the lower right angle. One would have thought that this displacement should prevent such an error as I fell into; but the spatial impossibility was disregarded and, as I have said, the colors in the shadow were absolutely distinct.

I had no special set for this particular occurrence, but I have often been annoyed by the children's leaving the wet rug on the floor instead of throwing it over the horse. The sight of the wet area was therefore in general entirely familiar to me.

D (2) An object is perceived; and, although the perceptive experience is adequate to the meaning of the perception, the *O*, more or less casually, challenges the experience itself, or its interpretation. This challenging leads to the disappearance of an imaginal overlay.

(T) I was looking out of the window, watching for the street-car, and I saw through the shrubs by the fence the brilliant red slats of a familiar truck; just patches of red, brilliant scarlet. As I looked, it occurred to me that what I was really seeing were dead leaves on a tree; instantly the scarlet changed to a dull chocolate brown. I could actually 'see' the change, as one sees such changes in a theatre with a shift of lighting. The scarlet seemed positively to fall off the leaves, and to leave behind it the dead brown.—I tried to recover the red by imagining the truck, and found that I could redden the leaves somewhat; then I made them leaves again, and found that I could brown them somewhat; but I could not get either the original scarlet or the later dead chocolate.

I went out to see what the color 'really' was, and found it to be a distinctly reddish brown. Hence this observation has in it *two* colors (scarlet of truck and dull brown of leaves) which emerged, as if by way of emphasis of a partial factor, from the true reddish brown of the leaves seen close.

D (3) An object is perceived; but the inadequacy of the perceptive experience for the interpretation forces the *O* to a critical challenging of the perception; and a partially adequate overlay disappears.

(T) I was passing through the back hall and my eyes fell upon a woman's umbrella standing in the umbrella stand; the handle was of dark wood, almost unshaped, and was pierced by a large loop of heavy twisted cord. This I saw as the hilt of a sword. The silk broadened out into the braided grip, and a line of light along the wooden handle became the steel of the guard. The hilt was seen, so far as I can say, with perfect distinctness for the first moment.

I was slightly set to see a sword because yesterday evening I had missed one of the family swords from its usual place and had casually wondered whether my son had carried it upstairs in the holidays.

The curious thing about this illusion is that guard and grip changed places: that is to say, just as the silk cord stood out from the shaft of the umbrella, so apparently did the grip of the sword stand off from the scabbard; and just as the light line of the handle was continuous with the shaft, so the steel guard seemed to be continuous with the scabbard. I think it was this discrepancy which called my attention to the object more critically, when the sword hilt disappeared and the umbrella was seen plainly enough.

As these instances differ in ways other than those regarding the manner of their discovery, it should be evident that a mere detailing of our incidents, under these three rubrics, while demonstrating how frequent and how intricate is the rôle of the imaginal overlay in the common perceptions, would tell us little of the systematic significance of the phenomenon. Several cross-classifications are necessary to bring apparently heterogeneous cases into relation, and to disclose the range of the overlay. The first of these classifications concerns the condition of familiarity. Every object, in the cases cited, was familiar; yet the familiarity of the truck was more immediate than the familiarity of the sword: every object was transformed; yet the transformation of the truck had to fulfill the requirements of an identification, while that of the sword merely suited the conditions of a certain occasion. Because of these distinctions we made the following groups, which we may roughly head *kind of perception* (P).

P (1) Individual identification; a particular object is identified and the transformation corresponds with this identification.

(T) I went into the half-light of the kitchen-closet for a teapot. We have two on the same shelf, of about the same form and size; one light chocolate brown, the other deep metallic blue. I wanted the brown one. But I deliberately passed it over and reached for the blue one, which I positively saw as brownish; only in the light of the kitchen did I note my mistake.—I made sure later, under the same conditions of adaptation, that the brown and the blue were easily distinguishable in the dusk of the closet. —I cannot say that I saw the brown one as blue; but I somehow saw it wrongly; and I certainly saw the blue one as brownish.

P (2) Generic identification; the object is identified as the member of a class, and the overlay permits of a more general interpretation.

P showed me a cake of dark brown soap and asked: 'Doesn't it smell good?' I smelled it, and I think it smelled like soap, scented with an unfamiliar perfume. I then noted the brown color and asked if it were dog soap. When P replied 'Yes', the perfume smell went entirely out and there was a strong odor of creosote. A little later J came into the room and picking up the soap remarked: 'This is imported sandal-wood; doesn't it smell nice?' When I smelled it this time the creosote was entirely gone and the unfamiliar perfume was there again. I am not familiar with the smell of sandal-wood.

P (3) Occasional transformation; there is no true identification, but the transformation conforms to a temporary set.

(A) I was lying in bed this morning, and as I thrust my arm back over the head of my bed I felt something round and very warm. I knew there was nothing of this sort behind my bed and wondered why I had had the experience. Then I remembered that at college there had been a steam pipe behind my bed, and that I had often been annoyed by hitting it with my arm when I first woke up in the morning. As I remembered this, the roundness went out of the experience, and I felt the hard wood of the window frame, still warm. A few minutes later I wondered how the window frame could have been so warm, so early in the morning. I felt the wood again, and it was slightly warm, but not nearly as much so as before. This was merely a mild warmth, while before it had been almost uncomfortably hot.

P (4) General transformation; there is no particular set, but the transformed object conforms to its general surroundings.

(T) I was looking from the back of the house across my own to a neighbor's garden. I saw that he had built a low wall of light-colored concrete, with an arch in it, semicircularly topped. I had the immediate idea of 'sunken garden'—I knew that he had one, though for some time I hadn't visited it. Then I suddenly had another idea—'no water there though!' As the idea came, the light concrete changed at once to dark grey, and the whole perception changed. The arch was now a round-topped break in a line of leafless bushes, and what had been concrete was a shingled wall and roof seen beyond the bushes. The structure was wet, very dark grey; I could see here and there the lines of the shingles, and I could see amazingly plainly (I say this in retrospect) the line of slate-coloured metal that capped the roof. I thought at once 'garage'. Then, immediately, the structure's lowness and smallness ousted that idea, and 'dog-kennel' came. But just as this idea was forming, I thought 'memory color?' and looked more closely. Instantly the dark grey of the building changed to light grey, and the perception was realized as a stretch of uniformly dirty snow lying on the ground beyond the broken line of bushes, stretching across to a low bank the darker line of which had given the metal of the roof.

This light grey was positively darker than the light concrete, and positively lighter than the gray of the kennel. I am absolutely certain of these changes.

Having thus, by a classification, disposed of one condition of the overlay, we attempted to treat the remaining two, the object-consciousness and the casual attitude, in a similar way. On the basis of our data, however, we were unable to make the necessary classifications. Any change in the object-consciousness, which was not a direct and complete shift to the observational attitude, turned out to be nothing more than a voluntary or involuntary exchange of meanings within and during the object-consciousness itself. A classification in terms of anything which might be likened to 'levels of object-consciousness' had to be abandoned. For the casual attitude, however, there were occasional hints which seemed to indicate 'stages of criticalness'. Yet an examination of the reports failed to reveal any measure for this criticalness; the *O*s were, apparently, unable to say just how critical was the attitude in a given situation; all that they could usually report was that it had been voluntarily or involuntarily induced, and that it had completely or only partially ousted the overlay. A classification of criticalness of attitude was, consequently, also impossible. Since, however, the *Os* could often detect more than one meaning; since they usually knew if a meaning or an attitude was voluntary or involuntary; and since they could report on the transformations resulting from the various combinations of these factors, the following tentative classification was possible (M = meaning).

M (1) Only one meaning is realized; the attitude is quite casual and the perceptive experience is adequately transformed.

(T) I came through the back hall into the front hall and saw some strange man sitting in a chair waiting for me. My heavy overcoat had been thrown over a chair, and was in fact standing erect so that a triangle of black cloth appeared over the back of the chair. There was a small table in front of the chair on which lay a pile of newspapers, and a whitish corner of newspaper made the cuff and hand of the seated figure. The chair, which was back to me, stood at right angles to a couch, on the far end of which was a yellow cushion. This cushion, seen over the black cloth, formed the hair and profile face of the visitor.

As I caught sight of the figure, I definitely saw the hand move a trifle upward, and the whole body stir as if about to rise. These apparent movements evidently corresponded with a turn of my head made with the intent of bringing the figure into clearest vision. Then, all in a moment, as soon as I focused clearly, the figure resolved into its elements.

I do not think that I was set in any special way for seeing a visitor in the hall, although I have an insurance compact pending, and until things of that sort are cleaned up I am always a little uneasy about intrusive callers. I may therefore have taken this individual for the insurance agent, although all that I can positively remember is my irritation at finding that any visitor had been allowed to get access to me on a lecture morning.

M (2) There is a realization, more or less clear, of opposed meanings; the attitude is at first uncritical, but immediately becomes critical; the perceptive experience is partially transformed.

(T) In the shadow of a large box which stood below a window I saw a ten-cent piece on the floor. As I stooped to pick it up I distinctly saw the metallic lustre of the silver; at the same moment (so far as I can tell) I knew positively that the disc was *not* of metal but of paper. I was already moving, and went on to pick the thing up; it was a paper disc from the end of a spool of cotton, white, with a border of black; within this was a ring of print in thin capitals; across the white central disc enclosed by this ring were two lines of print in similar capitals. As it happened, I had my glasses off.

M (3) Only one meaning is realized; the attitude is to some degree critical; but the perceptive experience is still partially transformed.

(A) It was late in the afternoon; the electric lights were on, but colors out-of-doors were still plainly visible. I was looking out of the window and absent-mindedly watching a train travelling north. The train soon disappeared, but I continued to look at the smoke until it had almost vanished. Then my eyes wandered casually toward the south. At the extreme right of my field of vision, I saw a dark reddish purple road, which cut deeply into the earth. I think I realized at the time that there was something bright and yellow about the color, but taking it as a road I continued to see it as purple. I also knew that I was too far away actually to see how deep it was. In spite of these two contradictions it looked like a road which absolutely belonged to the landscape; and I was willing to accept it as such. It was only after I realized that it ran diagonally across two fields, beginning abruptly in the middle of one and ending abruptly in the middle of the other, that I regarded it critically. Then the blue went out of the color and it became slightly orange (only very slightly), and I realized that the thing was much closer to me than I had supposed. It still meant 'road,' however, until I realized that it was the reflection, upon the window-pane, of an electric light in the room behind me. The color then became a very yellow orange and stood up in front of me.

M (4) A certain meaning is voluntarily put upon an experience; a casual attitude is voluntarily induced; the perceptive experience is adequately transformed.

(T) Thursday night. Coming down the gorge path, I took the bars of light across the ashes, near the top, for patches of glare ice. Only when I was actually avoiding them did I note the yellow-brown color and the uneven surface. A second lot of bars of light, lower down, failed utterly to give me the illusion.

Friday night. I looked for the upper patches of light, and got for a moment the appearance of ice,—only for a moment. The lower patches gave no hint of the illusion.

Saturday night. I walked blindfold down the path to near the lower patches; when I saw them, I had definitely, for a few seconds, the illusion of ice; but I could not hold it.—I did this in order to see if the lower patches would, under favorable circumstances, be able to produce the effect of ice.

M (5) A certain meaning is voluntarily put upon an experience; the attitude is almost casual; the perceptive experience is partially transformed.

(Bi) I was walking down State Street near the Infirmary when the sky was still light, but only about as light as the building in question, Rothschild's I think, when it was illuminated by the city lights. I cannot recall whether the sky was colored yet, or whether I had seen any colors this evening. I think, however, that it was, very probably, slightly colored at the time of my observation. My attention was attracted to what I took to be a drop in the horizon like a valley between the hills far across to the west. I saw a faint but clear red and above it, I think, a blue violet. I took the colors to be purely a perception of the sky. There were a few darker spots, very vague in outline but roughly rectangles with the long axis vertical. These spots caught my attention next. I think I changed both accommodation and convergence, and I saw then that the spots were windows. Practically simultaneously with the perception of the windows, the true yellowish white of the walls came out, and the color of my pseudo-sky had gone, as well as the sky itself. Then, just to see if I could, I let my original attitude return and the wall became colored again in a chroma about a third as good as that of the original.

M (6) There is a realization of one or more meanings; the attitude, whether voluntarily or involuntarily induced, is entirely critical; the perceptive experience is untransformed.

(A) Under an illumination from two oil lights, I took my new lavender hat from its box and showed it to a friend. She remarked "What a lovely pearl grey!" Until then the hat had seemed distinctly lavender to me; but as she said this I examined it critically and found the color to be a soft light grey, with no suggestion of color.

The previous references to partially adequate overlays warrant, as the facts themselves require, a final classification in terms of the *adequacy* of the overlay itself. The overlays, ranging from the completely adequate to the totally inadequate, fall into the following five categories (A).

A (1) Complete adequacy; the perceptive experience is so adequately transformed that the overlay can not be distinguished from untransformed sensory experience.

An historical instance of this is the following. Shortly after the discovery of potassium, Dr. George Pearson happened to enter the laboratory in the Royal Institution and upon being shown the substance, and interrogated as to its nature, he without the least hesitation exclaimed, 'Why it is metallic to be sure,' and then, balancing it on his finger, he added in the same tone of confidence, 'Bless me, how heavy it is!' "[9]

A (2) Partial adequacy in the form of a compromise; the characteristics of what would be an adequate overlay are not realized, neither are the characteristics of the untransformed experience.

(A) As I had my cup up to my mouth and was drinking, I heard someone ask: "May I have another cup of cocoa?" I immediately tasted cocoa. Then I suddenly remembered that I took coffee for breakfast regularly these mornings, and wondered how the waiter had made the mistake. I then became critical and finally found myself drinking coffee. There was an interval, however, between tasting the real cocoa and the real coffee, when the experience still meant cocoa, in which I could not have said from the bare 'taste' just what I was drinking. I got the taste of bitter and sweet, but the characteristic smell of neither of the two things.

A (3) Partial adequacy in the form of a combination; one necessary aspect of the perception is adequately transformed, another remains untransformed.

(T) As I was taking a cup of coffee from the maid's hand, I saw that the cream had been spilled from the pitcher over about a third of the tray. I said nothing, though I wondered why it had not been wiped up; and I was a little surprised to find that the cream pitcher was nearly full. I saw the smooth oily surface of the spilled cream distinctly; I noted also that the spill had run out here and there along the edge into blunt trickles. In spite of the twofold wonder I had no doubt at all of the reality of what I saw.

When I put my cup down on the tray, I saw that the pot was standing eccentrically upon an escalloped doily. This was my spilled cream and its trickling runners. The doily was of coarse white linen.

A (4) Partial adequacy in the form of a conflict; the conflict is between an overlay and the untransformed perceptive experience.

(A) From a box of assorted chocolates I took what I thought was a chocolate peppermint. When I bit into it I noticed that the color was light brown, yet I tasted it as fairly strong peppermint. A second later, however, while I was still tasting the peppermint, I began to taste caramel also. This conflict, backed up by the brown color, caused me to become critical. The peppermint flavor entirely disappeared.

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[9] J. A. Paris, *The Life of Sir Humphry Davy*, i, 1831, 268.

A (5) Inadequacy; a meaning is realized, but on account of the critical attitude the perceptive experience is not transformed.

(Ho) It was dusk, and as I was walking home my foot struck a common brown paper bag. I saw this color quite distinctly. I had kicked it along for a few feet, when I suddenly wondered if colors were really visible in that light. I then looked critically at the bag, and could not see a particle of color. It was merely a very dark grey.

These classifications are tentative, of course, but they should aid in the appreciation of the range and significance of the overlays in our everyday perceptions; the example under every rubric has been quoted in detail to illustrate the exact part played by the overlay in the particular perception. To make the illustration clearer and more general, we shall now exhibit, under rubrics representing their manner of discovery, other instances of the overlay which we have collected. Under every heading of this classification come first the examples of visual overlays, colors and perceptive characters, and then those of transformations in the other modalities. After every instance are symbols representing its place in the other classifications.

D (1) An object is perceived; at first neither the interpretation of this perception nor any of its experienced aspects excites *O*'s scepticism; yet some extraneous happening makes him realize that the perception has changed, or is changing, and that there has been an overlay in the experience.

a. The inspection of an object is interrupted, and when it is resumed the object is found to be changed.

(Re) I was walking down the street and I saw a big arc-light on the other side of the inlet. It had the orange color of an arc-light. I looked away for a while and then back at the light again. This time I realized that it was a fire, and I saw immediately that it was much redder than it had been when I took it for a light (P 4; M 1; A 1).

(Re) I was at dinner, and on the table in front of me were two silver candlesticks. I saw the polished silver quite plainly; it was very shiny and opaque, like the silver of the candlesticks we used to have at home. I looked away from the candlesticks to speak to my neighbor at the table, and when I again looked at them I saw that they were made of colorless, semi-transparent glass (P 2; M 1; A 1).

(Re) I heard the chimes play the first four bars of the Marseillaise, then an automobile came by and its noise drowned out the chimes for about ten seconds. When I heard the chimes again, a tune with which I was quite unfamiliar was being played (P 1; M 1; A 1).

(A) I was studying and heard the chimes playing 'Narcissus'. I do not know how long I heard this, but after I had been listening for a considerable time, my room-mate asked me a question. I did not hear the chimes at all while she spoke or while I was answering her. Just as I was settling back to my book, however, I noticed that 'America' was being played. America is played at this particular time every Sunday. I had recently had an argument with a friend about the similarity between 'Narcissus' and the 'Soldiers' Chorus' from *Faust*, and had, since that, listened for these two pieces to be played on the chimes, to see if I could discover wherein the similarity lay (P 1; M 1; A 1).

b. An interest in the perception causes *O* to examine it more intently.

(T) A tree had blown down in the back yard. I went out to look at it, and as I was going out of the door noticed a triangular chip out of one of the porch steps. I supposed that the falling tree had made the chip and went over and examined it. Upon examination it proved to be merely a triangular piece of sunshine lying on the step. Even when I saw this as a chip there was, I think, something pinkish about it, but because it meant chip I disregarded the pink and saw it as yellow. The sunlight itself was in point of fact pink (P 3; M 2; A 4).

(A) H often wears a medium blue sweater with a head-band which matches it exactly. She came into my room to-day and I saw the sweater and band, and they had just the same color. I was about to remark how becoming the color was, when I looked at the band rather critically and the blue went almost entirely out, leaving a grey with a slightly bluish tinge. I spoke to H about this, and she said that several other people had had the same experience, and that she thought there must be a good deal of blue in the grey. After a while she put on her coat, which was a pepper and salt mixture, and completely covered the sweater. I looked at the band then and could see no blue in it. I asked her to open her coat and let me see the sweater again. When she did this the band looked slightly blue again. The blue could be forced out by looking at the band very critically, but it persisted under more or less casual observation; it never, however, was nearly so blue as it had been the first time. The first time it had been just as colored as the sweater, but after this it was washed out, rather poorly saturated, blue. (P 1; M 1, M 3, M 6, M 5; A 1, A 2, A 5).

(A) Just before my eleven o'clock class I heard some one say that it was the fourteenth of March. I remembered that this was 'Class Day' at the college I had attended two years before, and I had a quick reference to the campus and the college buildings decorated in green, the class color. I thought no more about the fact, however, until the chimes began to ring at one o'clock. Then the idea of class day and the color green came again, but only for a second. Ten minutes later, as I was looking over my mail, I found a letter addressed to me in handwriting with which I seemed to be vaguely familiar, but which I knew belonged to no one with whom I corresponded regularly. When I opened the letter I saw that it was typewritten and began "Dear Class of 1920." I glanced rapidly over the first sheet and noted something about plans for a reunion, and then turned over to the second sheet to find whom it was from. At the bottom of this second sheet, I saw the signature of our alumnae secretary in the very same green of our class color. I thought "Isn't that typical of the girl, to use green ink to prove that she has a lot of class spirit? I believe the envelope was written in green too." I found the envelope again and, when I first looked at it, its writing had just the same color as the signature; but, as I continued to look at it, it seemed to become blue and then green again, so that I was really unable to tell its true color. To be certain I asked some one, and when she replied "O, most certainly blue," the green went out completely and staid out. I looked at the signature on the letter once more and it also was quite blue (P 3; M 1, M 2, M 6; A 1, A 4, A 5).

For several days I had been passing, at the same time every day, an iron fence about a hundred feet long, which had its preliminary coat of orange paint. On seeing the orange color, the fourth or fifth day, I muttered something to the effect that the color was a blot on the landscape, and almost simultaneously I realized that the fence had been painted black (P 1; M 1, M 6; A 1, A 5).

(B) On a couch across the room I saw a pillow cover with an Indian head on it. The face was seen in profile and there were lots of feathers sticking up above it. These feathers were red on a white background; the face itself was pink. I was interested in the pillow and walked toward the couch to examine it more closely. As I got nearer I saw that it was a Cornell cushion. The pink face was the yellow and white seal, the white background the letters C O R N E L L, and the feathers the red of which the cushion was made (P 2; M 1, M 6; A 1, A 5).

(T) My coat was hanging upon a nail. Across one sleeve I saw a wet streak which seemed to have been made by leaning against a wet shelf. I could see that the colors had been livened and quickened by the water and that the streak was still wet. Wondering how it came there I looked at it more carefully, and found the streak to be only a shadow lying on the coat, and the colors in the shadow to be darker but duller than the rest of the coat (P 4; M 1, M 6; A 1, A 5).

(Re) I looked up from my book and saw a rose lying in the middle of my desk. In a more or less casual way I had been looking at it for several minutes, and I had seen the pink color and the soft texture of the petals quite distinctly. Then I suddenly wondered where it had come from and looked at it more carefully. Immediately the color began to change and become faded, and I realized that it was an artificial rose. Then quite gradually the texture began getting clothy. I could see it getting more and more like cloth; some parts became cloth sooner than others. Finally the whole thing was cloth (P 2; M 1, M 3, M 2; A 1, A 2, A 3).

(Bi) Up in the workshop Dr. D had been making a cast for observations to be made with the Titchener quadruple pendulum. I knew what had been going on, but had forgotten about the pendulum and the cast for the moment. I turned toward the window above the work-bench. The apparatus was on the bench about directly in front of the window. As my eyes passed to such a position that the plaster block fell just off the fovea, I saw the top of the block of plaster just like aluminum which is unpolished but still smooth and bright. I think the sides of the block which were under poorer illumination did not become metallic. I am sure I had the meaning of aluminum, and there was a half-formed query which completed would have been, "why, how did he get the aluminum?" The perception vanished under scrutiny, but while it persisted it had the sheen and fine grey flecks of metallic lustre. It did not return; but I tried to give it a chance to return. I think the plaster stood out, after the metallic look had gone, as it should have, very white (P 4; M 1, M 6; A 1 or A 3, A 5).

(T) As I walked forward through the hall toward the front door, I saw lying on the floor to the right, as if the parcels-post man had just thrust it in, a large and rather battered looking package. It was evidently a square cardboard box which had suffered in the mail; it was wrapped in heavy brown paper, and I clearly saw some of the folds and crumpled edges of the wrapping as well as the lines of the string with which it had been tied. I can't positively say if I saw a cross of string with a knot in the middle of the cross, but that is definitely my impression; anyhow I saw lines of string very plainly. I swerved to the right to pick the thing up and found that it was simply the end of a rug projecting into the hall from the drawing-room. The rug was a particolored oriental affair, with a silky nap, and the light falling on it from the drawing-room windows had turned it into a sort of general pinkishness,—not at all the color of the lightish brown wrapping paper, but still near enough to that to show me how the illusion could have arisen. I tried to reproduce the illusory experience, by going back into the hall again, but could get only partial hints of it,—nothing at all as positive and definite as the original perception.

I was expecting an express package rather eagerly, though I had at the time no particular expectation of a parcels-post package. The set for the express may, however, have been enough to touch off the illusion (P 3; M 1, M 5. M 6; A 1, A 2, A 3, A 5).

I walked into my study and saw a small animal with brown fur rolling across the floor. I wondered how in the world it could have got there and went over and examined it to see what kind of an animal it was. I found it was no animal at all but my metal trash basket. The trash basket is yellow, but its color was not visible in the darkness of the room (P 2 or 3; M 1, M 6; A 1, A 5).

(Bi) I was sitting in Prof. T's Goldwin Smith office facing the window with F off to my right, well off at the edge of the visual field. I happened to move facing him just enough to bring him into view vaguely. I think that the light tint of his sock caught my attention first. As the perception formed, and it formed very swiftly, the sock became silken with the fine pattern of light and dark grey flecks. It had, as my eye turned more squarely toward it, all the usual character of a silk stocking. There was a definite meaning of stocking, I think, due to the length of sock exposed; about two thirds of the distance to his knee. Upon examination this sock became the usual dull cotton garment with no sheen upon it and no transparency at all. I turned my eyes away again as nearly as I could to the old position, but the silky perception did not return (P 3; M 1, M 6; A 1, A 5).

(Bi) I was working in the shop and happened to glance in the direction of the sink, and I noticed that the soap in the rack above the sink was wet. I saw the slick moist surface of the soap quite plainly. I was curious, as I knew that no one had used the soap that hour, so I looked at it more carefully. As I looked the wet stripped right off and the soap became dry. I was able, however, by imagining first wet and then dry soap, to make the slickness come and go (P 3; M 1, M 4, M 6; A 1, A 5).

(T) A dust-cloth was lying in the back hall, where there is a prismatic effect that I have often noticed and even produced by laying objects there. As I passed through, I saw the cloth as a towel of unbleached linen with a variegated border. I saw distinctly that the fabric of the border was of different material from that of the body of the towel. As the thing was unfamiliar to me, I looked again, and now saw the grey dust-cloth with the light on it. It lay in folds and the light spread across the upper fold (P 4; M 1, M 6; A 1, A 5).

(T) A curled leaf on the corner of my dressing-table was taken for a flat splinter of mohogany chipped from some bit of furniture. I am not unaccustomed to finding such splinters; the leaf was a novelty. The color was approximately right; but I saw the lustre of the polished wood, whereas the leaf was rough-surfaced (P 4; M 1; A 1).

I was walking up the street and saw P coming toward me on crutches. I saw her very plainly, and noted her slim figure and athletic build. I was surprised, however, to see her on crutches, for I didn't know she had had an accident. For this reason I continued to look at her, and I suddenly realized that it was not P. Immediately she became much fatter and her whole physique changed. I was amazed that I could have ever thought she was P.—Several days later I was walking along the street and had exactly the same experience (P 1; M 1, M 6; A 1, A 5).

(A) I was dressing on a Saturday morning and I heard the chimes playing one of the familiar foot-ball songs. I knew there was to be a game that afternoon, but I wondered why that particular song was being played in the morning rather that at noon as it usually was. This caused me to listen more carefully, and I discovered to my surprise that the piece was not "From Rocky Heights" but "Marching through Georgia." The rhythm and the grouping of the notes in the two pieces are similar (P 3; M 1, M 6; A 1, A 5).

(A) L plays the victrola every morning. To-day I heard it playing in the next room. I could hear the syncopated rhythm and could hear the tones go up and down, but I could not recognize the tune. I listened intently for several seconds before I realized that it was not the victrola but the carpet sweeper in the next room. The notes of the victrola were more distinct and I think higher than the buzz of the sweeper (P 3; M 1, M 3, M 6; A 1, A 4, A 5).

c. The *O* acts upon the object; and the object changes.

(A) Two months ago I bought some purple handkerchiefs to give to my *Os*. I had been very careful about getting a certain quality of purple, and had, on that account, become very familiar with that particular color. To-night I saw one of these handkerchiefs lying on my dressing-table. It was soiled and I saw the purple color most distinctly. I wondered how it got there; but I never for a moment doubted the reality of the color; although, if I had thought, I might have known that the purple wouldn't be so good under the yellow light. Still, when I saw that it was soiled, I reached for it to put it in my handkerchief bag. Just as my hand reached the handkerchief, the purple faded completely out and left behind a decidedly yellowish brown. The handkerchief was a yellowish brown one that had been given me a few weeks before and which I had not used very much (P 2; M 1, M 6; A 1, A 5).

(T) I had my 'near' glasses on when I came into the room. I had been wrapping up Christmas packages and I saw the end of a piece of twine lying on the desk. I saw the separate strands forming the twine, the shiny surface of the string, and the cut end. The details were absolutely complete. When I picked it up, it was a dull gray elastic band (P 3; M 1, M 6; A 1, A 5).

(N) A little while before I had found a dime on the floor of the lecture room. When I walked into my sitting-room a little while ago I saw another dime. It lay on the floor just opposite the window. I went over to pick it up, and found that it was only a mother-of-pearl button with two holes in it. I went back to where I had first seen it, and by persuading myself that the button was a dime could make it look like a dime again. I could see the silvery lustre, and the two holes in the button became the face of the coin (P 3; M 1, M 4; A 1).

(A) My hands were damp, and I reached for the towel I saw lying over the back of a chair, to dry them. The towel was a plain white Turkish one with a coarse weave. It lay crumpled up so that I didn't see its shape. My hand had almost reached it, when the coarse weave turned to wooliness and tiny pink stripes came into the white. I realized that I was picking up the coat to my flannelette pajamas. (P 2; M 1, M 6; A 1, A 5).

(Br) As I was coming down the stairs I saw a lot of white palings leaning against the wall opposite the railing. They stood out from the wall, they were a very good white, and the edges were sharply defined. I put my hand out to touch them and as I did so I realized that the edges were not sharply defined. Then the meaning of 'shadow' came, and I saw that the shadow of the railing itself had given the effect of palings. Immediately what had seemed to stand out from the wall became flat and the white became yellowish (P 4; M 1, M 2, M 6; A 1, A 3, A 5).

(Br) I was looking for letters in the post-box and I saw a brown paper package in the box. I could see the box standing out from the wall and there was a light wooly string around it. As I reached for it I realized that there was really no package there. Immediately the brown thing went back against the wall, and I saw the grains in the wood of which the mail-box was made. The string got darker and darker until it became a black shadow (P 3; M 1, M 6; A 1, A 5).

(A) I was coming home late in the afternoon; it was almost dark and the light on the front porch had been lit. I turned in at the gate and walked toward the front steps. As I reached the bottom step I saw a box wrapped in white paper lying just at the front door, partly in the light, partly in the shadow. I saw the whole of one of the long sides of this box,—the top of it receding back from me into the shade,—and a tiny bit of one of the smaller sides. The paper was very white and I saw its smooth texture, but I cannot definitely say that I saw any string. I would have picked the thing up, but when I reached the top step I found that there was no box there, but merely the rounded base of one of the posts which stood on either side of the door. This was made of wood and was a fairly dark grey.

I was not expecting a package; but when at home I am not unaccustomed to finding packages by the door at this time in the afternoon. I think the time of day was probably enough to touch off the illusion (P 3; M 1; A 1).

(A) I was doing my hair before the mirror and I saw a streak of powder on my chin. It was white powder and much lighter than my skin. The rough texture of it stood out noticeably. I started to rub it off, but I found that it wasn't powder but a small beam of light lying on my face. It lost its rough character immediately and became nearer the real color of my face; but I was able to see this small portion of my chin as different from the rest, as if a small piece of another skin, whiter and finer in texture, had been grafted perfectly into my face.

The critical attitude evidently prevented a second overlay by which the texture and color of this portion would have seemed the same as the rest of my chin (P 3; M 1; A 1; P 2; M 6; A 5).

I was knitting a sweater on half of a hair-pin off which some of the enamel had been rubbed, so that the glint of the metal could be seen. I dropped the hair-pin on the rug and stooped to pick it up. I saw it plainly; the glint of the metal stood out against the dark of the rug; but to my surprise what I tried to pick up was not the hair-pin but a thread in the rug itself. This thread was light in tint but was composed of several different colors (P 1; M 1; A 1).

(Re) While working in the laboratory yesterday morning I got some calcium on my clothes, and all day I was dusting little flecks of it off my coat. Late this afternoon I saw one of these white spots. I didn't doubt it for an instant, but started to flick it off. Immediately—almost in a flash—the color changed to yellow and I saw that I was looking at the sun shining through a button-hole (P 2; M 1, M 6; A 1, A 5).

(A) I reached for the sugar bowl and started to take out a lump of sugar. The particular lump I wanted looked just like all the rest; the reflection from the gold lining of the bowl made them all look slightly yellow, but this one had the same rough texture as the others and I saw it stand out from the sides of the bowl. Just as I was about to touch it, my chosen lump disappeared and I found myself trying to pluck out a piece from the side of the bowl itself. The shape was given this pseudo-lump by the position of the real lumps (P 2; M 1; A 1).

(Re) I was manipulating the motor in an experiment. When not actually working I had been absently sticking a nail into some old nail holes in the table upon which the motor stood. The holes were round and even and of course darker than the yellow wood of the rest of the table. I watched the nail as it went into each hole, but suddenly it reached a hole where it could not go in. I then looked at this place more carefully and it was nothing but an ink blot. Instantly the dull dark of the hole changed to the shine of the ink and the regular outline became uneven (P 2; M 1, M 6; A 1, A 5).

I saw a drop of water on the table-cloth and showed it to the persons on both sides of me, telling them to watch it crawl up my place-card. I put the card under the drop and the drop began to move. Then I pulled the card away from the table and I could feel the strain in my arm as I drew it up. I did this several times and all the while I saw the glistening surface of the water. When, however I put my hand down on the table, it was dry, and I found that my drop had been merely a reflection from a drop of water clinging to the inside of my glass. It was simply a darker spot on the table-cloth and had nothing watery about it. By putting the card on top of the shadow I could make the shadow appear to climb up the card, but it did not look watery again; neither could I get the pull in my arm (P 2; M 1, M 6, M 5; A 1, A 3, A 5).

D (2) An object is perceived; and although the perceptive experience is adequate to the meaning of the perception, the *O*, more or less casually, challenges the experience itself, or its interpretation. This challenging leads to the disappearance of an imaginal overlay.

a. There is a disagreement among two or more people about the perceived object.

(T) I saw a strange pocket-handkerchief, with broad band and spots of brown, lying on the table under the lamp. I said to my wife: That's not ours—one of the girls must have left it. She said: Why, that's my lavender handkerchief! I asked her if she really saw it as lavender, and she said: Certainly,—come here and see the light showing through; you'll see it's lavender. I still saw it as brown; and my wife said presently that it now looked brown or brownish to her. I could not get her to go behind her 'knowledge' that it was lavender; but so far as I could tell in her critical seeing of it it was brown (P 1; M 1, M 6; A 1, A 5).

(D) We were having what I thought was 'riced' potatoes for dinner. I looked at the creamy yellow color of the potatoes and said to myself, 'I wonder why my wife didn't have rice!' As I don't usually eat potatoes I did not take any, and as my wife saw me refuse she asked, 'Aren't you going to have any rice?' As she said this the yellow of the potatoes faded into the white of the rice. I tried to get the yellow back again by imagining the potatoes, but was unable to get any change in the color (P 4; M 1, M 6; A 1, A 5).

(A) Until I was eleven years old my hair was of a medium shade of brown. When I was eleven, however, I had an illness and lost most of my hair. The new hair which came in was quite dark. I noticed this and my friends and family often remarked about the change. Still even now—ten years later—when I see my hair in the mirror casually I see it as light, and am startled when any one likens it to hair which I see as much darker. When I look at it critically, however, it is always dark (P 1; M 1, M 6; A 1; A 5).[10]

(A) At the place where I was visiting the family was much excited because Aunt F—aged seventy-five—had got a very youthful lavender dress. The dress, which was made of a lavender and white checked material, was shown to E and me as soon as we arrived, and then put away again. Several nights later, E and I were in bed and the door of the wardrobe was open so that we saw part of the skirt of a dress sticking out of the wardrobe. E asked C—whom E and I were visiting—whose was the brown dress. C answered 'Why that isn't brown, its Cousin F's lavender dress!' I then looked at the dress, and finding it to be of a very good brown informed C of the fact. She insisted that E and I were both crazy and that she still saw it as lavender. She opened the door so that we could see the whole dress, with the light on it in the same way as it had been shining on the exposed part before. As soon as she did this I too saw the dress in the same lavender in which I had seen it in the daylight. I could, however, by becoming critical, force the lavender out and the brown in, and again, by thinking 'that is Aunt F's lavender dress' make some of the lavender return (P 1; M 1, M 5, M 6; A 1, A 2, A 6).

(A) At the supper table I was using a handkerchief which had originally had a lavender border, but which was now so faded that it appeared quite grey under the lamp light. I asked D what color it was and he replied "Grey, of course." L, overhearing him say this, interrupted with "Of course it isn't grey. I have one just like it and it's lavender." I asked her if she really saw it as lavender and she assured me that she did, and was unable to see it any other way (P 1; M 6; A 5; P 2; M 1; A 1).

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[10] T and Professor H. P. Weld report that, when looking casually into a mirror, they see their hair, which is grey, as light brown and dark brown respectively. Under like conditions another *O*, who has been bald for six years, sees his head thickly covered with hair.

(A) It was 'Spring Day' and about time for the finish of the races. A friend was reading aloud to me when suddenly I heard a loud shout. I interrupted her to remark that we had probably won the race. She wanted to know why and I asked her if she didn't hear the cheering. "What cheering? All I hear is the water running in that pipe over there." The sound then changed for me, it became harder and more metallic and also more intermittent. As I listened, I could by thinking first of the cheering and then of the pipe make the sound change. It did not, however, get quite so much like a cheer again (P 3; M 1, M 5, M 6; A 1, A 2, A 5).

(N) I was coming up the hill with a friend, and as we approached Cascadilla, where we had intended to eat breakfast, we commented on being too late for their breakfast hour by a wide margin. As we came up beside the dining-room I smelled coffee very strongly and said "We are just in time for the coffee grounds." My friend laughed and replied, "You smell burned gasoline from that truck"—a truck pulling up to the side of the building. I couldn't get the smell of coffee again but am not sure that I ever smelled the gasoline, as a fresh wind came up just as he spoke (P 3; M 1; A 1).

(A) One of the girls in the house has an alligator. The smell from some meat which had been left in his pan had been very objectionable when I left the house this morning. When I opened the front door at noon, I smelled the odor strongly and wondered why it had not been attended to. I paid little attention to it, however, as I was rather intent upon getting my mail. The letter I was looking for was there and I was reading it while walking upstairs. When I got to the top of the stairs, I saw one of the girls and asked her why no one had cleaned the alligator's pan. "Why, the alligator's gone," she replied: "we moved him to the cellar." To be certain, I went into the room where the alligator had been and found no trace of the odor. To check the thing up I went back down stairs. As soon as I got about half way down the steps, I smelled a fairly strong odor of scorched food. Not only was the putrid quality gone from the experience but the odor was now more voluminous and less oppressive than it had been (P 3; M 1; A 1).

(N) In the morning we had been making plans for Thanksgiving and among other things we mentioned a turkey. That evening we went out for dinner. About an hour after dinner we were visiting some friends and M began to tell about the delicious turkey dinner she had just eaten. When asked if the fowl wasn't duck, she said no, she was certain that it was turkey. She was positive that she had tasted the turkey flavor and was almost as sure that she had seen the white meat. She would not be convinced that it was not turkey until her host assured her that it was really duck (P 3; M 1; A 1).

(A) This noon we had parsnips for dinner. They were sliced and fried in long thin pieces and had about the same color as a yellow sweet potato. W thought they were sweet potatoes and tasted them as such. She ate several and would not be convinced of her mistake until she asked the steward. After this she reported that they tasted differently and that she did not like them. She seldom eats parsnips because she dislikes them, but she is very fond of sweet potatoes (P 2; M 1, M 6; A 1, A 5).

I was eating cake with chocolate sauce. I had already swallowed one mouthful when some one said something about blackberry sauce. Immediately what I had in my mouth tasted like blackberry. I looked down at my plate and what had been dark brown a little while before was now dark purplish red. I can't say that I positively tasted chocolate in that first mouthful, but I am certain that I tasted nothing like the later blackberry. We have cake with chocolate sauce about once a week and the blackberry was an innovation (P 2, M 1, M 6; A 1, A 5).

As I was passing through the kitchen I noticed a plate of rather poorly hardened fudge on the table. I went over and scooped a little up on my finger and stuck it in my mouth. It tasted like fudge which had too much salt in it. Because I didn't like the salt I didn't eat any more, but went on out of the kitchen. At breakfast the next morning my hostess asked me to have some anchovy paste and I recognized the dish of fudge. The salt was still there in the paste but there was a decidedly fishy flavor and no suggestion of chocolate. There had been no hint of fish in the taste of the fudge, although the taste of salt had been the most prominent thing in the experience (P 2; M 1; A 1).

b. The *O*, of his own accord, doubts a perception.

(T) Curiously enough, I was again tricked by the blue teapot only six days after my first experience. I went into the closet for the brown teapot, saw it on the shelf, and began to reach up for it. I had hardly begun to move when the memory of the former mistake flashed across my mind, and I clearly saw blue. There was no noticeable disjunction of time between the two looks,—I was still looking when the memory 'flashed,' and I kept on looking; the flash was momentary. All the same I saw brown before I saw blue. The brown teapot is a family possession of long standing, and I am therefore very familiar with it (P 1; M 1, M 6; A 1, A 5).

(A) Slightly less than a month after my first experience with the green ink, I found another letter from the alumnae secretary in my mail. The green of the address, as far as I can say, was precisely as it had been the first time. I cannot say that I first recognized the writing and then saw the green; the two things seemed to come at the same time. I merely saw a letter from the secretary addressed in green ink. I immediately became sceptical, remembering the earlier experience; so examined the color critically. It changed at once to a blue, which was darker than the green, and also darker than the blue of the former letter, with which I later compared it. Again I was able to get the green to return somewhat by thinking of the other experience and keeping my attitude fairly casual (P 3; M 1, M 5, M 6; A 1, A 2, A 5).

(A) For several weeks there had been a bluish green cloth on a screen in my room. Yesterday I removed the cloth but left the neutral grey screen in the same place. To-day when I came into the room I saw the blue on the screen. In a flash I remembered that the blue cloth was gone. The blue color was gone also. I am certain that the chroma of the blue to-day was poorer than it had been before, but that I neglected this fact until I realized that I was looking at grey cardboard and not blue cloth. I cannot say exactly what was the texture of this blue, but I have a definite impression that it was clothy. A few hours later the experience was repeated. And then I found that I could get some of the original blue back by 'imagining' the cloth hard enough (P 1; M 2, M 6, M 5; A 2, A 5).

(A) For three consecutive evenings I have had this happen. I have come home from the campus when it was too late for colors to be seen, but when the outlines of familiar objects were perceptible. As I turn a certain corner, I recognize a grove of pine trees, and I see the green of these trees. It is a fairly good green, but darker than that which these same trees have when I see them in the morning. Then I realize how dark it is and become critical toward these trees, and I find them to be a very dark grey with no trace or hint of green (P 1; M 1, M 6; A 2, A 5).

(A) I looked from the laboratory window and saw a great pile of dirt beside an unfinished building. I noted the deep red color of the earth, and had been looking at it for several minutes when I realized that earth of that color was not common in this part of the country but was what I had been used to as a child. I then became critical, and I saw the dirt as a rather poor violet with very little of the original red. I repeated this observation several times with a more or less critical attitude (I say 'more or less' because I was unable to keep it entirely casual as I wished). At first I was able to recapture a little of the red, but finally I was unable to get any of it (P 2; M 1, M 6, M 5; A 1, A 5, A 2).

(Bi) I started to turn on the electric light but decided that I didn't need it. Still as my hand reached the bulb I saw the whole side of the room at which I was looking as if it were flooded with light (P 3; M 1; A 1).

(T) I looked out of the front window on getting up, and saw the pavement as dry; I was curious as there was to be a football game. It was not raining at the moment, but it had been raining heavily, and everything in fact was wet; but I overlooked the wet road. I then looked out of a side window, and saw that the roof of the porch was very wet. So I looked again out of the front window and saw the pavement as wet—and as very much darker than I had seen it the first time (P 2; M 1, M 6; A 1, A 5).

(Bi) Yesterday I cut my finger and it bled considerably. This morning there was a small dark reddish brown scab where the cut had been. Once when I looked down at this scab I saw it as red and shiny blood. As I continued to look at it, it suddenly changed to the dull, dark brown scab. As I looked it continually changed from scab to blood and back again (P 3; M 1, M 6, M 4; A 1, A 5, A 1).

(Bi) I looked out of the window of Morrill to see if I saw any signs of the snow thawing, and was surprised to see parts of the walk beside the building as wet and parts of it as quite dry. When I had looked at this walk a little while before it had been entirely covered with snow, and I did not believe that all the snow could have melted in such a little while. So I looked carefully and the wet and dry spots of the walk became dirty and clean patches of snow (P 3; M 1, M 6; A 1, A 5).

(Re) When I took my seat at the concert last night the conductor had not appeared and only a few members of the orchestra had taken their seats upon the stage. I could see a mellow yellow brown showing through the backs of the empty chairs. Just then the conductor came forward and I wondered "How is it that he is coming out before all of his men?" As I was asking myself this, the mellow brown as seen through the chairs turned to a light cream, and I realized that what I had taken for empty folding chairs were music racks with sheets of white music upon them. Not only did the color change but what had seemed filmy acquired the slick texture of creamy paper (P 3; M 1, M 2, M 6; A 1, A 3, A 5).

(Bi) I was at the seminary at Dr. T's, and at some moment of lapse of attention from the subject of the discussion I heard a complex sound like a full harmony of an orchestra or band. This perception ran a short course unchanged in quality. When I became critical, the complexity was rapidly reduced but the experience was still a chord of several notes. Very soon, because the intensity increased rapidly, I discovered that what I was hearing was a trolley car. Even after I knew it was a car, the tonal character of the sound continued until the car was opposite the house, when at this full intensity all harmony passed and I was left with only a rumble and the slight singing quality of the motor. The seminary was being held in the room in which Dr. T's orchestra plays on Sunday evenings (P 3; M 1, M 3, M 6; A 1, A 2, A 5).

(A) I usually come home to lunch while the chimes are playing. When I reach the gorge path the sound of the water coming over the falls is so strong that I can hear above it only the accented notes of the piece being played on the chimes. If a new melody is started while I am at this place I cannot of course recognize it. To-day I came home from the laboratory at the usual time but as it was Sunday there were no chimes. Still, when I reached the place where the path is nearest the water-fall, I heard the accented notes of a tune written in two-four rhythm. I did not recognize the melody but I was sure of a change in pitch. Suddenly I remembered that it was Sunday and listened more closely. I heard only the rush of the water (P 3; M 1, M 6; A 1, A 5).

(Bi) I was walking down the corridor and I saw ten feet ahead of me a woman's glove hanging on the bulletin board. As I saw it, there was a positive odor of kid leather and a faint perfume—a definitely flowery odor. There was just a flash of these odors; for, as I wondered how I could smell the glove so far off, they both disappeared. I went up and sniffed at the glove; the scent of the leather was similar to what I had smelled before, but the perfume was quite different. The odors smelled close were no stronger than those that I had smelled ten feet away (P 2; M 1, M 6; A 1, A 5).

From a box of chocolates which my friend offered me, I took a large plain cream. When I had eaten about half of this piece, I decided I would rather have a peppermint if I could find one. I did find several peppermints and ate them. About five minutes later I saw the half eaten cream lying on the table and put it in my mouth. It tasted precisely as the peppermints had done. It wasn't until I had swallowed it that I realized that it was the other half of the plain cream (P 2; M 1; A 1).

(A) When taking a cold bath on an early winter morning, I notice that the room feels much warmer, if I turn on the electric light before getting into the tub. To make sure that the light did not actually raise the temperature of the room, for one week I measured the temperature before and after my bath with the light off, and for one week with the light on. There was no difference in temperature under the two conditions, but the difference in the feel of my body was amazing. When the light was off, my body felt chilled and I dreaded getting into the cold water; when the light was on my body felt quite comfortable and I anticipated with pleasure the cold plunge (P 3; M 1; A 1).

For many years I had to wear a brace on my foot. When the brace became dry it squeaked and I oiled it. To-day I was walking along with a girl who was dragging along an umbrella, so that it made a squeaky noise. As soon as I heard this, I thought—I must oil my brace. Then I felt very clearly the stiffness in my leg (P 3; M 1; A 1).

c. Another meaning for the experience comes.

I was sitting in a darkened room. A boy was standing in the lighted doorway so that I saw his figure in silhouette. I recognized him as Ba and I saw the yellow of Ba's hair. Then somehow I decided I had made a mistake, and that it was Bl that I was seeing. I then saw the yellow fade very quickly into black. But just as it had faded I thought 'perhaps it is Ba after all', and the yellow came back again. It was indeed Ba, but when I looked carefully I found that the yellow of his hair was not visible because of the light being behind him (P 1; M 1, M 6; A 1, A 5).

(T) On a very cold afternoon I went out on the kitchen porch to see if the door of the storm-house was firmly closed. The door faced directly the setting sun. As I pulled the door toward me, I pressed down the trigger of the latch, and so opened the slot in the door within which the trigger works. A beam of red sunlight flashed through the slot and crossed my thumb. I snatched my hand away, thinking I was to be burned by an electric spark; I had at the moment a swift reference to the children's scuffling on a wool rug, and then touching off 'snaps' from one another! Almost instantly I realized that the light was sunlight; but it was so brilliant, in the half obscurity of the storm-house, that I could almost if not quite recapture the illusion of the spark as I played with the latch (P 2; M 1, M 6, M 5; A 1, A 5, A 2).

(Re) I was walking down the street at night and I saw rough textured white frost by the side of the walk. As it was a cold night I did not doubt the frost, but suddenly I had the idea that I was looking at the large leaves of some lily plants which grew beside this walk. Then as I looked more closely the rough texture of the frost became slick and the leaves, although I could see no actual color in them, were darker in tint than the frost had been (P 3; C 1; A 1).

(A) I had my eyes shut while observing in an experiment in audition. When I opened them I saw a silvery lustre. At first this meant nothing to me and I asked myself 'What is it?' I recognized my black tie. The silvery lustre disappeared and I saw the ribbon as black with sunshine on it. By becoming casual I was able to see the black as grey again, but never as light or as filmy as I had seen it before I had the meaning of 'my tie'(P 1; M 1, M 3; A 1, A 2).

(Br) I heard the chimes play six notes of "Auld Lang Syne." Then I realized that an unfamiliar tune was being played. This tune was twice as slow as "Auld Lang Syne", so I had filled in three of the notes and heard the other three differently. The imagined notes were, I think, though I am not at all certain, duller and more thud-like than the others (P 1; M 2, M 6; A 2, A 5).

(A) Last night as I went from Morrill to White I saw Sibley ablaze with light. I knew that there was a lecture there. To-night I was walking from White to Morrill, facing Boardman, and when I got as close to Boardman as I had been to Sibley on the previous evening, I saw lights in all the windows of Boardman. As well as I can say in retrospect, at the moment I saw the lights I also heard the sound of applause coming from Boardman, and I thought, "The lawyers must be having a lecture!" I heard distinctly the intermittent sounds of clapping hands. Just then my attention was attracted by a large light beside Boardman and I saw that it was the headlight of a large truck or automobile. Then I knew that the sounds I had heard did not come from the building but from the truck. By this time the truck was at the top of the hill which it had been climbing and so the sound had possibly changed, but I am almost sure that the applauses were higher in pitch, lighter and more intermittent than any rumble of the truck could have been. They were certainly quite different from the sound of the moving truck as I now heard it (P 3; M 1; A 1).

(Fe) As I was passing by Cascadilla gorge I smelled a skunk. I glanced around, looking for the skunk, and I noticed an automobile ahead, going down the road. Immediately the odor changed to one of gasoline. I stopped for a while until the automobile was out of sight, and tried by sniffing to make sure that there was no skunk around. The gasoline odor soon disappeared, but the odor of skunk did not come back. Skunks are quite common around this gorge (P 3; M 1, M 6; A 1, A 5).

There were two boxes of wafers in a package my mother sent me. We opened one of the boxes last night and ate all of its wafers. They had a peppermint filling which was fairly strong. This morning I opened the second box and ate a few of the wafers. They had the same peppermint flavor as those of the night before. For some reason, however, I happened to look at the filling of my fourth wafer and found it to be brown, instead of white, as it should have been to be peppermint. There was no trace of peppermint in the wafers which I ate after this (P 2; M 1, M 6; A 1, A 5).

(A) I notice that at night when I first put my foot down in the bed I often feel the wooliness of the blanket. Then I ask myself how can it be the blanket as I am sure the bed is well made. Then I feel the harder, cooler, slicker, cotton sheet. Then if I wonder if perhaps the sheet has not come untucked and my foot is actually touching the blanket, I feel the warm wooliness again. Often I have to feel down in the bed with my hand before I can persuade myself that the sheet is well tucked in (P 2; M 1, M 4; A 1, A 1).

D (3) An object is perceived; but the inadequacy of the perceptive experience for the interpretation forces the *O* to a critical challenging of the perception; a partially adequate overlay disappears.

(T) I was looking from the back of the house across to the Knoll, and saw a small car going up the sloping road; it was a small closed car like an electric brougham. Behind it at a short distance came another odd-shaped car with some erection upon it; I took it for the moment to be one of the students' cut-down Fords. I thought nothing of the observation and was looking away when it struck me that the two cars were moving at an absolutely even rate, almost as if they were mechanically joined. I then saw that the two of them together were in fact one immense truck. The driver's house had given me my electric brougham; then came an immense stretch of yellow canvas which I had simply identified with the grey sky behind; and my Ford with something standing up on it was the dark oval at the back of the truck, which was not closed up, so that I could see a little distance through the thing under the hood of canvas. The decided way in which the two original cars snapped together when once I realized that they belonged to the same truck was excessively comic; I found myself laughing aloud though there was nobody present to whom I could explain the joke. The sky was sheer grey, the grey of fairly uniform rain-clouds; the canvas of the truck was a fairly chromatic light yellow (P 4; M 2, M 6; A 3, A 5).

(A) I was walking down the street and saw the smoke coming out of the chimney of one of the near-by houses. There was a lot of the smoke and it had the characteristic bluish tinge of the smoke from a fire burning well, out-of-doors on a clear day. As I walked on, I noticed that the smoke had no movement at all. So I looked more intently and realized that it wasn't smoke at all, but the snow on a steep bank behind the house. The trees near the house had given the smoke its proper shape, but most of the blue faded out when I realized that it was snow. Also the texture of the snow seemed to be thicker and rather rougher (P 4; M 2, M 6; A 3, A 5).

(A) I was looking for a two-cent stamp in a box of writing-paper. As I flipped over the pieces of paper, the light from an electric bulb filtered in upon the white sheets. I saw these yellow patches of light which flashed by as the pink stamps. Not only were they pink, but they also had roughly the shape of a stamp standing on end—about diamond shaped. I could also see some blurred marks on the stamps which stood for the face. I was certain even when I saw them that I did not have so many stamps, and I am almost as certain now that I saw the yellow of the light at the same time that I saw the pink of the stamps, and the irregular outline of the shadow at the same time that I saw the smoother shape of the stamps. The whole thing was over so quickly that I did not have time to become entirely critical, but I doubted the color and shape even while I was seeing them. The turning of them over had so altered the position of the papers and consequently the size of the patches of light that I was unable to get even the meaning of stamps a second time (P 3; M 2; A 4).

(A) When I opened my eyes this morning I saw a radiator, with six sections, opposite my bed. The radiator was very tall, but I remember seeing only a foot of it, the top foot. I saw the gilt color of the radiator and some rather blurred designs on each section. In a way these sections seemed to be all of the same size, and in a way their sizes seemed to vary. Also there seemed to be colors other than yellow in some of the sections and the gilt wasn't really bright enough. These three discrepancies were enough to make me become very critical toward the experience, and then the meaning 'books' came. The radiator vanished entirely and I found myself gazing at six books, all of different colors and shapes, in a small book rack on the top of a desk. In all the bedrooms at home, the radiators have the same position in reference to the beds that this desk had to the bed in which I was sleeping. I think the mere fact of waking up and seeing six upright things was enough to give the radiator (P 3; M 2, M 6; A 4, A 5).

(A) An hour before I had been talking to two girls in the hall of one of the buildings. I noticed that they were both wearing brown coats and hats, and, as well as I can remember, I remarked on the fact; I am sure I thought about it. Just now I saw these girls across the campus. I was too far away to distinguish their features, but I did make out their figures and saw distinctly the brown of their coats and hats. One skirt had a fringe on it where it should have been plain. This fact caused me to become critical, and as I did so the strangest thing happened. The girl with the fringed skirt remained as she was, but the other girl changed entirely. Not only did the color of her hat and coat change from brown to blue, but she actually shrank, almost gradually, in from the top, and spread out just as gradually toward the sides. She had been a tall thin girl in a brown coat and hat; now she was a short rather stout girl in a blue coat and hat. I ran to overtake these two people to see who they really were, and when I got almost up to them, I saw that they were not the girls I thought they were, but two girls with whom I was only very slightly acquainted P 1; M 2; A 3; P 1; M 1, M 3, M 6; A 1, A 2, A 5).

(A) I woke up this morning to find that it had rained in the window by my bed. When I got up I looked at the floor under the other window, on another side of the room, and saw there a small puddle of water. I saw the smooth liquid of the water, but I also saw that it stood much too high above the floor, about an inch I should have said. This height of the water worried me, so I went over toward it to examine it better. As soon as I started moving, but after I had started looking very intently at the puddle, the shiny smoothness disappeared and I was staring at a sandal over-shoe (P 3; M 2, M 6; A 3, A 5).

(A) When you look from a certain window of the laboratory you can see, off in the same direction, a road and a smoke stack. The road is about twice as wide as the smoke stack and is seen slanting back from the observer; the smoke stack stands erect. Just now I looked from the window and noted only the smoke stack. It had its proper size and vertical position but appeared too far to the left. Then as I looked at it to see why it was displaced, it spread out and became wider, and seemed to slant back into the hill, and I discovered that I had been looking at the road (P 1; M 2, M 6; A 3, A 5).

(A) Early this morning I heard the chimes playing "In the Gloaming." I wondered why they should play it so early in the morning, but it wasn't until several seconds later, when I noticed a defect in the rhythm, that my attitude toward the piece became in any way critical. Then I heard an entirely unfamiliar melody. The piece was repeated and I listened carefully to see how I could have made the mistake. I found the value of the notes in the first four measures to be almost identical with those of the first four measures of "In the Gloaming," and their pitches to be not so very different, but not at all the same (P 1; M 1, M 2, M 6; A 1, A 5).

(A) The chimes were playing "Seeing Nelly Home." I heard the chorus played straight through once, but after it was over I realized that there had been something wrong with the time; the pitch of the notes had seemed correct. So when the chorus was repeated I listened quite carefully and I found the melody to be strange to me, and that I had not only changed the pitch of most of the notes but had actually quickened some and retarded others, and filled in a few pauses (P 1; M 2, M 6; A 3, A 5).

(A) The chimes started "Deep River." They played for about three measures; then the time became too rapid, and I heard an unfamiliar melody (P 1; M 1, M 2, M 6; A 1, A 3, A 5).

We had chocolate pudding for dinner. Late in the afternoon, I went to the refrigerator and got out a bowl of the pudding. I put sugar and cream on it and began to eat it. I was swallowing the first mouthful when I noticed that something was wrong. Immediately what I had in my mouth tasted salty and greasy and I realized that it was gravy. I cannot say that it had tasted just like the pudding before this, but it had certainly tasted much more like it than it had like gravy (P 1; M 1, M 2, M 6; A 2, A 5).[11]

III. EXPERIMENTS

Although we have presented our casual instances first in this paper, the experiments which we are about to report were performed simultaneously with the collecting and classifying of these instances. As soon as, from some chance example, we caught the hint of an experiment we worked out and performed this experiment; testing, by varying them, the conditions we already suspected; checking, by a knowledge of the stimulus-object, the nature of the untransformed perceptive experience and the actuality of the overlay; and learning, by using many observers, trained and untrained, trustful and distrustful, more about the overlay itself. It should be borne in mind, for a clear understanding of the latter part of the paper, that these experiments were not performed after the completion of the logical classifications, but that knowledge gained from them aided in the making of the classifications.

The *O*s in these experiments, which extended over the greater part of two years were: Dr. H. P. Weld (W), Dr. K. M. Dallenbach (Da), Dr. L. B. Hoisington (Ho), Dr. H. G. Bishop (Bi), members of the instructing staff; Miss C. C. Braddock (Br), Mr. S. Feldman (Fe), Mr. J. P. Nafe (N), Mr. A. C. Reid (Re), Miss E. Moeller (M), Mr. A. W. Garrison (Gn), Mr. M. G. Scheck (Sk), and the writer (A), graduate students in psychology, all of whom had observed in previous experiments; Mr. F. L. Bixby (By), Miss F. H. Burns (Bs), and Miss D. D. Durling (Du), graduate students with no previous experience as *O*s; and 48 undergraduates. The undergraduates, with a few exceptions, were seniors and juniors who were working on minor problems in psychology or were taking the advanced laboratory course.

A. Natural Objects

Hering[12] and Katz[13] tell of five natural objects, snow, coal, gold, blood and grass, which the memory color falsifies especially. They also offer methods for eliminating the objectivity and thereby the memory color of these objects; Hering looks at the object through a tube, Katz views it through a hole in a screen. Our first problem of an experimental nature was a study of these overlays by the methods suggested.

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[11] We limit this list to the first hundred cases that came to our notice. The examples might be extended indefinitely. One has only to be engaged upon some intellectual task, such as thinking out a lecture or getting ready for an interview, and at the same time to be set for noticing memory color, —and the memory colors, especially if one moves about while thinking, fairly crowd upon one. We have a report of three marked instances noted within half an hour: after that, the *O* impatiently dismissed the memory color *Aufgabe!*

[12] *Loc. cit.*

[13] *Op. cit.*, 214 ff.

(1) *Grass.*—Grass was the first object studied. We wished to obtain, by comparison of the color of the object grass with that of its film, a measure of its memory color. We selected a patch of green grass approximately 8 x 5 m., and in a window directly overlooking this patch set up our apparatus, including a color mixer and a reduction screen, with a 1.25 cm. hole, placed 60 cm. from *O*'s head rest. For eliminating the contrast, which the reduction screen induced on the grass seen through its hole, and the shadow, which it caused on the disc of the color mixer, several alterations in the original arrangement of our apparatus were necessary. Consequently three weeks passed between *E*'s selection of the patch of uniform green grass and the *O*'s first attempts to equate the color of the revolving disc to that of the object grass. Not one of the 6 *O*s, Bi, Br, Ho, N, Re and A, could make a satisfactory match, because, as every one reported, the grass in the limited area was not of uniform tint or chroma. As it was now late autumn, we attributed this irregularity to the effect of frost, and suspended our work until spring. When, in the following spring, *E* noted that the grass was quite green the experiment was resumed. Again the *O*s reported that they could not equate the color of the disc to that of the grass, because the grass in the chosen patch still showed a variety of hues and tints. It was obvious that we must select another patch of grass which should present, under careful scrutiny, an area of uniform green of sufficient dimensions to fill the hole in the reduction screen. Although *E* and the 6 *O*s sought it during the remainder of the spring and during the summer, they were unable to find the requisite area. They found a number of plots, such as nearby lawns or golf courses, some of which had an extent much larger than that required, which were absolutely uniform and very green under casual inspection; but as soon as inspection became to a slight degree critical, the uniformity was gone, and the *O* saw not one green but many. In some cases, a patch, which had been all of one green, even showed spots of brown; but never was the greenest part of a patch, critically regarded, found to be intrinsically greener than the entire area had appeared under the casual attitude.

When, however, fields of grass or grain were so far from the *O* that their areas appeared very tiny, the uniformity of their color usually persisted. These remote fields apparently offered an opportunity for measuring the memory color of grass; and the fields of grain on a hillside two miles from the laboratory, but visible from its windows, were the objects selected for our study. For eliminating the objectivity of these far-away fields, we used a ring 1 mm. wide whose hole was 5 mm.[14] in diameter. This ring was placed on the end of a slender rod which could be attached to the window-sill. In the room behind the window, and placed at an angle of 90° to it, was a grey screen with a square opening slightly larger than the hole of the reduction ring. Through this opening could be seen the disc of a color mixer. The tint of the ring was equal to that of the screen, so that the contrast induced by each was the same. In the experiment the *O* was asked to select the field on the hillside which to him seemed the greenest, and to adjust the ring so that it included this field; and then to equate the color of the disc to that of the film and to that of the field seen as an object. The difference between these two matches we were to consider the measure of the memory color.

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[14] For reducing the color of these natural objects, *E* carried about with her small rings made from the odd numbers of the Hering grey series. The width of these rings was 1 mm., and the diameters of their holes varied, by steps of 1 mm., from 5 to 9 mm.

The *Os* were Bi, Ho, Na, W and A. Observations were made on clear and cloudy days. We never obtained a quantitative measure of the memory color of grass; but our method yielded the following results.

Clear Days.—Three *Os*, A, Bi, and N, saw a marked difference in chroma when a field was seen casually as an object and when its color was seen observationally as a film. Yet a record of this difference was impossible, because the very comparison of the color of the field with the color of the disc necessarily forced the *O* to become critical toward the color of the field; and the color of this field seen critically was practically identical with that of its film seen observationally, though quite different from that of another field seen casually. We append some reports.

A "I selected the greenest field on the hill and adjusted the ring so that almost its entire area was visible through the hole. I matched this film to the disc without any difficulty, and, leaving the match on the mixture, removed the ring so that I could make my second match for the color of the field. I looked at the field carefully to get a good estimate of its color and then turned to the disc. The color on the disc matched that of the field exactly. I was immensely surprised and looked again at the hillside. The field which I had selected had been the greenest on the hill; but when I now looked at it critically, I saw it as less green than the surrounding fields, whose color I still took more or less casually. As soon, however, as I became critical toward these other fields, their color also became more faded. I was able, by saying to myself 'This is the same old field and just as green as ever', to see the rich green again. But as soon as ever I looked at this field for the purpose of estimating its color correctly, this color was quite faded" (P 2; M 1, M 4, M 6; A 1, A 5).

Bi and N were instructed to select the greenest field, but to make their first match to the field, and not to the film, as A had done. They were both unable to make a successful match.

Bi "I cannot match the color which I originally saw. As soon as I become critical toward this field it becomes less green than those surrounding it. I can, by shifting my attitude, let the green return and see it as greener than the surrounding fields, but I cannot hold it long enough to make the match" (P 2; M 1, M 4, M 6; A 1, A 5).

N "That's funny. This field does not look the same color as it did before. It is now less green than the surrounding fields which before were less green than it" (P 2; M 1, M 6; A 1, A 5).

For 2 *Os*, Ho and W, there was no chromatic difference between the green film and the green field.

Under Abnormal Illumination.—When there was a haze of any kind across the valley, so that the hillside was seen under what Katz might term abnormal illumination, all five *Os* found a color difference between a casually inspected field and its film. Again, however, the critical attitude was as effective as the ring in dispelling this overlay; so that again a quantitative measure of the overlay was impossible.

A (rainy day) "I looked out across the valley and all that I could see was a grey mist, darker in some places than in others but with no hue. As soon as I made out the lighter patches to be the familiar green fields, their color was seen as green. This green did not stay long enough for me to make a match, but came and went as I was trying to hold it" (P 2; M 1, M 6; A 1, A 5).

Bi (cloudy day) "There was a decided loss in chroma when I looked through the ring at the field which I had just now seen as quite green; but the green which I now see without the ring is about as poor in chroma as that which I saw through the ring" (P 2; M 1, M 6; A 1, A 5).

Ho (smoke over the hillside) "The field which I selected (casually) was a green of rather poor chroma, but it was definitely green; now (critically inspected) it is a grey with only a hint of green, and the hint gets pretty weak. I really don't believe that I could recognise it as green. It has the color of what I saw through the ring, but there is a difference in texture" (P 2; M 1, M 6; A 1, A 1).

N (cloudy day) "The chroma is not as good as I thought it was. When I selected it, it was the greenest field in sight, and now its color can hardly be distinguished. It is about what I see through the ring" (P 2; M 1, M 6; A 1, A 5).

W (smoke over the hillside) "When I first saw it as field I did not doubt the green, but now there is a question as to whether there is any green there or not. When I looked through the ring, the yellow-green did not come" (P 2; M 1, M 6; A 1, A 5).

The results of this investigation, extending over a number of months, forced us to the conclusion that in our ordinary seeing of grass the memory color serves two purposes.

(1) If the grass is near and would under an observational attitude be mottled, the memory color causes this mottled area to appear uniform; in doing this it does not necessarily make the greenest parts of the area intrinsically greener, but it does make the less green parts equal to the greenest.

(2) When the grass is seen from a distance, the memory color usually enhances the chroma of the green, especially if the conditions are such that under an observational attitude there would be little or no 'color' visible. In both cases, however, a completely critical attitude is adequate to dispelling the overlay, even though the meaning of grass remain.

(2) *Snow.*—The problem, method and results of our experiments on the memory color of snow are so similar to those of the experiments on grass that a detailed account is unnecessary. Although we watched diligently for two winters, we did not find an area of snow, large enough to make color-matching practicable, which would remain completely uniform under critical scrutiny. The change to irregularity with a slight shift of attitude was perhaps more striking here than with the grass, and warrants our quoting a typical report.

A "I looked from the window of the laboratory and saw almost the entire campus covered with soft white snow. This uniformity lasted only a few seconds, however; for as soon as I looked more closely to decide which was the best spot to include in the hole of my screen, it broke up into irregular stretches of yellow between large patches and small streaks of blue. It would have been useful to have got a disc to match these blues and yellows, but this was impossible, because they showed further differences in tint. The yellow in some places was brighter than our whitest paper" (P 2; M 1, M 6; A 1, A 5).

The differences in hue were less on cloudy than on clear days; but even then differences in tint, due to the drifting of the snow and the uneven surface of the ground on which it lay, were noticeable as soon as the attitude became the least bit critical. Even immediately after a heavy snow, when these differences were greatly diminished, color-matching was unsatisfactory because of the rough texture of the snow.

Snow seen from a distance also often showed a variety of tints when critically regarded, as grass did not. The critical attitude always overcame the whitening effect of the memory color on distant snow, just as it cancelled the greening effect of the memory color for distant grass. Therefore no matches were obtainable, and as the reports were similar to those of the preceding investigation, we shall omit them.

(3) *Coal.*—We had been observing coal, in bins, in cellars and in heaps out-of-doors, for three months, before we found a trace of a memory color. The coal which we examined during this time did not change in color when seen casually, critically or (by adjustment of the ring) observationally. The first memory color for coal was discovered out-of-doors on a cloudy day. It was seen by A and two untrained *Os*.

A "I saw a heap of coal about fifty feet away. It was all of a uniform dark grey. I adjusted the ring so that it inclosed an area in the very middle of the heap. This area was much lighter than any of the heap had appeared in the earlier casual observation. I then took away the ring and looked critically at the entire heap. It was no longer a uniform dark grey, but was much lighter in some places than in others. I had happened to get one of these lighter places inclosed in my ring, and its tint was the same through the ring and without it when regarded critically" (P 2; M 1, M 6; A 1, A 5).

P "I thought the pile was all the same color when I first looked at it; but now, since I have seen the middle through the ring, I see that it has many shades" (P 2; M 1, M 6; A 1, A 5).

A continued her observations throughout the year. Although she had many experiences similar to the one quoted above, in no instance did she find the critical attitude failing to break up the uniformity of an apparently uniform heap of coal; nor, when the uniformity was broken, had the memory color intrinsically blackened the darkest part of the heap. The coal was never given in areas large enough to be examined from a great distance; but the results of the observations of coal seen at close range bore out the result of the investigation of grass, that one of the effects of the memory color is to make uniform familiar objects which under a critical attitude show irregularities. The effect of overlay was again to level up rather than to average.

(4) *Gold.*—The roof of the tower of the University Library aided in our study of the memory color of gold. This roof is made of slightly pinkish yellow tiles, of moderate chroma; but a number of the faculty and students take it to be gilt, and see it with a metallic lustre. *E* asked members of the department, and the girls in the house in which she lived, the color of the roof. Those who replied 'gold' and described the color and lustre, she asked to watch for it afterward and be sure. The reports of the trained *Os*, while agreeing with one another, differed from those of the psychologically untrained. We quote one report from each group.

N (trained) "I was on the East side of the tower at eight o'clock this morning and the sun was shining brightly on that side. When I first looked at the roof, it blazed out as solid brilliant yellow gold, all of a piece. Then I looked more carefully, as you had asked me, and it was nothing more than a pinkish buff with no metallic lustre. It seemed to be made of great heavy tiles which did not fall together very well and between which were big heavy seams" (P 2; M 1, M 6; A 1, A 5).

O (untrained, facing a shaded side of the roof) "It isn't gold from here, but a yellowish grey; but if you come around to the other side you will see it look just like gold. I have been watching it since you asked me to, and yesterday it was a perfect yellow gold. I showed it to the person I was with and he also saw the gold." Again (facing a side of the tower on which the sun was shining): "No, it isn't gold from here either, though it is more like it than it was on the other side. I am absolutely certain that it did not look like this yesterday but was perfect gold" (P 2; M 1, M 6; A 1, A 5).

Again the critical attitude was equal to dispelling the overlay.

(5) *Blood.*—Our procedure for studying the memory color of blood was simple. *E* washed the dorsal side of her left fore-arm with soapy water until the epidermis was soft. Here, about 10 cm. above the wrist, she drew

a thin irregular lightish brown line, about 4 cm. long, letting the light brown ink spread into the tiny grooves of the skin. On the same side of her dress she fastened the guard to a fraternity pin; the opened pin, which was fastened to the guard, dangled loosely with the point showing. The loose pin caught the attention of any one with whom *E* talked. When this person told *E* her pin was unfastened, *E* glanced quickly from the dangling pin to her arm and, exhibiting the scratch, exclaimed: 'Oh, it scratched my arm.' After the *O* had examined the 'scratch' *E* covered it with her handkerchief and demanded a description of the blood. When this was finished, *E* removed the handkerchief, and requested the *O* to take a critical attitude over against the mark on her arm and to describe the color and texture of what he saw.

Results. Four *Os*, Ho, N, Re, and Si, an undergraduate, found changes in color and texture, when the scratch was examined critically. All reported that the critical seeing of the scratch showed a smooth light brown line; but two reported that first they had seen a bright red scratch, and two that they had seen a dark red clot.

Ho "I saw it as a scratch of bright red blood. I saw a smear of blood. It was difficult not to see it as blood even when I examined it closely; but under careful scrutiny it changed color very quickly, as if it changed under my eyes without my actually seeing the change. Under this careful scrutiny it was light brown and smooth" (P 2; M 1, M 6; A 1, A 5).

Re "I saw red glistening blood like that from a fresh cut. I saw it standing up above the surface of the skin and running out into little streaks on either side. I did not doubt it for a moment; and then it changed to a flat brown mark" (P 2; M 1, M 6; A 1, A 5).

N "At first it was dark red and clotted over, like any wound that has stopped bleeding. I could see the raised skin on both sides of it. Now it is a yellowish red and all smooth" (P 2; M 1, M 6; A 1, A 5).

Si "I saw it as blood but not fresh blood. It was red but not shiny. It stood up above the rest of your arm. Now it is flat and brown. The red was darker and thicker than the brown" (P 2; M 1, M 6; A 1, A 5).

From our investigations of the perceptions of the five natural objects grass, snow, coal, gold and blood, we may say that Hering and Katz were correct in claiming that the seeing of these objects is ordinarily affected by a memory color. Although our investigation failed to give us a quantitative measure of a single memory color, it was thus not unfruitful.

B. Controlled Experiments: the Overlays in Perceptions of the Various Sense Modalities

Experiment 1. Smell

Three 'wrongly' perfumed artificial flowers were the objects in our olfactory experiment; a pink rose scented with violet perfume, a red rose with lily perfume, and a bunch of violets with lily perfume. The *Os*, who were Br, Fe, N and Re, and 18 undergraduates (3 with some psychological training as *Os* in former experiments, 15 psychologically untrained), did not know until the experiment was completed that they were taking part in a psychological investigation.

Our first method consisted in showing the flower to the *O*, telling him that it was a new kind of artificial flower which smelled as well as looked like the real flower it represented, and asking him if he did not think the imitation excellent. Afterwards the *O* was asked to shut his eyes and to identify the odor presented. Later we altered our method, so that *O*'s interest in the object, not *E*'s question about the odor, should initiate the smelling. In this modified procedure, the flowers were placed on *E*'s table or pinned to her dress. When an *O* noticed the flower, whether he took it to be real or artificial, *E* quite casually told him to smell it.

Results. Only one *O*, untrained, of the 10 *O*s (4 graduate students and 6 untrained undergraduates) serving under the first method, gave unequivocal evidence of the presence of an olfactory imaginal overlay. Of the 12 *O*s, serving under the modified procedure, 2 gave no evidence of such an overlay. A brief summary of the results of the 10 *O*s (Ba, Fu, and Sm with psychological training, the remainder untrained) is as follows.

Bc smelled the rose with the violet perfume as rose, the rose with the lily as rose, and the violets with the lily as violet. With eyes closed, she could identify both odors correctly (P 1; M 1; A 1).

Bn. Same as for Bc.

Bt smelled the rose with the violet perfume as rose; also the rose with the lily as rose, but reported that while she was smelling it the odor changed to 'something unfamiliar'; the rose with lily, she said, smelled as the violets had done after the odor changed. With eyes closed, she could identify the violet, but not the lily perfume (P 2; M 1, M 6; A 1, A 5).

Ba smelled the rose with the violet perfume as rose; and then reported that, when he realized the flower was artificial, the odor changed to that of violets. He identified the lily perfume on both of the other flowers (P 2; M 1, A 6; A 1, A 5).

Bk smelled the rose with lily perfume as rose, and then reported that the experience changed to lily-of-the-valley. She identified the two odors on the other flowers (P 2; M 1, M 6; A 1, A 5).

M smelled the rose with lily as rose. She suspected it was an experiment after this, and got no overlay for the other flowers. She could identify both odors correctly (P 2; M 1, M 6; A 1, A 6).

Fu first smelled the violet with lily as violet, and then reported that the odor became that of a lily. She named the lily and the violet of the two other flowers (P 2; M 1, M 6; A 1, A 5).

I smelled the rose with lily as rose, and then the experience changed and became unfamiliar. She reported that the odor of the two other flowers was also unfamiliar, and she could not identify either one with closed eyes (P 2; M 1, M 6; A 1, A 5).

L smelled violet with lily as violet, and then said: 'It's changed to lily.' She identified correctly the odors of the two remaining flowers (P 2; M 1, M 6; A 1, A 5).

Sm smelled the rose with violet as rose, and then reported a change in the experience, which she later identified as the odor of violets. She identified the lily perfume on both the other flowers (P 2; M 1, M 6; A 1, A 5).

The difference in result under the two methods demonstrates the necessity of that condition for the appearance of the overlay which in our classification we called the *casual attitude;* for with one exception (the untrained *O* who described three apparently adequate overlays) every *O* serving under Method 1 reported a tendency to doubt *E*'s statement about the odor and to detect something 'wrong' in it. This sceptical disposition we may consider the extremely critical attitude. The changes in the olfactory experience reported by the *Os* serving under Method 2 indicate a shift from a completely casual to a more or less critical attitude. This change occurred during the sniffing of the flower, usually when a flower which was thought to be real was found to be artificial.

Fu "I saw the violets in the vase on the table, and thinking they were real leaned over to smell them. As I got very close I noticed the clothy texture of the petals and realized that they were artificial. I sniffed at them just the same and for the first instant they smelled just like freshly plucked violets; then their odor changed to lily" (P 2; M 1, M 2, M 6; A 1, A 4, A 5).

After an *O* had once adopted this doubting or questioning attitude toward the odor, he reported no more overlays.

Experiment 2. Tone

This experiment was carried out in four adjoining rooms of the laboratory. In one room *E* beat out three familiar melodies ("America," the Cornell "Alma Mater," and the "Wedding March" from *Lohengrin*) upon a spiral gong, whose sole note had the pitch of the F below middle C.

Method 1. The *O* had been asked to observe in an experiment whose object was the determination of the intensive limen for melodies. His instructions were: "Listen for a melody which will be played in the next room. If the notes are strong enough for you to recognize the melody, name it. Then move your chair back two feet and listen for the next melody."

Method 2. The *Os* were uninstructed, and were supposedly observing in other experiments. They listened occasionally to the tonal experiment in the next room and later reported to *E* what they heard.

With the first procedure there were 14 *Os*, 2 graduate students, Fe and Re, and 12 undergraduates taking advanced laboratory courses in psychology; in the second procedure there were five *Os*, one graduate student, Br, two seniors with psychological training as observers, Ds and K, and two students in the advanced laboratory course, C and Ja.

Results: Method 1. Only 4 *Os*, all undergraduates, heard the melody ascend and descend in pitch, and these heard the change only in the first melody.

Bec "I recognized the first tune as America and I thought it was being played upon tuning forks of different pitches. When I heard the second piece (Alma Mater) I could not recognize it immediately; and while I was trying to figure out what it was I realized that the same note was being played over and over. I think the procedure must have been changed between the first and second pieces" (P 1; M 1, M 6; A 1, A 5).

Ber "In the first piece I somehow had an idea that you were just using one note, but still I heard the melody go up and down. In the second melody I was certain from the very first that there was no change in pitch" (P 1; M 2, M 6; A 4, A 5).

Ry "I heard the melody go up and down in the Wedding March, but I felt there was something funny about it, I didn't know what. In the second tune (America) I saw that it was all the same note" (P 1; M 2, M 6; A 4, A 5).

Sm "I thought I heard changes in pitch for the first two measures of America, but when it did not go as it should in the third measure I realized it was all being played on the same note. I heard no change in pitch after this" (P 1; M 1, M 6; A 1, A 5).

Method 2. All five *O*s heard some change of pitch.

Br "At first I realized that some kind of a tone experiment was being carried on in the next room. I do not know whether I heard rise and fall of pitch then or not. Suddenly I heard "God Save the King;" and I remembered beating it out as a child. Just then I heard it as all one pitch, but I said to myself "Is it all one pitch?" and I immediately heard it go up and down. It went neither as high nor as low as it should, but sounded as if you were playing it on certain notes, one for high, one for low and one for medium. Then I wondered if there were three notes or only two, and immediately I realized that there was only one" (P 1; M 3, M 6; A 2, A 5).

Ds "I recognized all three tunes, but the range of the notes did not seem to be great enough. The middle notes were all right, but the higher notes seemed to be flat and the lowest ones not low enough" (P 1; M 3; A 2).

K "I heard the three tunes, but there was something queer about the pitch of the notes. That's why I wondered what kind of an instrument you were using" (P 1; M 3; A 2).

Ja "My partner and I heard the Wedding March and wondered who was playing it. After that I heard no more melodies, but one note over and over" (P 1; M 1, M 6; A 1, A 5).

These results give us descriptions of two of our partially adequate overlays; the conflict and the compromise. They also show, those from Method 1 in particular, how this partial adequacy sooner or later forces the *O* to become critical. A comparison of the results of the two methods demonstrates once more the necessity of an entirely casual attitude for the appearance of an overlay. Method 1 did not necessarily cause the *O* to doubt the change in pitch of the melody, but it did require him to listen carefully to the melody itself. This attitude was therefore more nearly critical than that induced by Method 2, but evidently not so critical as that induced by the questions in Experiment 1.

Experiment 8. Taste and Smell

Our materials were the Minoras (chocolate mint wafers) and Anolas (chocolate flavored sugar wafers) confused in one of the casual instances.

Method 1. E told her *O*s in Experiment 7 that, to repay them for their kindness in observing, she would give them some peppermint wafers of which she was especially fond. For ten successive observation periods she offered the *O* a box of Minoras, was insistent that he take more than one, and nearly every day spoke of the peppermint flavor. On the eleventh day the box which she offered the *O* contained Anolas. She asked him to describe how the first wafer had tasted and how the second wafer was now tasting.

Method 2. Upon opening a cupboard in the laboratory *E* expressed surprise at finding a plate filled with wafers (Minoras). Bi, Sm and Ber who were near were really surprised to see them. *E*, being unable to find to whom the wafers belonged, proposed that they four eat some; and as she bit into one exclaimed, "Why it is flavored with peppermint!" The following day the Minoras still on the plate were replaced by Anolas. At separate times *E* suggested to Bi, Ber, and Sm that they see if the peppermint wafers were still there; "if so, let's eat some." *E* asked for a description of the flavor of these wafers.

A also was an *O* in this experiment.

Results. Method 1. (*a*) By and M declared that the flavoring of the wafers was just as it had been on the preceding days. By ate only one wafer, but M ate three, and insisted that she could taste the peppermint in each one (P 2; M 1; A 1).

(*b*) Gn and Sk each thought that the peppermint in his first wafer was weak. With his second wafer, Gn reported that he could alternately taste and not taste the peppermint; but Sk continued to taste weak peppermint in his second wafer (P 2; M 3; A 2, A 4).

(*c*) Bs and Du tasted only chocolate (P 2; M 6; A 5).

Method 2. (*a*) Ber reported that her first wafer was just as 'pepperminty' as those she had eaten the day before, but that the second one, eaten after *E*'s question, was quite different (P 2; M 1, M 6; A 1, A 5).

Bi reported that when he first tasted his first wafer it tasted decidedly minty, but that while he was eating it he wondered if there was some trick, and the peppermint taste immediately disappeared (P 2; M 1, M 6; A 1, A 5).

(*b*) For Sm, both wafers that she ate, one before and one after being questioned, tasted somewhat like peppermint, but not as much so as those eaten the day before (P 2; M 3; A 2).

A's report is as follows: "I was of course very familiar with the taste of the Minoras, as I had eaten one or two with each of the *O*s. This morning I felt hungry and reached absently for a wafer. The wafer that I picked up seemed to have a brown filling and I judged it to be an Anola. The first bite was merely chocolate; but I wondered if, by making myself believe that it was a Minora, I could make it taste of mint. And the second bite of the wafer tasted of peppermint, but only faintly. The curious thing was that this weak peppermint lasted. This led me to look carefully at the wafer; and I then saw that its filling was white. The third and last bite was more strongly peppermint, but even then not as strong as it should have been. The second wafer that I took from the same box, which I found to be marked 'Minora', was distinctly peppermint" (P 2; M 1, M 2, M 6; A1, A2, A5).

A's report describes an adequate overlay and its gradual disappearance, similar to that reported by Fu in Experiment 1. The similarity of results under the two different methods demonstrates that one impressive experience with an object is as efficient in stamping in an overlay as many indifferent experiences with the same object. In Method 2 the extreme familiarity of the situation kept *E*'s question about the taste from forcing the *O* into a completely critical attitude toward it. Here we have evidence of the close relation between perfect familiarity and the casual attitude.

Experiment 4. Weight

With Sir George Pearson's experience with the potassium in mind, we devised experiments for studying the imaginal overlay of lifted weights.

Method 1. A wide-mouth glass vial, 7.5 by 3.75 by 1.25 cm., bearing the label *Hg*, was lined at the bottom and half way up the sides with tin foil, and filled to the height of the tin foil with water. Had the bottle been half filled with mercury it would have weighed a pound; with the water it weighed 4 oz. We placed it among the tools on the workshop bench. Bi, Br, N and Re, finding it in their way and moving it, were asked to report on their experiences when lifting it.

Method 2. The covers of Titchener's *Text-Book of Psychology* and *A Beginner's Psychology* were filled with basswood so that in form and size they were identical with the ordinary page-filled books. Their weights however were different; the real *Text-Book* weighs 26 oz., the false one weighed only 14; the real *Beginner's* 19 oz., but the false one 11. These false books were substituted for the real on the shelves of their owners. Unless the *O*, having lifted the book on his own account, volunteered a description of his experience while lifting it, *E* asked him to lend her his *Text-Book* or his *Beginner's*, and after he had handed it to her demanded this description.

Results: Method 1. The lifted mercury failed to reveal the presence of an overlay in the experience of any of the 4 *O*s. Every one reported that he realized, more or less clearly, that the mercury would be heavy, and that he was therefore 'set' to pick up a heavy object. The lightness of the weight he noticed immediately and suddenly, as soon as he began to lift the bottle.

Method 2. (*a*) Bi, Da, and W were unable to report on their kinaesthetic experiences while lifting the books. All three reported that their attention was directed not toward the feel of the book, but toward the sight of it and the fact of lending it to *E*. All three were surprised to find how light the false books really were.

(*b*) Br and Fe described experiences which we, with certainty, can designate as overlays.

Br "I took the *Text-Book* from the shelf to look up a reference. Because it was heavy I rested it on the desk while I opened it. When I found that it was wood I lifted it again, and it was light, but not as light as I thought it should be. I picked up another book of the same size, noted its heaviness, and then took up the *Text-Book* again. This time it was much lighter than it was the second time I lifted it" (P 1; M 1, M 2, M 6; A 1, A 2, A 5).

Fe "I saw that the *Beginner's* was made of wood while I had it in my hand. Before that it had felt, so far as I can say, as the real book should, and even then it was still heavy. Then as I continued to feel it, it very gradually became lighter and lighter" (P 1; M 1, M 2, M 6; A 1, A 2, A 5).

(*c*) Ho detected the lightness in weight as soon as he began to lift the *Text-Book*.

A comparison of the results from the two methods indicates that the overlay is more effective when the difference between it and the untransformed experience is small than when it is great.

That Bi, Da and W were unable to report on any overlay because of the relative unclearness of their kinaesthetic experiences is further proof that our interpretation of Hering's "not paying especial attention" as a casual attitude, and not as a lack of clearness, was correct. Yet there was probably something imaginal in the experiences of Bi, Da and W which prevented their noting, as did Ho, the extreme lightness of the books; for their familiarity with them was no less than Ho's.

Experiment 5. Pain

The instructions to the *O*s, who had been asked to observe in an experiment which should determine the limen of subcutaneous pain, were: "The pressure on the palm of your hand will be gradually increased. Watch the pointer, and report the place where you feel pain. Do not wait for intensive pain, but report the first positive change in the quality of the experience." The pointer of the algometer, which exerted the gradually increasing pressure for the first four observation periods, was very large, and the white face over which it moved was marked with heavy black numbers. For these four days, five values were taken for each hand. In the fifth observation period, which occurred two weeks after the last of the preceding four, the *O* was reminded of his approximate limen. The procedure was the same as before; but the spring had been removed from the algometer, so that its pressure, after it was first placed upon the hand, remained constant at a point considerably below that of the pain limen.

The *O*s were Re and 8 students from the course in quantitative psychology.

Results. (*a*) Four *O*s reported the change as soon as the pointer reached the critical mark. They all described this change as being identical with the changes on the preceding days.

(*b*) Three *O*s reported a few 'faint' or 'weak' changes; then 'no change'.

Ba "Same quality, but not as intense as before" (P 3; M 3; A 2).

Gs "I must be sleepy to-day. I feel the change, but it is weaker than usual." This report was for the five trials on her right hand; she reported 'No change' for the left hand (P 3; M 3, M 6; A 2, A 5).

Re "An indistinct something, but not much like pain." "Faint, but otherwise all right." After this he reported that the experience did not change, despite the movement of the pointer (P 3; M 3, M 6; A 2, A 5).

(*c*) Four *O*s reported that they felt no change in their experiences, although they saw the movement of the pointer.

The reports cited under (*b*) again afford a description of a partially adequate overlay (compromise); and the change to inadequacy apparently indicates a growing criticalness of attitude occasioned by this partial adequacy.

The similarity of this experiment to the experiments reported by Edwards in his study of Sensory Suggestion[15] leads to a comparison of the two. We note one fundamental difference between Edwards' general method and our own. The attitude of his *O*s was by hypothesis observational, and the images which they described were evoked by an intruction which *E* gave them while under this attitude. The smelling, listening, tasting, and lifting of our *O*s in the 4 experiments just reported were of necessity object-consciousness smelling, listening, tasting, and lifting. Their images were not grafted on to their experiences by a formal instruction, but came to them as a given and integral part of a familiar object. Even in the test series of this experiment, though we did instruct our *O*s that the pressure would be increased and implied that the quality of the experience would change, we relied for our imaginal overlay not upon the instruction but upon the familiarity of the total situation, including the sight of the rising pointer and the black numbers. This situation we assumed to be an object-consciousness situation. Yet it is barely possible that on the test day our *O*s took an observational attitude toward the tactual experience, and that the change in quality was brought about solely by the instructions; just as, in some of Edwards' experiments, the image which his *O*s describe might have been produced by the recognition of a familiar object and not by observational instructions.

Experiment 6. Touch

The materials were eight squares of cloth,—two velvet, two silk, two cotton, two serge,—all with both sides identical, folded over and sewed so that they were all of equal thickness.

Method 1. The *O* sat at a table on which was a narrow board covered over with a black cloth. *E* gave him these instructions: "Before you are four squares of material. They are velvet, silk, cotton and serge. Begin with the left hand square and try to identify them by feeling them between your thumb and finger. When you are sure of any sample, thrust it over the board. If you are sure at the first attempt, throw out the sample at once. If you are not sure you may test the samples over and over, beginning always at the left and going straight through to the right. Close your eyes, and when I say 'now' begin to feel the left hand sample." There were always four samples on the board, but never, under this procedure, more than three different kinds of material; every set therefore contained two identical samples. These were never placed next each other. Every set contained a new substitution and offered a different succession of the samples.

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[15] A. S. Edwards, *An Experimental Study of Sensory Suggestion*, this JOURNAL, xxvi, 1915, 99 ff.

When the *O* had named all four samples and thrust them over the board, *E* covered them again with the cloth and asked *O* to open his eyes and describe the feel of each sample which he had named. This procedure was continued until the *O* was suspicious of the method and failed to give any evidence of the presence of overlays in his experience. Then Method 2 was adopted.

Method 2. The instructions were the same as before; but, for the first six observation periods, samples of all four materials mentioned in the instructions were actually presented. In these practice series the *O*, after he had identified the four samples, was allowed to feel each sample as long as he wished, with his eyes open. In the seventh observation period there was a 'substitution series' which contained duplicate samples. Whatever the result of this series, the *O* had a 'practice series' in the next observation hour; but in the hour after that, a different 'substitution series'. This procedure was continued until every material had been substituted for every other material. The number of practice series between successive substitution series varied from one to four; and reports of the feel of the cloths was required for the practice as well as for the substitution series.

The *Os* in both procedures were Bi, Fe, N and Re; 6 students, Ba, Ber, Gi, He, Ja and Ru, with minor problems in psychology and with some training as *Os* in other experiments; and 5 undergraduates, Ad, Al, Bu, De and Je, taking the course in quantitative psychology.

Results: Method 1. Only one *O* (De) failed to report any evidence of an overlay. The distribution of the overlays which appeared in the reports of the 16 other *Os* was as follows. (1) In 13 cases (cotton identified as serge 6: Bi 1, Ber 1, Fe 1, Je 1, Gi 1, N 1; serge as cotton 6: Ad 1, Ba 1, Bu 1, Fe 1, Je 1, Re 1; silk as velvet 1; F) the overlay was apparently quite adequate; the missing sample was named and its feel described. This feel evidently came when the *O* first touched the sample and persisted until he thrust it over the board, for the *O* never doubted its reality or the statement that there were four different materials before him.

Gi (cotton as serge) "Cotton had dull feel. Serge was rough" (P 2; M 1; A 1).

Bi (serge as cotton) "Number 1 felt woolly and rough, and what I felt as woolly made me say serge; number 3, the cotton (serge), when I took it up was rough, but did not feel woolly. The serge was softer than the cotton and tended to compress between my fingers more than the cotton" (P 2; M 1; A 1).

Fe (silk as velvet) "Silk is smooth and hard. Velvet is smooth but yielding" (P 2; M 1; A 1).

(2) Twice an adequate overlay was late in coming.

He (cotton as silk) "Silk felt different from cotton on second feel. Didn't have slick feeling for a moment, but then the slick feeling came and I felt that they were different" (P 2; M 6, M 1; A 5, A 1).

(3) In seven instances the overlay was only partially adequate, but this partial transformation was stable (serge as cotton 4: Al 1, Ja 1, N 1, Ru 1; cotton as serge 2: Ja 1, Ru 1; cotton as silk 1: Je).

Ja (serge as cotton) "These two felt much alike, but I felt the rib in the serge and not in the cotton. The cotton had a smoother feel and the serge felt more loosely woven" (P 2, M 2, A 2).

Ru (cotton as serge) "Had a hard time distinguishing cotton and serge. Cotton seemed coarser, while the serge had ridges in it and felt somewhat finer than the cotton" (P 2; M 2; A 2).

(4) Three times, however, this partially adequate overlay disappeared between successive feels of the sample which it transformed (Re cotton as serge 1, serge as cotton 1, silk as serge 1).

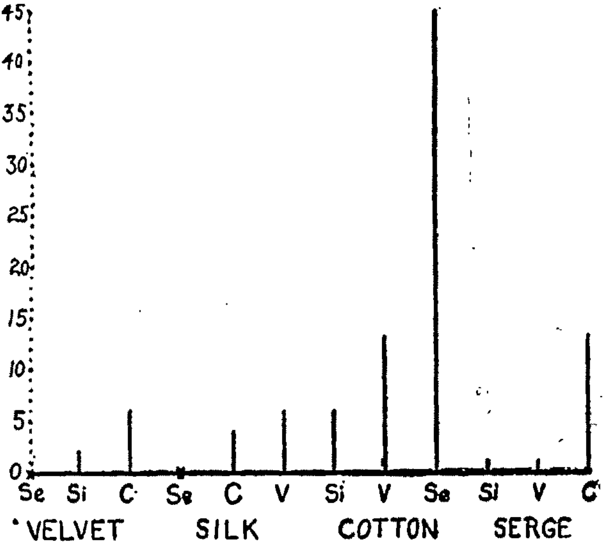
Re (cotton) "At first feel, I thought that the weave of the first was heavier and a little larger, and it felt a little harder, I think. This was my first impression, but the second time I couldn't tell any difference" (P 2; M 3, M 6; A 3, A 5).

Method 2. The effect of the practice series on the attitude of the *O*s was different. Seven *O*s, realizing that they had made 'mistakes' in identifying the samples, resolved not to be 'fooled' again. Their attitude became very critical, and the practice series tended to heighten this criticalness. They reported no overlays in either the 'practice' or the 'substitution' series. Yet the practice series recaptured the confidence of 9 *O*s (Al, Ber, Bi, Fe, He, Je, N, Re, Ru), who thought that *E* was really trying to help them in identifying the samples, and reestablished their casualness of attitude. They reported overlays in the practice as often as in the substitution series, and described those which disappeared and were recognized as imaginal as readily as those which persisted and were mistaken for sensory.

The chart shows the contamination of each actually presented sample by the overlay of another sample, without regard to the adequacy of the overlay or the amount of transformation.

In regard to adequacy, these 97 overlays showed the following distribution.

(1) In 13 instances the transformation of the perceptive experience by the overlay was complete and permanent (cotton by serge 5: Fe 3, Je 1, Re 1; silk by velvet 3: Fe 2, Re 1; cotton by silk 2: Fe 1, He 1; silk by cotton 2: He 1, N 1; serge by cotton 1: Je). The criterion of adequacy was not alone that an actually presented sample was named and described as a material totally different, but also that (*a*) two different materials felt in immediate succession were reported as feeling entirely differently, or (*b*) two different materials felt in immediate succession were described as identical, or (*c*) the *O* was so sure that he was feeling the familiar material that he remarked on the fact.



The ordinate represents the number of cases. The abscissa represents the overlay: the initials, the quality of the overlay; and the full names, the material contaminated.

(*a*) Ge (two serges) "Cotton had fine texture and was yielding to the touch. Serge was less yielding to the touch and I could feel the individual fibres" (P 1; M 1; A 1).

(*b*) Fe (cotton and serge) "The second and third felt just alike I felt the first one and said to myself 'rough, serge'; then the third one felt just like it" (P 1; M 1; A 1).

(*c*) N (silk and cotton) "The first time I felt the two cottons they both felt a little like serge. The second time they were normal again" (P 1; M 1; A 1).

(*d*) Re (cotton, silk, serge and silk called cotton, velvet, serge and silk) "The cotton at first felt a little like velvet; it seemed that I could get the springy texture of the velvet. But as soon as I felt the real velvet I was sure" (P 1; M 1; A 1).

(2) In 10 instances an adequate overlay faded before the *O* could name its material (cotton by serge 6: N 5, Re 1; cotton by velvet 2: He 1, Re 1; silk by velvet 1: Fe; serge by velvet 1: He).

He (serge) "Felt like velvet and then it felt too hard and too cold for velvet. For the first instant I felt the pile and the softness of it; then this gave way to hardness and I recognized it as serge. The softness went out instantly" (P 1; M 1, M 6; A 1, A 5).

N (cotton) "I was looking for serge and I thought I had it. I would have said serge if I had had time to get the word out, but before I could get the word out it lost its roughness and became harder. The roughness melted right away. Then I looked for the softness and it was gone. The melting was gradual but quick. I didn't feel it as rough and then as smooth, but felt it rough and the roughness gradually but very quickly changed to smooth. Then it was cotton" (P 1; M 1, M 2, M 6; A 1, A 2, A 5).

(3) The 74 partially adequate overlays can be divided as follows.

(*a*) In six instances the transformation was so nearly complete that the sample was named from the overlay, but was interpreted as a different grade of the same material (cotton by serge 4: Ber 2, Ru 2; serge by cotton 2: He 1, Ru 1).

Ru (serge) "Another cotton. Did not feel like the other cotton, was more rigid. Still it felt more like the cotton I had just felt than the serge of my memory" (P 1; M 2; A 3).

(*b*) In 8 instances, where the transformation was less complete but still permanent, the sample was again interpreted as a new one, but this time it was described as resembling equally two of the familiar cloths (cotton by serge 5: Al 1, Ber 1, Bi 3; cotton by silk 1: Je; silk by velvet 2: Bi).

Ber (cotton) "I haven't felt this one before. I should say it was part cotton and part serge" (P 1; M 2; A 3).

Bi (cotton) "Not warm, fluffy or scratchy enough for serge; on the other hand more fluffy and more scratchy than cotton. If I rub it lightly it is fluffy and soft. When I compress it the fluffiness yields. Could not feel any weave; it is smooth. Heavy like cotton; the texture is like the serge. Neither cotton nor serge, perhaps a shortnap cotton-flannel" (P 1; M 2; A3).

Je (cotton) "New material. Texture not coarse enough for cotton and too slippery, yet not smooth as silk" (P 1; M 2; A 30).

Bi (silk) "Velvet silk; sticks like velvet, slips like silk" (P 1; M 2; A 3).

(*c*) In 27 instances the imaginal contamination was so slight that the sample was correctly named; yet the overlay came as an integral part of the tactual experience and its effect was constant (cotton by serge 8: Al 1, Bi 2, Je 1, Re 3, Ru 1; cotton by velvet 6: Bi 2, Je 1, Re 1, Ru 2; cotton by silk 3: Bi 1, Je 1, Ru 1; serge by cotton 3: He 2, Re 1; velvet by cotton 2: Al 1, Re 1; velvet by silk 3: Al 1, Bi 2; silk by cotton 1: Re; serge by silk 1: Ru).

Al "Cotton felt as if it might have a little serge in it, just a little" (P 1; M 2; A 3).

Bi "Cotton felt a little velvety, like a very short pile velvet; had the softness of the velvet, but the bumpiness and heavy bulk of the cotton" (P 1; M 2; A 3).

Re "Cotton had smooth silky finish, but not enough to be silk" (P 1; M 2; A 3).

Bi "Last velvet a little silky; kinaesthetically it was silk, tactually velvet. Had soft slipperiness like silk. Felt different from the first velvet which was heavy and unyielding" (P 1; M 2; A 3).

Re "First silk didn't feel so smooth or so glossy, the second one had more of the real silk feel. The texture of the first one was very fine but not slippery; like cotton in this respect, but not as rough in texture as the cotton" (P 1; M 2; A 3).

(*d*) 33 partially adequate overlays were transitory; their disappearance was in general described as a 'fading' or 'melting' (cotton by serge 18: Al 1, N 8, Re 9; cotton by velvet 5: Al 1, N 1, Ru 3; serge by cotton 5: Re 1, Ru 4; velvet by cotton 3: Ru 3; cotton by silk 1: N 1; silk by velvet 1: He 1.

Re "When I first felt the cotton, I had the tactual experience of serge twills. I felt the rough surface of the serge. Then it faded out; not fully, immediately, but I could feel it getting less and less. When I identified the sample as cotton, I discovered that the twills weren't there at all" (P 1; M 1, M 2, M 6; A 1, A 3, A 5).

This complete disappearance of an overlay usually left behind it the untransformed experience, but not always. The perceptive experience was sometimes retransformed, partially or completely, by a second overlay.

Al "When I first felt the cotton, it had a little velvet in it, just a little. This was just at first, then it changed and felt more like serge" (P 1; M 2; A 3).

He "The silk (which was in fact cotton) had a little bit of the texture of the velvet for a moment. I felt it again and could not detect the velvet" (P 1; M 2, M 1; A 3, A 1).

N "When I first felt the silk (which was in fact cotton) it felt softer than it should, more like velvet. Then it became normal again and just like the other silk (the real silk)" (P 1; M 2, M 1; A 3, A 1).

In this experiment, which was more thorough than any of those preceding it, we secured a confirmation of the more important results of these earlier experiments. The destructive effect upon the overlay of the critical attitude, first manifest in its extreme form in Experiment 1 as a doubting, and modified in Experiment 2 to a careful attending, was confirmed by the negative results of the seven *O*s whose attitude remains critical during the whole of Method 2. Yet the check of the condition of familiarity upon this attitude, first shown in Experiment 3, was further demonstrated by the descriptive reports of the 8 *O*s whose confidence was reestablished by the practice series. These 97 descriptions, while they are of necessity descriptions of integral components of perceptive objects, are as truly phenomenological as perceptive reports can be; and they have their analogies in the reports of the former experiments. Furthermore the results of Method 2 provided the material for demonstrating the fact, indicated in Experiment 5, that the closer the similarity between the required overlay and the untransformed experience, the greater is the liability of the occurrence of this overlay.[16] From a consideration of these 6 experiments it should be evident that the rôle of the overlay in the perceptions of the various sense modalities is identical; that in every one its appearance is dependent upon the conditions of the object-consciousness, casualness of attitude, and familiarity; that its degree of adequacy accords with the favorableness of these three conditions and the similarity between it and the untransformed experience; that, with a change in any of these conditions, there will be a corresponding change in the perceptive experience itself; and, finally, that if this change consist in the disappearance of the overlay the disappearance will, in general, be described as a 'fading' or a 'melting'.

Experiment 7. Color

In this experiment we hoped to secure an objective measure of a visual overlay, or true memory color, and a better psychological description of this overlay than we had hitherto obtained.

The experiment was conducted in a dark room, 380 x 227 cm. On one of the narrower walls of the room hung the stimulus object; directly across from it, and attached to a shelf built out from the other narrow wall, was a color mixer; midway between the object and the mixer, the *O*'s swivel chair was fastened to the floor. All three were a little to one side of the long axis of the room. Slightly to the other side of this axis were two white reflectors on standards, separated from each other by 1 m.; one of them (Light 1) was 1 m. from the mixer and turned toward it at an angle of 110°, the other (Light 2) was equally distant from the object and was turned toward it at the same angle. When these reflectors were fitted with bulbs, each illuminated one half of the room. Each was attached to a double switch, by means of which either or both lights could be turned on.

In every experiment, Light 1 was on when the *O* came into the room, Light 2 was off, and the disc of the color mixer was covered by a grey screen. *O* saw the glass of Light 1 only when he entered the room; never during any of these experiments did he see the glass of Light 2 or, except when it was in rotation, the disc of the color mixer. At the beginning of every observation period, 2 min. were allowed for the *O* to become adapted to the illuminated portion of the room.

Preliminary Experiment A

The stimulus object was a conventional jar-shaped figure, 41 cm. tall and 42 cm. wide at its broadest part. It was made from Denison's "American Beauty" crepe paper stretched over light grey cardboard. Both reflectors were fitted with 200 watt Mazda lamps, and the light from each bulb was filtered by Gage "Daylight" glass.

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[16] A similar—and similarly indeterminate—remark is made, with regard to recognition, by E. Rubin: *Visuell wahrgenommene Figuren*, i., 1921, 27.

The *Os* in this preliminary experiment were: Bs, By, Du, Gn, M and Sk.

The instructions, given orally by *E*, were: "On the wall behind you is a jar. When I say 'ready-now', you are to turn and face this jar; when I say 'ready-now' again you are to turn again and tell me how the color of the disc compares with that of the jar, and I will change the disc to make it more like the jar. Continue to do this until you are sure that the color of the disc matches that of the jar." *E* threw the switch so that both lights were on while *O* was turning, but only Light 2 when he faced the jar and only Light 1 when he faced the disc. Every inspection, of jar and of disc, was of 5 sec. duration. Only one satisfactory match was required in every observation period, but the *O* was warned that this match should be exact for hue at least, and for tint and chroma also if the judgments were not too difficult.

To divert the critical attitude, necessitated by the requirement of an exact match, from the jar (whose color, until the test experiments, remained constant) and toward the disc (whose color changed continually) the *O* was always instructed to look from the jar to the disc, but was never allowed to look directly from the disc back to the jar. The changes in the color of the disc were made in serial order. In the first 5 observation periods, the series were short and the changes were made by units of 2°. After this, the series were all longer, their end-points far from the probable match of the *O*, and the size of the steps was gradually reduced from 15° or 20° to 2°. If, anywhere in a series, the *O* gave a doubtful judgment, he was not allowed, as stated above, to look directly from the disc to the jar as he wished, but was made to wait a few seconds, while *E* made some apparent adjustment of the motor, and was then instructed to look again from the jar to the disc. If he gave a second doubtful judgment, *E* changed the color back a number of degrees and began a new series. If a satisfactory match was found in a comparatively short series, *O* was asked if he would like to 'begin' another series. By means of these 'beginning' series and the apparent blunders which she made in adjusting the colors of the disc, *E* arranged that every *O* should see the jar approximately 40 times in every observation period. Every *O* observed 3 times a week for 25 observation periods; every *O* therefore saw the jar approximately 1000 times in the "Daylight" illumination.

In order that the *O* should maintain the attitude of the object-consciousness established by requiring him to make only one satisfactory match of the jar in every observation period, *E* talked to him about the jar while he was observing, and toward the end of these preliminary experiments made a thin irregular scratch on one of its sides.

The last of these preliminary experiments was completed three weeks before the *Os* observed in Test Experiment 1.

Preliminary Experiment B

The stimulus-object was a 'peacock blue' silk waist; the illumination was the same same as in Preliminary Experiment A; and the method was similar to that used in that experiment. The *Os*, who were N and Re, observed three times a week for two months, and each saw the waist approximately 1000 times. The last of these preliminary experiments was completed nine months before N and Re observed in Test Experiment 2.

Test Experiment 1

The stimulus-object was the jar of Preliminary Experiment A. Light 1 still contained the 200 watt bulb and the Gage glass, but Light 2 contained a 50 watt bulb and its light was diffused by a plain ground glass. The intensity of the illumination of the two lights was the same, but Light 1 still gave a "Daylight" illumination, while the illumination of Light 2 was yellowish.[17] The jar was the only object in the room which could show the effect of this yellow illumination.

The procedure was the same as in Preliminary Experiment A. The end-point of the series had been judged "too blue" in the earlier experiments.

The *Os* were those who had observed in Preliminary Experiment A. After this experiment they again matched the color of the jar in the "Daylight" illumination for three observation periods before they observed in Test Experiment 2.

Test Experiment 2

The stimulus-object was a jar, identical in size and shape with the jar of Preliminary Experiment A, but made from Hering grey paper no. 37. Light 1, with 200 watt bulb and Gage glass, was on during the entire experiment, Light 2 remained off.

E told the *Os* of Preliminary Experiment A that she wished to find out how well they could see the color of the jar when Light 2 was turned off, and gave them this instruction: "When I say 'ready-now', you are to turn and look for the jar, which will be in its usual place. As soon as you can distinguish its contours, turn back to the disc and tell me how its color compares with that of the jar and I will change it, as I did in the other experiments, until you have found an exact match." The clause about the contours was inserted for two reasons: (1) to insure the *O*'s seeing an object, (2) to divert any criticalness of attitude, occasioned by the novel situation, from the color and to the contours. The instructions given to N and Re were like those given to the other *Os*, except that *E* had first to describe the shape of the jar for them.

The procedure, except that the switch was not thrown, was identical with that of Preliminary Experiment A, and the series was one whose end point had been judged "too red" in these earlier experiments.

Control Experiments: Experiment 1

The object was an egg-shaped figure of approximately the same size as the jars. The illumination was that of Test Experiment 2.

Experiment 2

The stimulus-object was the jar of Preliminary Experiment A, and the illumination was that of Test Experiment 1.

Bi and Ho were the *Os* in each experiment, and in each they were asked to make 10 satisfactory matches of the color visible in that illumination. The procedure for Control Experiment 1 was the same as that of Test Experiment 2, and the procedure of Control Experiment 2, except that the *O* was allowed to look directly from the disc to the object, was like that of Preliminary Experiment A. In both of these experiments, however, the *O* made more than one satisfactory match in an observation period.

After the completion of these experiments, the matches of the various *Os* under the various illuminations were shown to Bi and 4 students in the laboratory. They were asked to name the colors in terms of the color pyramid, and then, if possible, to give them objective names. The psychological term and the most appropriate objective name are included under the 'quantitative' results.

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[17] We wish to thank Professor S. H. Gage, of the Cornell Medical College, for suggesting this method of varying the quality without varying the intensity of the illumination; also for helping *E* to grind the plain glass.

Results: A. Quantitative

(*1*) *"Daylight" illumination*

(*a*) Averages of the 25 matches of the *O*s of Preliminary Experiment A:

O	Red	M.V.	Blue	M.V.	Black	M.V.
Bs	248.88	±5.03	97.16	±2.49	13.96	±4.83
By	248.76	±6.43	96.92	±5.28	14.32	±5.05
Du	246.44	±6.21	96.08	±2.90	17.48	±6.22
Gn	251.6	±2.4	95.84	±1.79	12.56	±2.3
M	247.28	±4.0	96.92	±2.48	15.80	±3.79
Sk	247.2	±2.96	97.48	±3.26	15.32	±3.56

These matches were all red purples or "blue fuchsias."

(*b*) As the arrangement of the apparatus in Preliminary Experiment B was slightly different from that of all the other experiments, numerical results can not be given; but both N and Re described the color which they saw 1000 times as a blue green.

2. *Yellow illumination*

Averages of the controls and matches of the *O*s of Preliminary Experiment A.

O	Red	M.V.	Blue	Black	White	M.V.	Yellow	M.V.
Bi	351.7	± .62	0	0	1.3	±.96	7.0	±1.0
Ho	352.4	±2.32	0	0	0		7.6	±2.32
Bs	330		15	15				
By	332		0	20			8.	
Du	310		30	20				
Gn	298		56	6				
M	335		10	15				
Sk	330		15	15				

The matches of the controls and of Bi were red oranges or "tangerines." Those of the other 5 *O*s were all red purples; Gn's was a true fuchsia, Du's a red fuchsia, and those of Bs, M and Sk were like the color of the flower of the "Indian pink."

3. *Lowered illumination*

Averages of the controls and the matches of the *O*s in each preliminary experiment:

O	Red	Blue	M.V.	Black	M.V.	White	M.V.	Green	M.V.
Bi	0	18.0	±3.6	288.5	±7.2	47.2	±5.0	6.3	±3.1
Ho	0	21.3	±3.16	283.8	±7.16	36.4	±4.8	18.5	±3.1
Bs	135	52		173					
By	122	80		158					
Du	0	0		335		25			
Gn	78	32		232		18			
M	16	90		204		50			
Sk	20	30		285		25			
N	0	32		265		18		45	
Re	0	28		292		10		30	

The matches of the controls and of Du were greys.[18] The matches of Bs and By were blue fuchsias; Bs's differed only in tint and chroma from her matches in the preliminary experiments, By's was slightly bluer. Gn's match was still bluer and more of a plum color. The matches of M and Sk were lilacs. The matches of both N and Re were poorly saturated "peacock blues."

B. *Qualitative*

Of the 5 *Os* who served in Preliminary Experiment A and whose matches for the grey jar showed the presence of a memory color, Bs alone reported that the color which she saw in the lowered illumination persisted during the entire observation. She did not once doubt that she was seeing the familiar jar, or that the color which she matched, despite the fact that it "never got all the way out to the edges," was visible in the dim light. The only change which she noted was that, while she looked at it, "it shrank in from the edges and then spread out again."

By, Gn, M and Sk all reported that the color which they saw did not cover the entire jar; and although they also did not doubt that they were seeing the familiar jar, they reported that the color was instable, and that its instability interfered with a successful matching of it. They all voluntarily described this fleeting color. The spontaneous reports of all 4 *Os* included characterizations of size, form, texture, and mode of disappearance. We quote their reports under these characterizations.

Form and size.—By "Doesn't have contours of that thing at all. I can't say how big it is, but it seems to be in the upper left hand corner where I used to look when I matched it."

Gn "Color must be over the entire jar, but I can see it more distinctly in the center than around the edges."

M "Hasn't any shape; can't say anything about its size."

Sk "Red predominated toward upper right hand corner, just around the border."

Texture.—By "It's filmy."

Gn "Like looking at grey through a mist. The mist is darker than the object." "As if you could color gas and have it so finely mixed that you couldn't see puffs in it." "A filmy appearance."

Mo "The color is filmy, airy, like a mist at night; foggy, not regular like light thrown from a lantern; a film." "The grey is more impenetrable than the color. The other is thinner than this. The grey is like a surface that I can see into."

Sk "The red is filmy; flamelike."

Disappearance.—By "Sometimes it is just a flash. Sometimes it stays a perceptible length of time." "When I first turned around it was almost normal, but it faded to a dark purple." "The fading takes some time, but cannot be measured in seconds. It gives the impression of fading rather than of snapping, like pulling a string of an electric light quickly, so that the light goes off quickly, but you are aware of decreasing intensity."

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[18] The white paper used in this experiment, the Milton Bradley glazed white, was slightly orangish; hence the blue and green needed by Bi and Ho to match satisfactorily the Hering grey.

M "It is gone as soon as I see it, as if it were fading away as I see it. I just see it going. Fading is like the vanishing of a mist, but the stuff is so vague that it can't really vanish, it fades."

Sk "Red goes. Going is not sudden but gradual; wave-like. Doesn't take a long time. Easy movement, nothing startling or sudden, still it doesn't lag."

According to their own reports, the instability of the color which By, M, and Sk saw when they first turned around accounted for the predominance of blue in their matches. According to these reports, in the fading of the color the red went out before the blue, so that the matches were for the color which had already changed from its original appearance toward blue.

By (reports given after his first match, which was made from the more stable color) "I got it fine when I first turned around. Absolutely certain of that patch of color. Could have matched it again, if I could only have got it consistently."

Mo "Just as I got around there was a mist just as pale as could be. The color was like the original, but I couldn't match it because the grey came in and made it darker. The mist is blue but looks pink. The pink wabbles and changes."

Sk "Red purple like standard. The red goes out and so does the blue, but the blue seems more stable. Can't match red because I can't find it when I look for it."

In this experiment, because of memory color, impressed through long familiarity, a jar, whose untransformed color was "tangerine," was seen by two *O*s as fuchsia and by three as the color of an "Indian pink"; and a jar whose untransformed color was grey was seen by two *O*s as fuchsia, by one as plum color, by two as lilac, and by two as peacock blue. All of these colors were objectively measured.

The spontaneous descriptions of the four *O*s, who noticed that the color which they saw did not cover the entire jar and was instable, indicate that, in its ultimate psychological nature, the memory color is a film.

IV. CONCLUSIONS

When we first began our task of giving to the transforming images, typified by the memory color, their systematic setting, we knew them only as the surface colors which Hering and Katz described and the tones and odors which Stumpf and Henning mentioned; and our only tools were the conditions of appearance stated or implied in the discussions and instances of Hering and Katz. We found, however, that the range of the transforming image, which we now called the imaginal overlay, was not limited to the surface colors, tones and odors of very familiar objects, but that it included the qualities in all sense modalities, attributes other than quality, and such perceptive characters of experienced objects as size, form, texture and position. Indeed, it became more and more apparent, as we proceeded, that the imiginal overlay, in some form or other, plays a not unimportant part in most, if not all, of our everyday perceptions.[19] To learn just what this part was, just how well the overlay played it, and under what conditions it played it best, we adopted two courses: (1) we studied instances of the overlay as we found them occurring in every day life, and (2) we sought, under controlled conditions, to examine carefully the overlay itself. As we went on the conditions, hardly more than latent in the discussions of Hering and Katz, became continually clearer; and we were finally able to define them as: (1) the object-consciousness, (2) an uncritical or casual attitude toward the transformed character of the perception, and (3) familiarity with the perception.

The means of discovering our casual instances were three: (1) an accidental change in a perceptive experience, (2) a doubting of a perception which caused a change in its experience, and (3) a realization of the inadequacy of a perceptive experience to the meaning of the perception. It soon become evident that a mere cataloguing of these instances under rubrics representing their means of discovery was inadequate to telling all that could be known about them; and that to bring them into systematic relation several cross-classifications were necessary.

The first of these bore upon the immediacy of the familiarity (which need amount to nothing more than the lack of a feeling of strangeness) of the perception; the second, upon the relation between the meaning (or meanings) of a perception, the differences in the critical attitude, and the resulting transformation; and the third, upon the adequacy of the overlay itself. We could assign to its proper place in these three classifications every casual instance discovered by the three methods indicated above, and every introspective report given in the controlled experiments.

Our experimental search took us into two different paths; the first led to a study of the memory color transforming the five natural objects, grass, snow, coal, gold, and blood, which Hering and Katz had mentioned; the second to an investigation of the overlays in the perceptions of the various sense modalities. Because an attitude critical enough for equating their colors dispelled the overlay from the five natural objects, a quantitative measure of their memory colors was impossible;

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[19] We may instance a discovery made by one of our *O*s in the course of this investigation. The *O* in question works with 'near' or 'reading' glasses. In the familiar surroundings of his study he is able to read across the room the titles of familiar books and to mark and respond to the changes of expression on the face of a familiar visitor—until the need of careful scrutiny arises: then he at once becomes aware that books and face are mere indistinguishable blurs, and automatically changes his glasses. The imaginal extension of 'near' vision was not noticed until the *O* was asked to look out for cases of imaginal overlay.

yet the results of the first five experiments proved that in the seeing of the familiar objects the presence of a memory color is common and that it plays a dual rôle. (1) If the visible area of the object is fairly large and under an observational attitude would be mottled, the memory color causes it to appear uniform and in the best color visible under the observational attitude. (2) When the visible area is small and, though uniform, would under the observational attitude be weak in chroma and altered from the normal in hue and tint, the memory color enhances the chroma and corrects the defects in the hue and tint of the entire area.

The investigation of the perceptions of the other sense modalities not only substantiated the fact, first disclosed by the casual instances, that in these perceptions there are images exactly analogous to the memory colors of the visual perceptions, but it also gave us better phenomenological descriptions of the totally and partially adequate overlays. Furthermore every experiment either extended or refined our knowledge of the conditions of the overlay's appearance. The smell experiment showed that the extremely critical attitude, the sceptical attitude, completely ousts the overlay. The tonal experiment indicated that even a modified form of the critical attitude, a careful attending for the overlay, is unfavorable to its appearance. Yet the very next experiment, that dealing with taste and smell, demonstrated that a strong feeling of familiarity can prevent this careful attending from completely (or even partly) dispelling an averlay. This experiment moreover proved conclusively that one impressive experience with an object is sufficient to make it familiar. The results of the experiment with lifted weights took a slightly different turn; they showed that, even though the three essential conditions are favorable, the overlay appears more readily when the difference between it and the untransformed experience is not too great. The results of the touch experiment verified the results of all preceding experiments, afforded more detailed descriptions of partially adequate overlays and their usual manner of disappearing or fading and made possible the construction of a chart which demonstrated that the closer the similarity between the required overlay and the untransformed experience the greater is the probability of the appearance of this overlay.

For a complete story of the overlay, only two things were now lacking: first, an objective record of a memory color, and secondly a psychological description of it. Our last experiment supplied both needs; it gave us quantitative measures of strikingly different overlays which transformed the same presented object, and revealed the fact that in its ultimate psychological constitution the memory color proper is a film.

THE PRINCIPLES AND TECHNIQUE OF MENTAL MEASUREMENT

By TRUMAN L. KELLEY, Stanford University

This contribution has been in part inspired by two articles written by E. G. Boring,[1] and would have appeared earlier had not difficulties in connection with the second part of my treatment caused delay. Boring's conclusions are generally destructive, and tend to leave one with the feeling that there is no sound statistical basis for mental measurement, and little for other psychological measurement. My treatment readily falls into two parts: a criticism of Boring's negations, and a specification of certain things which I consider to be essential in a sound statistical procedure as applied to mental measurement. I shall quote freely from Boring's articles, referring to the first as "Math." and the second as "Logic," but a reading of them entire in connection with this paper is urged.

PART I.

If two series of measures are homogeneous, *i.e.*, if they measure the same basic phenomena, there will of necessity be much in common between them. Thus if 1000 reaction times of a certain individual give a certain distribution, and if a second 1000 by the same individual are obtained, under conditions as nearly similar to the first as possible, we shall expect the frequencies in similar categories to be approximately the same in the two series, if they are truly homogeneous. If these frequencies are quite different we must conclude that, owing to practice, fatigue, or other unanticipated and unmeasured cause, the series are not homogeneous.

Pearson has defined the statistical treatment for making the comparison between two series such as these, each of which is influenced by random errors. The treatment follows that of the more elementary problem—the comparison of a series influenced by random errors with a standard invariable series—which likewise involves the basic question of the significance of a difference. We shall consider the simpler case. Let it be given that individual A makes the record indicated in the third line of the accompanying table, and let it be required to determine how significant is the difference between this record and the standard record given in the first line of the table.

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[1] Mathematical vs. scientific significance, *Psych. Bul.*, Oct., 1919. The logic of the normal law of error in mental measurement, this JOURNAL, Jan., 1920.

Reaction Times	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*j*	Sums
Given standard distribution	1	19	28	25	13	7	3	2	1	1	100
Obtained distribution of reaction times of individual A	0	12	28	30	14	4	5	4	1	2	100
Cell divergence	−1	−7	0	5	1	−3	2	2	0	1	0
Cell square divergence	1	49	0	25	1	9	4	4	0	1	
Cell square contingency	$\frac{1}{1}$	$\frac{49}{19}$	$\frac{0}{28}$	$\frac{25}{25}$	$\frac{1}{13}$	$\frac{9}{7}$	$\frac{4}{3}$	$\frac{4}{2}$	$\frac{0}{1}$	$\frac{1}{1}$	10.275

Square contingency = χ^2 = 10.275   No. of cells = 10

From Elderton's tables P = .330 when χ^2 = 10.275 and the number of categories is 10. Note that numerical values for the reaction times have not been needed. Thus *a*, *b*, *c*, etc. is not a quantitative series; it is not even an ordered series; all that we may definitely say of *a*, *b*, *c*, etc. is that they are distinct categories or cells, and in their entirety cover the whole range of reaction times. Of course, if the series is an ordered series, or if it is a quantitative series, the contingency treatment given above is in no way altered. The calculation of χ^2 is simple as shown, but its meaning is complex because its value depends upon two factors, the similarity between the observed and the standard series, and the number of cells. In order that the number of cells may not be a factor in the interpretation Pearson[2] has developed a "goodness of fit" method, involving "P" which is obtained from χ^2 and the number of cells.[3] The meaning of P is stated by Boring to be the "probability that the difference is not due to chance." In more detail we can say that P is the probability that a random sampling of N measures of the distribution represented by the standard series will yield as great a divergence from the standard series as that given by the obtained series.

What assumptions are involved in the derivation of the measure P? The answer to this question is important, as it accounts for much of the difference in point of view between Boring and myself. (See especially p. 418 of this article.) I can discover but one assumption: it is that, if the obtained series is in reality a random sampling of the series given by the standard, then the random distribution of each cell frequency around the standard cell frequency as mean is given by the normal law of error. To illustrate: the standard cell frequency for cell *d* is 25. It is assumed that successive random samplings of 100 measures, each of the same basic data as represented by the standard, will yield sometimes more and sometimes less than 25 in cell *d*, and that the distribution of the numbers yielded will give a normal distribution with 25 as its mean. There is at times an error in this assumption, which Pearson has recognized, and that is that, if the theoretical call frequency is very small, it is more reasonable to assume a skew binomial distribution than a normal distribution to represent the distribution of cell frequencies resulting from successive samplings. Pearson therefore objects to the use of this method, if such small theoretical cell frequencies cannot be avoided. As to just what constitutes a small theoretical cell frequency, in the sense that the error thereby introduced into P is material, that is not an issue raised by Boring. If raised it could be treated numerically, as the difference between skew binomials and normal distributions is well-known; and I venture to say that it would lead to substantially the same conclusions as reached by Pearson. This particular, and minor, shortcoming of the measure P is therefore not the point at issue. Boring does object to placing reliance in the measure P, though, as I shall show, he is not consistent in this in as much as he later recommends its use. I quote (*Math.*, p. 336) with the purpose of discovering the nature of Boring's objections.

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[2] On the criterion that a given system of deviations from the probable in the case of a correlated system of variables is such that it can be reasonably supposed to have arisen from random sampling, *Phil. Mag.*, July, 1900.

[3] W. Palin Elderton, Tables for testing the goodness of fit of theory to observation, *Biom.*, V., 1, 1902.

"It is a common experience of scientific persons working with human data that these formulae frequently give values for the probability of differences that are 'too high.' One works, for example, with the performances of a group of women and a group of men in a mental test and one finds a 'significant' difference,—perhaps a probability that 99 times out of 100 the men will do better than the women,—and yet one is convinced that there is no 'truly significant' difference indicated. Or one determines the deviation of an observed curve from an ideal form and finds, let us say, that only 2 times in 100 would data that tend to follow the ideal form deviate as much from the ideal as do the observed data; and yet in plotting the observations along with the theoretical form he may note that the two functions are sensibly the same, and may feel inclined (if he is not scared off by Mr. Pearson) to say that the ideal function actually does represent his data. It is with the basis for this particular scientific attitude that I am concerned.

"It appears that the apparent inconsistency between scientific intuition and mathematical result is not due to the unreliability of professional opinion, but to the fact that scientific generalization is a broader question than mathematical description."

If we "feel inclined" to draw a conclusion at variance with the data, that settles the matter. In this paragraph quoted Boring reveals a conviction which permeates his article. For a conviction to precede an argument is good scientific procedure; but the merit of the conclusion depends upon the rectitude of the argument; and certainly before an argument is made it is unsound to say that "it appears . . . that scientific generalization is a broader question than mathematical description." Nothing of the sort "appears" at this stage, or in my opinion at a later stage, of the argument.

I would be willing to defend the proposition that "scientific quantitative generalization is not and cannot be broader or more exact than the mathematical statement." I have added, for exactness, the word "quantitative," and this word should have appeared in Boring's statement, as he is here specifically discussing a quantitative measure of probability. To quote further:[4] after a discussion of sampling Boring states (*Math.*, p. 337):

"So it happens that the competent scientist does the best he can in obtaining unselected samples, makes his observations, computes a difference and its 'significance,' and then—absurd as it may seem—very often discards his mathematical result, because . . ."[5]

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[4] *Math.*, p. 336, footnote: "The differences between Pearson on the one hand, and Merriman and Airy on the other, *Phil. Mag.*, *op. cit.*, 171 ff., are of this order; Pearson is statistician, and Merriman and Airy scientists. It is interesting to find Pearson shifting ground in *Biometrika*, 2, 1903, p. 367, from a statistical result that lengths of forearm do not fit the Gaussian curve to a scientific conclusion that they do." I cannot pass this over without objecting. A very careful reading of the p. 367 leads me to an opposite conclusion, and I commend the citation to the critical reader.

The procedure described is such as I believe a competent scientist never resorts to. If he will not trust such mathematical findings as are contrary to his wishes, that fact in no sense provides scientific warrant for discarding them. "Absurd" hardly characterizes this procedure. Is it not rather a matter of the honesty of the investigator? The intent of this quotation seems to me to be to give warrant for keeping and using data if they support an established conviction and otherwise discarding them. I do not believe that Boring would do or defend this, and I should be much interested in learning his present view. The preceding quotation does not stand alone, and the following scarcely improves the situation (*Math.*, p. 337):

"Since in the nature of the case it is impossible for him (the scientist) to state in numerical terms the degree of representativeness that his samples possess, conclusions must ultimately be left to the scientific intuition of the experimenter and his public. Such an outcome with respect to the measure of the probability of difference is not wholly satisfactory, but it is inescapable."

Have we returned to "intuition" as a method of proof? I think not; nor in fact do I think Boring has whole-heartedly done so; for it will be obvious from quotations that he himself later (see quotations on p. 417) advocates a mathematical means of proof.

I have quoted extensively from this early article and with a part of it I agree,—particularly with the last sentence: "The case is one of many where statistical ability, divorced from a scientific intimacy with the fundamental observations, leads nowhere." If Boring will interchange the words "statistical" and "scientific" I will still agree in this matter of quantitative scientific investigation.

In turning from this earlier article to the later we find the same distrust of certain, I fear none-too-well appreciated, statistical procedures. I am well aware that I do not appreciate as one should the subtle and important logical foundations of statistics, but that does not prevent my being concerned over the dangerously large mote in my fellow psychologist's eye. With reference to "the logic of the normal law of error in mental measurement" I contend(*a*) that Boring has looked upon the logic of probability as altogether deductive and has quite failed to realize its inductive bearing, and that as a consequence "probability" can no more seem adequate to him as a means of handling ordinary psychological problems than would any other purely deductive treatment; (*b*) that certain striking phrases such as "knowledge cannot be wrought out of ignorance" and "the equal distribution of ignorance" and "cogent grounds for assuming" have so appealed to Boring and laid hold of him that they have been used in improper senses and connections to the obscuration of the argument; (*c*) that Boring has made a very material mathematical error: in his claim that, in the sense presented, the psychometric function is a function of the unit of measurement; and also (*d*) in his failure to realize that the assumption of normality which he condemns is involved in the very methods which he advocates. As a consequence of the foregoing I contend that the limitations which Boring ascribes to much of statistical procedure are unwarranted and that the appropriate field of statistics is much broader than he pictures.

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[5] I have closed this quotation in the middle of a sentence, because so doing does not change the meaning with reference to the point I make, and because the entire sentence seems to me to be confusing. The rest of the sentence is ". . . in his judgment the mathematically 'significant' difference is nevertheless not large compared with what be believes is the discrepancy between his samples and the larger groups which they represent."

Before taking up these four points in order I must admit a difficulty in getting consistent meaning from all that Boring writes. For example, we find (*Logic*, p. 14):

"We remarked that Pearson in 1894 gave evidence of being influenced by the sanctity of the normal law. His faith was not great for he noted its limitations in 1895*. In 1909 the faith was gone.†"

And again (*Logic*, p. 25):

"This quotation is as near a confession of what the Biometric School is trying to do as we are likely to get. It is frankly seeking always to see Gauss in nature."

Either quotation taken separately would be understood as expressing Boring's belief; but taken together, which one should be at liberty to do, they produce a certain disquietude.[6] This peculiar unawareness of Boring's right hand of the commitments of his left cannot at times be so easily dismissed as in the preceding instance, as it is more material to the argument. This is important in connection with my first point (*a*).

(*a*) Boring quotes Arne Fisher (*Logic*, pp. 4 f.) to the effect:

"It thus appears that a rigorous application of the principle of cogent reason seems impossible. However, a compromise between this principle and that of the principle of insufficient reason may be effected by the following definition of equally possible cases, viz.: Equally possible cases are such cases in which we, after an exhaustive analysis of the physical laws underlying the structure of the complex of causes, influencing the special event, are led to assume that no particular case will occur in preference to any other."

Dr. Fisher is an authoritative exponent of the deductive school (with some exceptions and qualifications, the Continental school) of statistics. In line with this point of view we find Boring writing (p. 2):

"We can not say that *a* is twice as likely to happen as *b* because it happens twice as often, for this is simply to reason from frequency to probability *and ultimately to identify the two.*"

Also in line with it (*Logic*, p. 6):

> "Let us state the case in roughly syllogistic form.
>
> The tossing of a penny gives, when indefinitely repeated, 50% heads;
> The event in which we are interested is a tossing of a penny;
> The event in which we are interested gives, when indefinitely repeated, 50% heads.

This leads us to predict what will happen to our particular penny, if it is tossed repeatedly; and, what is more important, it defines for us the place of cogent reason and the sphere of ignorance. We must know what a penny does when tossed again and again, either directly from observation or indirectly by inference from other observations. The major premise must be based upon cogent reason and *there is no limit to the exactitude of the knowledge.* Ideally the major premise may be considered to be indubitably established. Moreover we must know that the event in which we are interested is the toss of a penny in the sense of that term in the major premise. Our minor premise, by 'an exhaustive analysis of the physical laws underlying the structure of the complex of causes influencing the special event,' *must also be established and without limit of accuracy by cogent reason.* Of what then do we remain ignorant? Of the manner in which the particular, in the middle term, is subsumed under the general." Boring seems aware that this is not the only point of view which statisticians have taken, as I will immediately show by quotations, but this is the specific point of view which he criticises. Personally I cannot defend probability as having rigid, or rather universal, deductive warrant and I am glad that I cannot, as the method is much broader than this one logical procedure. Immediately preceding the previous quotation we find:

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* *Philos. Trans., loc. cit.*

† *Philos. Mag.*, 5th ser., 50, 1900, 173.

[6] I may mention my personal conviction, not based upon an actual count, that Pearson assumes normality in the majority of his developments, but, except where made a major and admittedly moot issue, it is an assumption of normality of errors of observation or of errors in frequencies in the case of random sampling, and not an assumption of normality of form of distribution of mental or biological trait.

"In the initial instance this fact (that heads come as often as tails when tossing pennies) must be observed. Thereafter, we may reason that other homogeneous, one headed, well-tossed pennies will in the long run, come heads fifty per cent. of the time, and we may even go further and reason that a homogeneous, six-faced, well-tossed die will in the long run come up equally often on every face. What we do is to assume probability on the grounds of observed frequency and its conditions."

There is also given a footnote upon this page (*Logic*, p. 5) citing Edgworth, Pearson and Westergaard as approving the assumption of probability upon the grounds of observed frequencies. After admitting, as Boring here does, that we assume probability on the ground of observed frequency I see no consistency in maintaining the position represented by the italicized portions (italics mine) of the preceding quotations. To deny the right to identify frequency with probability (*Logic*, p. 2) and then to do so (*Logic*, pp. 5f.) would present serious difficulties in Boring's argument were it not for the fact that he seems immediately to forget the second (*Logic*, pp. 5f.) point of view.

This identification of frequency with probability necessarily enters into social statistics. It is the inductive element. It belongs in the process and is not, as Boring implies in the sentence (*Logic*, p. 8) "At this level common sense takes care of the theorist," an outside factor in the shape of "common sense" which comes to the rescue. I know of no single practical problem in insurance, enonomics, education, psychology or biology where earlier observed frequencies have not been taken as the guide to later expectation, and this is inherent in a sound statistical procedure. Even the Mendelians take an observed ratio of, let us say, 53:47 as suggestive of 50:50 and not of 75:25. The pure inductive treatment would be to take such a ratio as presumptive of 53:47, and to refine or correct the assumption or expectation as more evidence is gathered. This assumption is never "indubitably established" and it never can be, as exactly comparable situations at two different periods of time cannot be guaranteed. It is purely theoretical to consider such a situation. Part of the inexactness of the earlier discovered frequencies, as indicative of frequencies operating in general and for all time, is discovered by the later experiment, and thus a second and more accurate concept of the frequencies in the various categories is obtained. Thus is truth gradually wrought out of ignorance.

The syllogistic statement in the quotation given on a preceding page is not typical of the ordinary problem, which may be stated:

1. The sum total of our previous, though admittedly fallible experience, suggests that under certain conditions fact A occurs x per cent. of the time and fact B (100-x) per cent.

2. The conditions with which we have to deal seem to be the required ones.

3. In this present experiment fact A does occur $(x+\delta)$ per cent. of the times and B $(100-x-\delta)$ per cent., which, in the light of the size of the probable error of δ, suggests a revision, slight or great as the case may be, of the major or minor premise, or of both.

If Boring objects to this statement of the case I must reply that I cannot argue with reference to a situation where the "major premise is indubitably established", or to one in which the minor premise is "without limit of accuracy." Such a situation parallels no practical problem of statistics.

We remain as ignorant after the experiment as before "of the manner in which the particular, in the middle term, is subsumed under the general." To remove this particular kind of ignorance has not been the purpose of the experiment, which in general has been to increase the state of knowledge of the premises. When the first and second premises have become so well established that a new investigation suggests insignificant revisions of them, we have attained the certainty sought in our problem, whether it be in mental measurement or some other social field. I could cite numerous illustrations of this particular procedure in mental measurement studies, and cannot cite one of the type of Boring's syllogistic statement.

(*b*) The doctrine of the "equal distribution of ignorance" as it has grown out of the purely deductive concept of probability has, in the hands of some, led to such excesses as the determination of the number of elements in Sirius. I can no more defend this than I can Quetelet's (and I regret to say that he is not alone) assumption of normality of distribution to prove normality of distribution. There is, however, a meaning in which the expression is serviceable. Let us suppose (1) that all the information available (say a brief previous experiement or a number of casual observations) points to 50 per cent. of A's and 50 per cent. of B's, and (2) that as nearly as we can tell the present events are similar to the preceding; then (3) with reference to any single additional measure our ignorance is equally distributed, since we have no criterion as to whether the measure will be an A or a B. Under these conditions to assume a probability of 0.5 for A is not illogical, though of course the assumption may be wrong as subsequent experiment might show.

Under these conditions are there "cogent reasons" for the assumption? To "assume," according to Webster, is "to take for granted as the basis of argument." Thus an assumption is an hypothesis or proposition to be accepted tentatively and tested later. An hypothesis is a guess, and fundamentally an assumption is also. "Cogent" means "powerful," "strong," and as used by Boring and further defined by him, "there is no limit to the exactitude of the knowledge." Thus, in substance, *we have reasons for our guess so powerful that there is no gainsaying them.* The very statement reveals the inconsistency, for an assumption so fortified is no longer an assumption but a certitude. Boring has so defined "cogent" that there is no place left for "assumption," and that is exactly the point he makes; that there should be no "assumption." This position is untenable, as every act of life testifies. The bases of conduct are probabilities, not certainties, and the scientific procedure is to measure and express these probabilities, continually assuming the more and more probable as the basis for future expectation. Cogent "without limit to the exactitude of the knowledge" is without a parallel in reality, and to get any meaning at all I must discuss degrees of cogency and degrees in the reliability of assumptions. In every case the probable error in the assumption increases as the cogency decreases, and *vice versa*. But a little thought will convince one that there is an assumption-aspect in every act of voluntary (including experimental) life, and a theory of statistics that did not parallel this would be useless. The real problem, then, is ever to decrease the violence of the assumption, and secondly, when either one of two assumptions will serve a given situation, to choose the more probable. Fortunately statistical method does not depend for its warrant upon such cogency as Boring advocates, and an assumption based upon the best obtainable evaluation of preceding experience is a sufficient starting-point in the process of increasing knowledge at the expense of ignorance. Thus, to answer the question put at the beginning of this paragraph, I should say the reasons were sufficient for the purpose.

(*c*) If the distribution of the heights of a number of individuals, expressed in inches, gives a normal curve with mean M_1, and standard deviation σ_1, we shall still have a normal distribution if the heights are expressed in centimeters, though the mean M_0 and the standard devation σ_0 will have numerical values which are different from M_1 and σ_1. Again, if we measure the heights from a table top three feet above the floor upon which the individuals stand we should obtain measures giving a normal distribution. Thus any linear transformation of the scale does not change the form of distribution. This is fortunate for, if a biological unit (possibly the diameter of some elemental cell) exists, it is only necessary that the scale of units used be related to it by an equation of the sort

$$y = a + bx$$

in which a and b are constants, x the number of biological units, and y the number of units in the scale used, in order that the form of distribution shall be the same whether the biological or the other units are used. It may be beyond likelihood to ascertain the biological unit and measure upon a scale of such units, though quite possible to determine a scale linearly related to it. This relationship between units holds whether the form of distribution be normal or otherwise; thus a Pearson Type III curve remains a Type III curve if another scale linearly related to the first is employed.

Boring's reference to the "all important matter of the unit" does not make it clear that the very great freedom in units here indicated is permissible. In fact Boring seems to recognize this fact in one part of his paper and to deny it in a second. He states (*Logic*, p. 15): "Suppose then that nature does conform to the normal law; we shall not in practice be fortunate enough to obtain normal curves of distribution unless we have chanced to use nature's own unit of measurement, or some unit directly proportional to it."

The statement "directly proportional" implies an equation of the sort

$$y = bx;$$

whereas the more general relation

$$y = a + bx$$

is sufficient. However, even the freedom permitted by the use of proportional units is brought into question on p. 23 and, as I read it, actually denied on p. 27. We find (*Logic*, p. 23):

"When we change from stature to the forearm we have no guarantee that the inch means the same thing biologically. And we must use a biological unit, if we are to bind our class together by a biological concept, if we are to predicate the normal law of all *human* characters. That the form of distribution is a function of the unit is unescapable." This last sentence (strengthened by the first) seems to me to be an assertion that the particular unit used must be identically the biologic unit if we are to obtain a concept as to real biologic form of distribution. Again (*Logic*, p. 27):

"To say that the phi-function of gamma is the psychometric function is equivalent to saying that the dispositional variations of the psychophysical organism, when measured in a scale of units proportional to the scale in which the stimulus is measured, follow the normal law. If the lifted weights are measured in grams and the psychometric function approximates the phi-function of gamma, then the organism varies in the amount of its disposition for judging 'heavier' or 'lighter' according to normal law, provided always that the amount of this disposition is measured in grams. This relationship is scarcely obvious and the reader may need to read elsewhere before he can accept it."[7]

The first sentence in this quotation is none too clear to me, but it seems irreconcilable with the second. But let us continue. In a footnote on the same page we read:

"Keller used Wundt's fall phonometer, and the all-important unit of stimulus, which is only just mentioned, is the cm. The psychometric functions of the lifted weights are for grams."

Here again are not the words, "provided always that the amount of this disposition is measured in grams" a positive assertion that one may not use ounces, pounds or kilograms? Is not "the all-important unit of stimulus is the cm." an assertion that inches would not have served? I cannot understand why Boring should make these statements unless he thinks that the situation here, psychometric functions, is upon a different basis from the earlier situation, frequency distributions. In dealing with distributions he surely knew that a proportional unit of measurement would suffice. As further evidence that he was aware of this point I would mention that his own illustrations of changes in units which change the form of distribution or change the form of the psychometric function are all non-linear transformations, being in fact of the type

$$y = x^2$$
$$y = \log x$$
$$y = \sin^2 (x/2).$$

Again, on p. 28 we find:

"In the case of the 'introspective' psychophysics we are not to expect a generalization, because everything depends on the unit."

This, as it stands, and doubly so in connection with the preceding quotations, conveys a very wrong impression. Whereas it is quite beyond our present prospects to isolate a particular biological mental unit, it is rather more conceivable that, in fields where there are such natural units that play a part in life, they will manifest themselves as some simple function than otherwise. If this function is linear, our measures are immediately serviceable; if logarithmic, one simple intermediate step is needed, etc. If there is such a thing as a biological mental unit it is difficult for me to conceive that our sensed awareness of it is not related to it in some very orderly manner, whereas I judge it would seem to Boring difficult to conceive that it could be so related. I must, however, qualify this statement, for Boring does seem to realize that there is something elemental in the "sense-distance." He writes, p. 31:

"The great difficulty is, as we have just pointed out, to find anything that we may properly call a psychological unit. The sense-distance is such a unit. It is a unit of measurement that is mental *per se*;" and, footnote:

"The thing is so simple when it is seen!"

Science has apparently succeeded in determining units linearly related to natural units in the matters of length, mass, time, temperature and in several other chemical and electrical fields, and all of these are available to the psychologist as starting points. Errors in human judgments in these fields and measured in these units have been found to distribute themselves according to the normal law (or in distributions so close to the normal that there is no present value in distinguishing between them). I am setting up no fetish in pointing out this truth. As a consequence of this situation I claim that the assumption of normality of distribution of errors in human judgments in other fields is the most reasonable one to make as a point of departure. I shall return to this point later, but would first examine into Boring's affection for the normal curve.

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[7] Boring's footnote is: "Boring, A Chart of the Psychometric Function, *Amer. J. Psych.* 28, 1917, 465 ff." I am unable to follow Boring's reasoning here as I find nothing in this very lucid article bearing upon the present point.

(*d*) I could quote at length and entertainingly, showing that Boring considers the idea of normality to be a fetish and the assumption of it under any circumstances, "with the bare hint of an exception . . . in the psychometric functions of errors of observation," unwarranted. This one possible exception will not concern us in this present section (*d*).

Now what is left in the way of statistical devices? I quote from p. 32, and secondly from p. 32-33.

"We are left then with the rank-orders of our psychological quantities, given by reference to a fixed but arbitrary extra-psychological scale; and it is with these rank-orders that we must deal."

"There is nothing new in the contention that mental measurement is impossible, whereas now we do gain the assurance that rank-orders at least are validly demonstrable. And there is a great deal that can be done with rank-orders. We can deal with frequencies, medians and quartiles. For example, it is considerable to know of two groups that the lower quartile of the ranks in one overlaps and is practically coincident with the upper quartile of the other. What we must remember, however, is that we are dealing with the statistics of medians, quartiles, contingencies, and correlation ratios; not with the statistics of averages, standard deviations, coefficients of correlation, and linear regressions. All those statistical constants, that imply a scale of equivalent units, violate in use the conditions of the case and lead to a precision of result that is an artifact."

Let us examine these permissible statistical procedures. A correlation ratio is the ratio of two standard deviations, each of a variable which is quantitatively measured, *i.e.*, measured upon a scale of equivalent units. Surely Boring did not intend to approve of this. We may put this down as a mathematical slip, and proceed to the other measures. For the calculation of a median or quartile in the case of grouped data some assumption as to the quantitative relations between groups and the distribution of measures within groups is necessary to avoid a grouping error. Since we must not deal with graded measures, we will assume that Boring has in mind such a large number of categories in his ordered series that there is no correction for grouping needed. Limitation to series with such large number of categories would in itself be a very serious practical handicap, but we will pass this over, and suppose that we have determined a median for a certain ordered series. Standing alone, it is meaningless. It is only in its relation to some other percentile of the same group, or of a new group, that it has significance. The comparison cannot be of numerical values, for quantitative measures are ruled out. We can therefore only make a comparison of the type 25 percent. of the scores of group A exceed 50 percent. of those of group B. But what of this fact? It is empty in meaning unless we can interpret it in terms of the magnitude of the difference between the two groups, *e.g.*, as .6745 σ, or in terms of the improbability of the situation arising as a matter of chance. The first means of interpretation is obviously ruled out; and the second, involving as it does a measure P, derived from frequency values of categories,—just as at the beginning of this article P was derived from χ^2 and the number of cells,—has involved in it the assumption of normality of frequency of distribution under random sampling, and is thus likewise ruled out. We cannot interpret quartiles or medians without resort to assumption of normality or some other form of distribution or resort to quantitative concepts. We have still to consider measures of contingency; χ^2 the square contingency, Φ^2 the mean square contingency, C the coefficient of contingency, and P the measure of probability already described. I will not weary the reader with a long exposition, but will simply say that the meanings of the first three of these are involved, and that I at least am not able to interpret them without resort to one or more assumptions as to form of distribution. The meaning of P and the assumption of normality involved in it have been made clear.

Our conclusion therefore is that none of the measures advocated by Boring is free of those things which he inveighs against. It is this situation in particular which leads me to suggest that Boring's criticisms were made without due appreciation of the foundations of statistics. If he had realized that the assumption of form of distribution or of comparability of units is basic in every statistical treatment, I believe it would have led him to a discussion of the relative merits of different assumptions and not to a total condemnation of assumption. The dictum "assume not at all" is just as impossible in practical statistics as it would be in everyday matters of conduct.

I will quote from Boring's last paragraph, p. 33.

"The initial error in the application of the theory of probabilities was the assumption of the law of insufficient reason. It was wrongly supposed that knowledge could somehow be wrought out of ignorance. This very error, however, has never been routed. . . . But, if in psychology we must deal—and it seems we must—with abilities, capacities, dispositions and tendencies, the nature of which we can not accurately define, then it is senseless to seek in the logical process of mathematical elaboration a psychologically significant precision that was not present in the psychological setting of the problem."

To do what Boring considers "senseless" is to my mind the exact purpose of mathematical elaboration; and as the error of believing that knowledge can be wrought out of ignorance is still strong within me, I shall devote the rest of this article to a discussion of mathematical procedures, coupled with appropriate assumptions, which I anticipate will lead to an increase of knowledge.

PART 2.

It might seem axiomatic that there can not be a science of quantitative measurement until and unless there is established a particular unit of measurement. This is, however, true only in a limited sense; for it is quite conceiveable that one could have a science of physical phenomena in which the units were such that the scale of time intervals was the square of the present intervals measured in seconds, and in which the length scale was logarithmic as compared with the present scale in centimeters, etc. Of course, in terms of these new units, all the laws of physics would be stated by means of formulas different from and in general more cumbersome than our present formulas; but nevertheless we could have an exact science. The existence of the science does not lie in the units employed, but in the relationships which are established as following after the choice of the units.

A parallel situation holds with reference to mental measurement; so that, starting with units however defined, if we can establish important relationships between phenomena measured in these units, we have proceeded scientifically. The choice of the unit is purely a question of utility. It is preferable so to define it as to lead to the simplest possible algebraic statements of the important relationships. There may be many such relationships, and the simplest units for one purpose may not be the simplest for a second. However, just as Euclidean geometry is more serviceable than non-Euclidean for all-around practical needs, so we may expect that there is one way of defining mental units which will be generally more serviceable than any other. Without denying the possibility of other workable systems, it seems that, in a civilization such as ours, steeped in the elementary associative and commutative principles of arithmetic and algebra, much is to be gained in simplicity and accuracy of interpretation if the units employed in mental measurement obey these well-known laws.

Let us view this from the standpoint of certain everyday problems. I shall use the terms "general intelligence" and "mental age" with their usual meaning, which is a sufficient definition for my present purpose. Here in the United States we have an elementary public-school system adapted to the progress of a child from grade to grade by yearly steps, and we frequently wish to know how far ahead or behind a child is in terms of time. Thus, "one year advanced" is an immediately serviceable concept for elementary school needs. For this reason the mental age and the mental year, or unit, in the mental age scale, are concepts which in spite of their shortcomings may be expected to persist. They are of uncertain value in the high-school, and very confusing in connection with still higher ages. All told they are not generally serviceable measures, and for three reasons closely related to each other: first, the mental year is such a unit as does not conform to the elementary laws of algebra; secondly (this is not yet indubitably established), disparate mental factors are thrown together; and thirdly, and possibly merely as a consequence of the first two, regression relationships when wide ranges in mental ages are involved are generally nonlinear.

As to the first: if we add the mental year 5.0–6.0 to the basic mental age 5.0 we shall obtain a mental age 6.0; but were we to add the mental year 18.0–19.0, which is known to be practically zero, to the same basic mental age, we should obtain 5.0+ as the answer. Some may object, saying that we cannot make and are not called upon to make this second addition, but can properly make the first. This point is well made; and because it is, the peculiar non-additive character of the mental year is not directly apparent. It however does enter, indirectly, whenever calculations covering several mental years and utilizing means, standard deviations, product moment correlations, etc., are made. Otis[8] has pointed out several of the many advantages resulting from a use of additive units, and I shall not dwell further upon this point.

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[8] A. S. Otis, Some logical aspects of the Binet Scale, *Psych. Rev.*, 23, 1916.

Secondly: one can argue that the entire general intelligence, and therefore as a consequence mental age, idea is wrong, and that adding the ability to "copy a square" to "repeat six digits" and obtaining the "difference between a king and a president" is like adding a box of soap to a keg of nails and obtaining a bolt of cloth. There is enough experimental justification for this point of view that the mental measurer should ever be on the watch to discover qualitative differences which may be embodied in his seemingly quantitative results; but on the other hand the rich reward during the last half-generation in the more fruitful understanding of important individual differences, and the better adaptation of teaching method to individual ability which has been consequent to general intelligence measurement, fully justify the thorough examination of differences which are apparently qualitative with the end in view of discovering quantitative relationships. Whereas the second concept, soap + nails = cloth, has served no end, the first has led to procedures the general excellence and justice of which as compared with methods displaced can not be gainsaid. The quantitative treatment of mental differences needs no further justification than this, though its most ardent advocates will admit it needs much refinement.

Thirdly: when mental age is one variable and when any one of a few score of our psychological tests is the other, and when wide ranges of talent are involved, it is common to find a tendency of the regression line of score upon mental age to become parallel to the mental age axis at the upper end. It seems reasonable to expect that this result is mainly attributable to the lessened intrinsic significance of a mental year high in the scale as compared with one low in the scale. Thus, if the mental year is given progressively less numerical value as the scale is ascended, regression relationships will generally tend to become linear.

This same result could be accomplished by progressively changing the value of the units of the psychological test, the mental age units being left unchanged. This would, however, be an unfortunate means of bringing about linear relationships, because the units now employed in the better psychological tests, *e.g.*, National Intelligence, Army Alpha, Terman Group, Otis Group, Trabue Completion, Thorndike Handwriting Scale, Hillegas Composition Scale, etc., are very approximately proportional to sensed differences in ability as determined by opinions of competent judges. There is a warrant in the sensed difference which is basic, since the purpose in measuring and classifying by means of tests is to do better and more economically that which human judgments already attempt to do. The determination of individual differences in units, however divinely given, which have no discoverable relationship to individual differences as sensed by teachers and employers, or by pure psychologists if they are to interpret the test results, is without the specific value which we are regularly seeking. I find myself in hearty agreement with Boring upon this point; and though we might not define "sensed difference" in the same way I do believe our definitions would result in proportionate units, which is all that is necessary. Because the sensed difference is a mental unit per se, it is serviceable in handling problems which are mental per se.

I have endeavored to show that the mental year and the mental age have fundamental defects for quantitative statement and interpretation. The intelligence quotient (I.Q. = mental age ÷ chronological age) suffers from the defects already pointed out of its parents, mental age and chronological age, though to a certain extent these defects are recessive, not appearing in the offspring, as is apparent from considering the entirely straightforward way in which an I.Q. of 1.00 may always be interpreted. If the I.Q. is not greatly different from 1.00, and particularly if elementary school ages only are involved, it is probably a good measure, though not one the use of which we can defend for the most refined and theoretical work.

Let us consider in more detail what are the defensible bases for determining the units of a mental scale. The statement as made presupposes that we have an ordered series. I anticipate that much greater difficulty will be experienced in determining a homogeneous ordered series than in scaling it afterward. I hope at a later date to offer criteria for determining if a number of mental tasks involve one or more qualitatively different mental functions; but for the present discussion I shall assume that we are possessed of an ordered series qualitatively homogeneous, and wish to determine quantitative values for the various items in the series. The reader may have in mind a series a, b, c, . . . of brightness intensities, concerning which we know that they constitute a single series such that, in intensity, $a < b < c <$ etc. Making no use of physical measurements of candle power, strength of electric current, or the like, we wish to assign numerical values to the items in the series such that of items scaled 10, 11, 15, 16 the feeling or appreciation of difference (11–10) is equal to that (16–15), etc., throughout the entire scale.

The question arises who is to do the "feeling." To meet practical needs, it should be the type of person or persons called upon to make, or known to be capable of making, such judgments in daily life and whose procedures are to be assisted by the possession of the scale, *e.g.*, painters, photographers, dyers. Scalings built upon the judgments of individual X might conceivably be different, and not merely different in some constant proportion, from the scalings yielded by the judgments of individual Y. If our purpose is not to study the idiosyncrasies of X and Y, but to obtain a scale of general utility, we should utilize many judges, giving such weights to their separate determinations as we may have auxiliary evidence to warrant.

There are different psychophysical methods for obtaining judgments of difference, and without attempting to designate some one as best we may state two axioms. (1) Since it is assumed that the scale constructed under psychological conditions *A* will be serviceable under practical conditions *B*, therefore the more similar *A* and *B* the less violent the assumption. Corollary: generally the greatest practical need for assistance is in the matter of fine judgments; therefore the "just noticeable difference" may well be incorporated in the derivation. (2) That method is preferable which shows the greater consistency with itself, *i.e.*, which results in scalings having, for a given number of judgments, the smallest probable errors. To profit by this axiom one would ordinarily have to resort to brief preliminary scalings by the various methods and roughly determine the probable errors.

Whatever method is chosen, some preliminary assumption as to the distribution of judgments must be made; and *the better this assumption is as judged by the distribution of judgments upon the completed scale the more accurate will the scalings be.* Not having seen this criterion of the validity of the assumption proposed or recognized by any previous psychological or educational scale-maker, I will give the steps in detail together with a numerical illustration. Consider the scaling of brightness intensities from the judgments of a single judge.

Lemma: A true, or biological, intensity scale for this judge exists. Upon it intensity items *a, b, c,* . . . have numerical values *A, B, C,* . . . A single form of distribution of errors of judgment for this judge exists (if this is not true the problem is much more difficult, though not necessarily insoluble). If by good luck we assume the true form of distribution and know the number of times each item *a, b, c,* . . . is judged more intense than each other item in the same series, we can deduce a set of values *A′, B′, C′,* . . . linearly related to *A, B, C,* . . . Finally, we may calculate the form of distribution of errors of judgment of this judge in terms of the scale *A′, B′, C′,* . . . and this form of distribution will be the true form. All of this will be obvious to the person who has constructed a scale, such for example as the Hillegas Composition scale.

Proposition: If the form assumed is not the true form, the final form calculated in the units of the A', B', C', . . . scale will not be the assumed form nor the true form but intermediate. Corollary: starting anew, assuming this intermediate form, second intermediate form may be obtained which is still closer to the true form. Etc., for still closer approximations.

I need not dwell upon the Lemma; but as the Proposition itself is far from obvious I shall cite three things in its support: (1) a brief handwriting scaling experiment; (2) a numerical illustration of the process; and (3) the reason for it as consequential to the properties of an average.

(1) Using rankings of judges (equivalent to assuming a rectangular distribution of values of handwriting samples) at their face value, I obtained scale values of the samples of handwriting and found that the distribution of samples in these units was approximately normal. Thus the assumption of a rectangular distribution did not lead to a final form of distribution which was rectangular.

(2) The following illustration is made brief and does not involve the assumption of normality. This fact makes it quite artificial, but it should be the more convincing to one who fears the surreptitious introduction of the "normal fetish." Let us then be given intensities a, b, c, d whose true values, measuring from intensity a are 0, 1, 3, 4 respectively. Further, the true distribution of errors of judgment of judge X is triangular, being represented by an isosceles triangle with base 10 and altitude 0.2. The equations of the lines defining this triangle are

$$y = 0;\ y = .2 - .04x;\ y = .2 + .04x.$$

Integrating this distribution we obtain the psychometric function. We shall need it for one-half the curve only. Integrating, $y = 2 - .04x$ we obtain,

$$\int y\,dx = .2x - .02x^2 \qquad (x \text{ varies from 0 to 5})$$

Thus the psychometric function, P, for the upper half of the curve is given by

$$P = .5 + .2x - .02x.^2$$

To illustrate: intensity b is one unit above a; therefore, substituting 1 for x we obtain .68, the proportion of times that b is judged more intense than a. Similarly, with this situation as described, the judgments will be as indicated in the accompanying table.

TABLE I.

	Proportion of "greater judgments"
$a > a$	.50
$b > a$	.68
$c > a$	.92
$d > a$	.98
$b > b$	.50
$c > b$	.82
$d > b$	.92
$c > c$	.50
$d > c$	.68

The problem before us is, knowing only the proportions of greater judgments as given in the table, to determine the function

$$P = .5 + .2cx - .02(cx)^2.$$

The c has been introduced because all we need are units proportional to the true units.

We may first and incidentally note that the Lemma holds true here. Thus, let us assume a triangular form of distribution represented by the equations

$$y = 0;\ y = A - A^2 x;\ y = A + A^2 x$$

which is the general statement under the restrictions that the total area shall equal 1.00 and that the distribution be symmetrical. We then have

$$P = .5 + Ax - A^2x^2/2.$$

Being given proportions, P, and utilizing this integral of the distribution, we obtain x-values as follows:

TABLE II.

$a > a$	$.50 (= P)$	*therefore*	$x = .0$	(The solution $x = 2/A$ is clearly extraneous)
$b > a$	.68		$x = .2/A$	
$c > a$	.92		$x = .6/A$	
$d > a$	.98		$x = .8/A$	
$b > b$	.50		$x = .0$	
$c > b$	.82		$x = .4/A$	
$d > b$	.92		$x = .6/A$	
$c > c$	.50		$x = .0$	
$d > c$	.68		$x = .2/A$	

TABLE III.

$$\begin{aligned}
\textit{Distance}\quad \overline{ab} &= (b-a) = .2/A \\
\overline{ab} &= (c-a)-(c-b) = (.6-.4)/A = .2/A \\
\overline{ab} &= (d-a)-(d-b) = (.8-.6)/A = .2/A \\
\overline{ab} &= (d-a)-(d-c)-(c-b)(=.8-.2-.4)/A = .2/A \\
\textit{Average}\quad \overline{ab} &= .2/A
\end{aligned}$$

$$\begin{aligned}
\textit{Similarly,}\quad \overline{bc} &= (b-c) = .4/A \\
\overline{bc} &= (c-a)-(b-a) = (.6-.2)/A = .4/A \\
\overline{bc} &= (d-b)-(d-c) = (.6-.2)/A = .4/A \\
\overline{bc} &= (d-a)-(b-a)-(d-c) = (.8-.2-.2)/ = .4/A \\
\textit{Average}\quad \overline{bc} &= .4/A
\end{aligned}$$

Similarly, Average $\overline{cd} = .2/A$.

Thus, assuming the true distribution, we obtain scalings by the method shown, in which each distance is the average of several separate ways of calculation, which bear the true proportion to each other. Now let us reverse the process,—let us start with these obtained scalings and the proportion of "greater judgments" corresponding, and derive the psychometric function P′. Beginning with some point, let us say a, we have the following table:

TABLE IV.

Proportion of "greater judgments"	at distance indicated above a
.50	.0
.68	.2/A
.92	.6/A
.98	.8/A

Attempting to fit a third degree parabola to these points we find that the cubic term drops out, and we obtain as the result

$$P' = .5 + Ax - A^2x^2/2.$$

The fact that this has turned out to be the equation assumed (the assumed distribution being of the form of the true distribution) is evidence that the Lemma holds.

Returning to the main development: we are to start with the data of Table I.; assume some incorrect form of distribution; under this assumption scale the intensities a, b, c, d; on the basis of these scalings calculate the form of distribution of judgments; and, finally, note if this is closer to the true form than the assumed form.

I shall assume a rectangular form of distribution (this certainly differs from the true triangular form). The equation is,

$$y = A$$

and the limits in x are, $-\ .5/A$ and $.5/A$.

$$\int y dx = Ax.$$

For the upper half of the curve

$$\text{P} = .5 + Ax \quad (x \text{ varies from } 0 \text{ to } .5/A).$$

Being given the proportions, P, of Table I. and utilizing this integral of the assumed distribution of judgments, we obtain x-values as follows:

TABLE V.

(Compare with Table II.)

$a > a$	.50 *therefore*	$x = .00$
$b > a$	.68	$x = .18/A$
$c > a$	.92	$x = .42/A$
$d > a$	.98	$x = .48/A$
$b > b$	.50	$x = .00$
$c > b$	.82	$x = .32/A$
$d > b$	.92	$x = .42/A$
$c > c$	.50	$x = .00$
$d > c$	.68	$x = .18/A$

TABLE V.

(calculated as were the distances in Table III.)

$$\textit{Average } \overline{ab} = .08/A$$
$$\textit{Average } \overline{bc} = .23/A$$
$$\textit{Average } \overline{cd} = .08/A$$
$$\textit{Sum } \overline{ad} = .39/A$$

In order to facilitate comparison of these values with the true values, let us set $.39/A = 4$, the true value for the distance ad. Then $A = .0975$ and we have, Table VI:

TABLE VI.

P	Value obtained under rectangular distribution hypothesis	True values
.50	$\overline{aa} = .00$	.00
.68	$\overline{ab} = .82$	1.00
.92	$\overline{ac} = 3.18$	3.00
.98	$\overline{ad} = 4.00$	4.00

Passing a cubical parabola through these four points yields

$$\begin{aligned} P'' &= .50 + .268\,x - .065\,x^2 + .0070\,x^3 \\ P &= .50 + .20\,x - .02\,x^2 \\ P' &= .50 + .12\,x \end{aligned}$$

The equations for P (the true function) and P′ (the assumed function) have been written down for comparison, the arbitrary constants being so chosen that the equations agree for the points ($P = .50$, $x = .00$) and ($P = .98$, $x = 4.00$). Since P″ is closer, in the constants involved in it, to P than is P′, the proposition holds for this case.

If we now repeat the process, assume the function P″, and determine a new psychometric function, we obtain

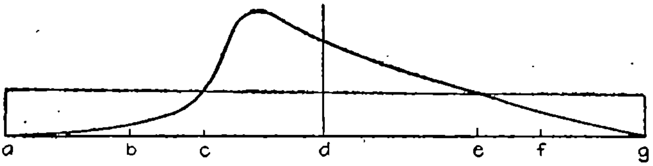
$$P''' = .50 + .207\,x - .024\,x^2 + .00064\,x^3$$

which is still more similar to the function P. In fact, by this time all resemblance to the original erroneous rectangular hypothesis is lost.

In actual work the procedure would not be as simple as in this articicial problem because, having but four points and assuming my data to be entirely trustworthy, I have not resorted to least-square methods for determining P″ and P‴. The principle, however, is the same in the more difficult problem, so let us examine the steps taken with a view to ascertaining what it is that leads to this fortunate result.

(3) I shall present the point of this section by considering the relationships between one form of distribution, the true form, and a second, the hypothetical or assumed form. For simplicity of treatment it will be necessary to deal with actual forms, but the reader will see that the treatment is entirely general and that the argument may be carried through in a manner similar to that here followed and to the same conclusion if he advises with his own special deity in determining the true form and is advised by his demon in selecting a hypothetical form. We will start with a true distribution, skewed, unimodal, and having diminishing frequencies as we approach the lower, *a*, or upper, *g*, limit of the range. For the hypothetical distribution we will take a rectangle upon the base $\overline{ag}$. Both distributions have total areas 1. The rectangle cuts the true distribution in points, measured along the base line, *c* and *e*. We will call the area upon base $\overline{ac}$ and under the true curve AC, and that under the hypothetical A′C′, and similarly for areas resting upon other segments. Note that $AC < A'C'$; $CE > C'E'$; and $EG < E'G'$.

Let us further say that point d on the base line is the median of each distribution. Then AD = A′D′ = .50. Let us divide the $\overline{ac}$ and $\overline{eg}$ segments still further, in any arbitrary manner, obtaining points on the base line as indicated below and areas under the true and hypothetical curves as designated.



Areas-true	AB	BC	CD	DE	EF	FG
" -hypoth	A'B'	B'C'	C'D'	D'E'	E'F'	F'G'
	AB<A'B'	BC<B'C'	CD>C'D'	DE>D'E'	EF<E'F'	FG<F'G'

We will start scaling from point d. Thus if AE proportion of judgments designate stimulus y as greater than stimulus x its correct distance above x is $\overline{de}$. However, with the incorrect distribution as hypothesis it would require A′E′ proportion of "greater" judgments to result in the y being designated as just $\overline{de}$ above x. Since AD = A′D′ and DE > D′E′; therefore AE > A′E′, and a proportion AE of "greater" judgments results, under the incorrect form of distribution hypothesis, in a too large numerical value, let us call it $\overline{de} + \Delta$, for the distance that y is above x. Similarly, any proportion of "greater" judgments lying between AE and AG will result, under the incorrect hypothesis, in scalings numerically too large, but *decreasingly too large*; for the proportion AG results in the scaling $\overline{dg}$ under both the true and the incorrect hypotheses. Thus, if stimulus z yields a proportion AF of "greater" judgments than x, the scaled distance under the incorrect hypothesis will be $df + \Delta - \delta$ where $(\Delta - \delta)$ is positive. Subtracting the first from the second of these two scalings, we obtain for the distance apart of y and z the quantity

$$(df + \Delta - \delta) - (de + \Delta) = ef - \delta$$

which is numerically smaller than the truth. If we compare z with y directly, we are utilizing the distribution in the neighborhood of d and, as already shown, our obtained scaling for this neighborhood will be numerically too large; let us call it $ef + \delta'$. Thus we have established the proposition that the error in scaling, when utilizing the middle portion of the assumed distribution, is of the opposite sign to that present when utilizing outlying portions of the same distribution. As a consequence of this, in the long run, average scalings based on those from both inlying and outlying portions of an assumed distribution diverge from the errors of either of these separately in the direction of true scalings. Thus, in the numerical illustration given, the averaging of four different ways of calculating each distance was the step responsible for the excellence of the scalings, and as a consequence for the obtained distribution being closer to the true distribution than was the assumed distribution.

We may now continue the argument of page 423, broken into by the proof of the Proposition used. The reader will clearly see that the preliminary assumption may be made strictly tentative and need not prejudice the form of distribution as finally determined. As Boring has properly stressed, if we have either the form of distribution or the unit, we can determine the other. Therefore, if we can secure a rank order of items in a homogeneous series, and if a single form of distribution of judgments (of the same judge for separate parts of the scale, or of several judges for the same part, or of several judges for several parts) exists, we can determine it and give numerical values to the items in the series. These numerical values will be additive in the ordinary sense, a difference of a unit in one part of the scale equalling a difference of a unit in a second part, as this has all been involved in the method of derivation,—the units being, of course, in terms of sensed differences. I believe that the sensed difference is the most serviceable and defensible basis for scaling a single homogeneous ordered mental series; but there are other very defensible bases resulting in scalings the units of which are equal in a very important sense, but not necessarily in the meaning of being sensed as equal.

We may start with the hypothesis that the variability of performance of several individuals, working at different times upon equally difficult tasks, under as similar conditions as can be established, is of a certain constant form; scale the difficulties of tasks on this assumption; calculate the form of distribution of variability of performance; and, if this form is the same as the assumed form, accept the scalings as final; but if not the same, revise the assumption and repeat the scaling as indicated in the case of judgments, attaining the true form of variability of performance; finally securing true scalings in the sense that the units are equal in terms of the variability of performance of individuals. It is very possible that the units thus defined would be sensed as equal, but we have no compelling a priori ground for asserting this. This method may prove cumbersome in actual use because of the necessity for certain controls,—allowance for fatigue, training, change of interest, inequality of assignments, etc.

A third method of scaling is to assume that the variability of a group is constant, assume a form of distribution, correct the assumption as may be necessary, and scale the items of the series. Units here are equal in terms of the variability of the group; but, as we cannot hope to secure biologically pure strains when working with human material, we cannot expect to define our group, and accordingly our unit, in a manner that has ultimate significance. This, however, is a very theoretical criticism; for such a group as "white native-born 10-year-olds in the public schools" is quite definite enough to serve as a basis for most of our school needs. The method which has been used widely, of assuming the form of distribution of the grade or age group, differs from that here proposed in that it has utilized no check as to the accuracy of the original assumption. The method here described assumes constancy of form of group distribution, not normality of it.

The possession of units, equal to one another in any one of these three senses, would give much greater flexibility in treatment than is possible with a mere ordered series. I have heard the statement "where a difference can be recognized it can be measured" attributed to Thorndike. This may be correct, but I can as yet only heartily subscribe to the proposition that "where a difference between items in a homogeneous series can be recognized it can be measured."

Let us now treat of the bearing of correlation relationship upon scaling. One may ask: Why deal with two variables? Has it not been shown that data with reference to a single variable suffice? Yes, they suffice in the handling of the problems of the single variable; and if a given problem involves two variables this does not lessen the basic significance of the scalings of each of the variables separately. However, there may be short cuts to this task of securing commutative units, and there may be situations in which we have only secondary interest in the units. Thus, for example, if five tests are being used to select salesmen, it may be quite immaterial to us if the units have significance per se. If their multiple correlation with the criterion is satisfactorily high our purpose is accomplished. Of these six items, five tests and criterion salesmanship, we are concerned with securing a scaling of salesmanship only upon the sensed difference basis, and are quite content to let the units of the five tests take care of themselves, provided only that they yield a good correlation with the criterion. We can change the units of the five tests at will, and shall wish to change them so that the multiple correlation of the battery of five with salesmanship will be maximum.

The fewer crude approximations are resorted to, the less of significance will be lost. If we assume a linear regression in a situation where a decidedly culvilinear one obtains, we have decreased thereby the size of our multiple correlation coefficient.

It is however impractical, owing to the statistical difficulties involved, to attempt a multiple correlation calculation of six variables with sundry sorts of curvilinear regression. Even the handling of the three variable curvilinear regression problems is very complex.[9]

We must therefore as a practical matter change our five independent variables into five auxiliary variables yielding linear regression and being therefore more amenable to statistical combination. Thus, keeping the salesmanship units inviolate, let us compress or stretch the units of the first test in some simple systematic manner so as to bring about linear relationships in the table involving the criterion. Theoretically this is not always possible, but experience would suggest that for ordinary situations involving single valued regression lines it can be done. Let us do the same with each of the other independent variables. Then every correlation table involving the criterion will show two linear regression lines. Isserlis has shown[10] that, if any three of the six regression lines found in the three correlation tables of three variables are linear, then all six are linear. Thus we see, by the repeated use of Isserlis' proposition, that if we make the regressions with the criterion linear we shall at the same time make all the regressions in all the intercorrelation tables linear. We shall thus have a final result to which the linear multiple correlation method is entirely applicable. Thus, ordinarily, non-linear regressions between test and criterion constitute no bar to multiple correlation technique.

This method has been proposed so as to bring about a high correlation with the criterion; but is it not likely that it would at the same time result in securing test units of essential merit per se provided the criterion units have such merit?

In the preceding paragraphs we have considered a situation in which the units of the criterion were established and the units of the tests only at our option. In certain theoretical problems the units of the criterion may be of no particular significance except that they give an ordered series. In such case we may change at our option any or all of the units of measurement. Let us say that we desire to change them so as to obtain the simplest possible picture of interrelationships. Surely, if it is possible to do so, it is the part of wisdom to chose a simple picture of mental relationships rather than a complex one.

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[9] See Karl Pearson, On the theory of skew correlation and non-linear regression. *Math. Contrib. to the Theory of Evolution*, Biometric Lab. Pub., Cambridge Univ. Press, 1905. Also: On a general method of determining successive terms in a skew regression line, *Biom.*, 13, 1921.

[10] L. Isserlis, On the partial correlation ratio, *Biom.*, 10, 1914.

The following theorem: "If the arrays of a correlation table are equally variable the correlation surface is normal,"[11] suggests a procedure which, if possible, will result in a great simplification of concept of mental relationships.

One further theorem, stated without proof and following from Narumi's proofs is: "If in a correlation table marginal frequencies are normal and if regressions are linear then the correlation surface is normal." Thus, utilizing Isserlis' theorem and the one just stated, we see that, if it is possible so to change the various units of measurement that all the arrays in the tables involving some one of the measures are equally variable (the number of psychological test intercorrelation tables in which this is quite approximately so without changing any of the units is great), then all of the intercorrelation tables are normal. We reach a picture of mental relationships which is peculiarly simple to comprehend and to treat statistically. Should data be such as to show that this is a possible picture of certain mental relationships, it would be very valuable to attempt to discover further properties of the units pertaining to this system. Should these units turn out to be sense differences, or performance differences, we should be provided with doubly serviceable units.

I have suggested four units of value in mental measurement and have described how each one can be determined: (*a*) the sensed difference unit, (*b*) the variability in performance unit, (*c*) the group variability unit, and (*d*) the unit resulting in the simplest picture of interrelationships. It remains for experimental work to establish whether or not these four are really different. If so, we shall probably need to keep them all and shall need accurate determinations of functional relationships between them.

Whatever the experimental findings are it is certain that, if we can count (and we do so count in our daily affairs) upon a constancy in sensing differences, a constancy in human performances, or a constancy in mental relationship, the possibility of a unit and a science of mental measurement exists.

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[11] This theorem was first stated by me in October, 1922, without proof, in the form of two theorems. (*a*) If the arrays of a correlation table are equally variable the regressions are linear; and (*b*) if arrays are equally variable and regressions linear the correlation surface is normal. Later Seimatsu Narumi, a student in Pearson's Laboratory, proved that both theorems are true, thus leading to the single one given.

THE PSYCHOLOGY OF FEELING OR AFFECTIVE REACTIONS

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Feeling phenomena have always occupied a very peculiar position in psychology. On the one hand, they have been considered as partaking of the most pronounced inner and subjective character. Much more than any other kind of psychological facts have feeling phenomena therefore been considered elusive and unamenable to investigation. On the other hand, from a very early period of modern psychological history the study of feeling phenomena has always involved an emphasis of the so-called external or bodily character of our feeling reactions. For example, upholders of the James-Lange theory of emotions or feelings and their historical predecessors emphatically stress somatic factors if only as expressions of internal mental states. In point of fact this stressing of visceral factors in the study of feelings constitutes in a genuine way one of the actual historical beginnings of a behavioristic attitude in psychology. And so while feeling phenomena have always carried with them the basis for their naturalistic treatment as definite scientific data, still it is probably the bias of internal mentality which has hitherto prevented any naturalistic description of them. To the writer this condition seems intolerable in view of the great importance and prominence of our affective life. Accordingly, the following paper is an attempt to analyze and describe feeling activities as objective scientific facts.

I. *The Nature of Feeling Responses.* Feeling reactions constitute a distinct type of behavior segment in which the responses, though always morphologically overt, do not, however, effect any immediate change in the stimulus objects as in the case of most of the other overt forms of activity. Only one exception is possible, and that is when both the stimulus and the responding individual happen to be the same person. This, however, is clearly a peculiar and non-typical situation. In affective behavior, then, the response or the action to the stimulus is not performed upon the stimulus but upon the acting person himself. In other words, the peculiar fact about feeling responses is that, instead of the person producing some effect upon the stimulus object, the person himself is affected. Emphatically, we are dealing with a genuine adaptational response, although the mode of adaptation is such that the active changes taking place are localized in the acting person or in his changes of posture or attitude toward the stimulus rather than in the objects or persons acted upon.

The description that we have given implies that a direct overt form of action is performed; that is to say, the stimulus object or adjustment stimulus is immediately present and not absent as in implicit behavior. In implicit activity it is obviously the normal condition that no changes are brought about in the adjustment stimulus or stimulus object; but that is because the latter is absent, and substituted for by some other object. In the case of overt affective behavior, although no effect is produced upon the stimulus, there is nevertheless a direct and immediate contact of the acting person and the adjustment stimulus object.

Moreover, the affective reaction is not a delayed reaction which, after being initiated, is not consummated until a later time, nor is it in any sense action which is inhibited or in some way prevented from occurring. Such notions are implied in the idea that feelings involve a passive state of mind prior to a final and resulting expression which affects persons and objects. Also affective responses do not constitute indifferent functioning of the person, which is attached to or accompanies some other kind of action. No, the affective response is a direct reaction made to its own correlated stimulus. But this is not to deny, however, that very frequently affective behavior influences other actions, as we shall later have ample occasion to observe.

How can we further characterize the intimate nature of affective behavior? To formulate an effective identifying description we must refer to a few members of a large series of differing modes of affective action, each of which depends upon the specific character of its stimulation as well as upon the behavior history of the acting person.

The simplest form of such an affective response consists primarily of an increase or a decrease in the general functioning or activity of the person. The individual may be merely retarded or accelerated in his activity, or he may be said to react positively or attractively, negatively or aversely, to some object. This type of affection represents an apparently unlocalized and ineffective type of behavior which is visibly the farthest removed from producing changes in stimuli objects or persons. The latter simply start up a specialized type of behavior change in the partial or total activities of the person. Possibly this type of reaction can be illustrated by the response of well- or ill-being induced by a bright or a dull day.

Another form of affective responses displays a less marked passivity on the part of the person. This type differs from the kind just treated in that the person is more definitely directed toward the stimulus object and his action seems more definitely organized with respect to some particular stimulus. That is to say, the reaction system more definitely involves a certain organ or series of organs of both the external skeletal and visceral forms. Such reactions are exemplified by the elementary anticipations or pleasures induced by seeing or smelling food.

Among the more complicated affective responses we may note those in which the person is made depressed or elevated, either in his general behavior attitudes or in some specific action toward an object or person. In these cases the stimuli produce in the person more decided and more positive reactions, stirring him up more definitely and arousing or depressing him more acutely. In these cases the person is more sensitive to the qualities of the stimuli in general and to their particular organization of details. A fairly good example of these reactions is offered us by simple aesthetic responses.

In still more complex situations the effect of the stimulation upon the person will be to increase or decrease his contact with certain things or to arouse his interest in the object or person to the end that he will put himself into a posture for further stimulation in order to be more and more affected, that is, pleased, aroused, depressed or otherwise activated. So intense and complex may the response be that all actions, to other than the affective stimulus, may be inhibited, while the person becomes more and more active, either aroused or depressed, with respect to the stimulus object. In intense situations this direction of activity toward the affective stimuli may fairly amount to a total dissociation of the person. In different words, the person may become manic or stuporic, depending upon whether the stimuli conditions are arousing or depressing. But in no case does the affective response as such involve any actual modification or change in the stimulus object.

Feeling reactions are characterized then primarily by the fact that, even when they are most active and energy-consuming, they do not constitute operations upon the stimuli objects or persons. How is this situation possible? The answer to this question lies in the fact that the affective responses are actions in which the person is above all and essentially responding with internal mechanisms. It is for this reason that the traditional descriptions of affective phenomena have contained the term non-localisability. Now as a matter of fact the affective reactions, as far as the reaction systems of the organism are concerned, are always definitely localisable, for they represent actual responses of the person to their appropriate stimuli. These localisations cannot always of course be observed by the unaffected onlooker. Howsoever "passive" and immediately ineffective feeling responses are, they are still genuine psychological responses and not merely organic states or tones. Feeling reactions consist of actually organized reaction systems. This warning need be made only with reference to the very simplest of our affective reactions, since it is only those, consisting as they do mainly of visceral mechanisms, which superficially appear as merely conditions of the organism instead of actual direct responses. When we consider the more complicated affective reactions, such as the passion responses,[1] they cannot be described of course as other than specific organized reactions.

And finally we must point out that affective reactions must not be looked upon as indifferently induced by different kinds of objects. This error too is founded upon the belief that feeling reactions are merely biological conditions instead of definite psychological reactions. Were affective responses indifferently aroused by various dissimilar objects they could not be considered as definite psychological phenomena. As a matter of fact affective responses are always aroused by specific stimuli, although we must admit that frequently we experience great difficulty in distinguishing from one another the details of different affective stimuli as well as their corresponding reactions. This condition is, however, exceedingly common in the domain of psychology.

II. Distinction of Feeling Responses from Emotional, Desire and Pain Reactions. Because of the nature of feeling reactions, especially of the simpler and unlocalised type, they are very frequently confused with other types of reactions resembling them in some respect. It is essential, therefore, to distinguish affective reactions from the three kinds mentioned in the heading of this section, which more closely than other forms resemble affective responses.

A. *Emotional and Affective Reactions*

And first let us distinguish feeling from emotional responses. In the history of psychology the feeling and emotion reactions have always been very much confused, and more especially has this been the case since the emergence of biological conceptions in psychology. The basis for this confusion lies in the fact that emotions very prominently involve organic or visceral reflexes, and so emotional reactions apparently constitute a specific type of affective behavior. This resemblance, which is of course genuine, is, however, merely superficial and entirely unjustifiable. To identify feeling reactions with emotional responses because they both involve visceral reflexes is just as illegitimate as to reduce to one type all the reactions which do not involve visceral mechanisms. Such procedure in both cases results in overlooking all the essential characters of the different types of action. And so, as a matter of fact, when we study emotional and feeling reactions as specific behavior situations, we find very definite differences between them. What are these differences?

Foremost is the fact that in the emotional behavior segment there occurs a total disruption of the person, because a reaction system to some overwhelming stimulus fails to operate. Lacking the appropriate or customary reaction the resulting disturbance of the individual is followed or accompanied by the operation of many reflex responses of a visceral sort.[2] But let us note that these reflex responses are replacement actions; they are the only type of behavior the person can perform under the peculiar conditions of the disruptive situation; but, unlike feeling reactions, they do not serve as appropriate responses to definitely correlated stimuli. These replacement reflex reactions we must look upon as really biological processes. The person's psychological personality is for a moment, at least, in abeyance. Quite otherwise is the case in actual feeling behavior. Here there is, as we have already indicated, a definite reaction system involved in every case, a reaction system (as we have pointed out) involving visceral factors, but in every instance constituting a definitely organized mode of reaction.

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[1] See classification below.

B. *Feeling and Desire Responses*

Feeling and desire reactions are also confused. The factual basis is that desired objects are very frequently not attainable, and therefore the reaction seems to be one in which no effect is produced upon the stimulus object. Moreover, even when the object is attainable, the desire behavior segment[3] involves two distinct phases. The first phase is an implicit response in the form of an intellectual attitude, wish or thought concerning an absent object which is substituted-for by some other thing.[4] Or it may be an anticipation of possessing a present desirable object.

The second phase is the action of securing the object thus desired. The first phase is probably the more important of the two and should, if it properly bears the name desire, lend its name to the total behavior segment. The second action in the behavior segment is a result and consequent of the first. This statement we make upon the basis of the relative independence of the two actions; for the first may occur without the possibility even of the second transpiring, and in a great number of cases when the second and consequent action is possible it may be, and very frequently is, inhibited. Technically speaking, the first action is a precurrent response which calls out, or is a necessary antecedent of, the second reaction. Now, if this description of desire is correct we find here an acceptable basis for distinguishing between desire and feeling reactions. The latter, unlike desire responses, are usually, if not exclusively, the final or completing overt reaction systems in the behavior segments in which they are found.

Additional plausibility is lent our distinction of feeling and desire reactions by the fact that, whether desire responses are precurrent or final, as they would be if they were not connected with another final reaction, they are implicit activities, and thus very distinctly different from the majority at least of feeling reactions.

Feeling responses are also wrongly identified with desire responses through the historic identification of desires with the so-called vegetative functions or appetites. Such identification of very different processes by the use of a common term is greatly to be deplored. For the so-called vegetative functions are clearly immediate visceral reflexes and do not involve the protracted and mediate form of directedness-toward-some-stimulus-object that feeling responses do. The fact that desire reactions are frequently stimulated by visceral reflexes argues more for their difference than for their identity.

Another fact which makes possible the identification of feelings and desires is that in very many cases the thing or person desired is the sort which produces some affect in us. In other words, desires and feeling reactions are very frequently found to operate together. Possibly it is true that most of our desires are based upon or connected with some kind of feeling reaction.

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[2] Cf. J. R. Kantor, An Attempt Toward a Naturalistic Description of Emotions, *Psychol. Review*, 28, 1921, 19-42, 120-140.

[3] For a description of a behavior segment cf. A Tentative Analysis of the Primary Data of Psychology, *J. of Philos.*, May, 1921, 254-269.

[4] The thing present may of course be a substitute for the same thing possessed.

Just how unsatisfactory this identification of the two types of behavior is we may judge from the fact that the desire reaction may be composed of entirely different components. The desire reaction proper, when it is not connected with a feeling response, which is frequently the case, may not involve any more internal visceral responses than are usually ascribed to non-feeling reactions. This would depend upon the kind of stimulus object desired. Of course when the stimulus object for the desire is unconnected with activities like eating, sex, or (to be more general) does not involve intimate personal or organic conditions, then the reaction is entirely different and is composed of widely divergent elements. We must not overlook the fact that some desire reactions may be considered as practically completely intellectual in character and therefore essentially remote from feeling responses, although here again our intellectual reactions may be connected with feeling responses.

C. *Feeling and Pain Reactions*

Of the three types of reactions which are incorrectly related to feeling responses, pain reactions are probably more frequently and more intimately identified with feeling activity than either of the other two. No doubt the basis for this situation is contained in the fact that, while pain reactions in common with emotional and desire responses simulate feeling responses in that they do not involve changes in their stimuli objects, pain reactions do involve definite modifications in the person's own condition.

This resemblance between feeling and pain reactions does not, however, in any sense overshadow the decided differences between them. We may point out at once that, in general, pain responses are definitely discriminative or perceptual in character, whereas feeling reactions are primarily immediately adaptational. In other words, pain reactions are precurrent and sometimes anticipatory of further final responses. In this sense pain reactions are passive activities just like any other sort of discriminative action, whether visual, auditory, or any other. All of these discriminative or perceptual responses are passive and thus unproductive of direct changes in their stimuli because they are, on account of their vestigial character, partially implicit actions. On the other hand, however, when the pain reactions do lead to further and more immediately adaptive reactions, that is to say, when their final responses are not inhibited, they bring about very definite changes in their stimuli objects, and in this instance are sharply distinctive from feeling activity.

In considering the relation between pain and feeling reactions we must not be misled by the fact that in the case of pain some actual changes, such as laceration of tissue, take place in the person. For this type of change is very different in at least two ways from the changes in feeling responses. In the first place, the changes in pain reactions constitute the kind of contact with the stimulus object that bears the name of a medium rather than of a response; that is to say, the laceration or other modification of tissues constitutes the mode of stimulation rather than a feature of the response. And in the second place, even the actual changes in the organism performing pain reactions may be located in the superficial anatomical and functional features of the organism and not in the deeper mechanism as is the case with the feeling reactions. Even the older psychologists have pointed out that in pain reactions there is a decided outwardly directed activity involving exteroceptive stimulation and specialized neural pathways such as we find in the case of other perceptual behavior systems; and therefore they have contrasted pain with feeling activities. Moreover, the relative independence of visceral changes of even very exaggerated forms of pain responses argues for undoubted difference in the two kinds of activities.

As in the case of the desire reactions the very close interrelations between pain and feeling responses argue for a distinction in type and function of response, since the fact that pain reactions are possibly always stimuli for feeling reactions indicates a very great difference between them.

III. *An Analysis of Feeling Reaction Systems.* As a means of achieving a further understanding of feeling or affective responses it is important to compare their reaction systems, or their unit response factors, with those of non-affective behavior. Our plan is to indicate in what respects affective reaction systems are similar to and dissimilar from non-affective reaction systems, irrespective of any specific differences between affective reaction systems themselves. Not because the latter differences are not exceedingly important and the inevitable results of stimulation by different stimuli objects and conditions do we neglect them, but rather because we find similar differences between the individual members of every type of behavior.

Of the twelve or more unit factors of a reaction system[5] most show marked differences when the affective reactions are compared with the non-affective reaction systems. It is these that we shall emphasize in our brief survey of the unit factors of the reaction system.

(1) So far as the discrimination factor is concerned the feeling reaction system operates precisely like that of any behavior type. In other words, the total reaction or adjustment performed is differential with respect to the qualities of the object, the general nature of the total stimulus object and its relationship to other things (its settings).

(2) Much the same statement may be made with respect to the attention factor of the affective reaction system; since, if we mean by attention the fact of the person changing his response from one stimulus to another, there would hardly be room for any greater difference between affective and non-affective reactions than there would be between any two particular responses in either the affective or the non-affective class.

(3) When we consider the comparative affective factors in the different reaction systems we are impressed of course with the great difference between feeling and non-feeling reaction systems. In the latter, especially when they are definite non-feeling systems, there is indicated only a very slight change in the visceral and other internal mechanisms of the acting person,[6] whereas in affective reaction systems these changes are by far the most prominent features of the whole behavior situation.[7]

(4) Feeling reactions are naturally stimulated by objects and persons in precisely the same way as non-affective responses. In consequence, the

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[5] For a description of a reaction system see article mentioned in Note 3.

[6] It must not be overlooked that every reaction system is itself a unit in a behavior pattern or situation, that is, a unit in some total adjustment of the person, and therefore different reaction systems overlap. We must be understood therefore to be considering ideal or abstract reaction systems.

[7] The critical reader will observe that as a matter of fact the names feeling and non-feeling reaction systems are attached to these specific forms of action.

receptor functions of affective reaction systems or the modes of actual contact with stimulus objects are the same as in other types of behavior. The point to be observed is that, just as in the case of other types of behavior, so here we can observe that the reacting person is primarily in visual or auditory contact with a stimulus object. The same similarity of contact with stimuli is found between affective and non-affective responses whether the stimuli objects or conditions are located within or without the organism.

(5) As we must expect from the general character of affective responses the specific reaction systems will involve an overwhelmingly greater proportion of glandular functioning than is true of non-feeling responses.

(6) Furthermore, it follows also that the sympathetic neural mechanisms will have an exceedingly large component share in the operation of such behavior mechanisms, whereas, in the non-affective response systems, the somatic mechanisms are relatively more prominent.

(7) In both the affective and non-affective reaction systems probably no difference in the functioning of the central neural factor can be observed.

(8) Again, the muscular mechanisms will include a greater prominence by far of the unstriped muscles. Or to put the matter more plainly, the primary muscular action components in affective responses involve the visceral functions, although we should never make the mistake of assuming that the striped muscles are not at all involved in affective reaction systems. At this point we may be reminded again that the reaction system is merely an abstracted action of the organism. Probably the most important indication of the place of striped muscle mechanisms in affective response systems is found in the inclusion of facial grimaces and other facial movements so clearly observable as factors of at least the intense feeling responses. Moreover, who can overlook the large place that the vocal and other linguistic apparatus and functional mechanisms take in affective responses?[8]

IV. Are There Precurrent Feeling Reactions? In our description of feelings we have used as a basis affective reactions which constitute the final factor or act in a behavior segment. That is to say, we have considered only behavior segments which are considered as affective adaptations and which consequently carry the name of affective behavior segments. These segments are affective because, no matter how many other reaction systems they contain of a perceptual or other non-affective sort, their chief function from the standpoint of description is affective. In such behavior segments the feeling factor definitely concludes the adjustment in question.

Now the problem arises whether affective reactions are always final responses, or whether they can serve in other capacities in behavior segments. For example, cannot feeling reactions operate in the same manner as perceptual reactions, namely as precurrent responses to some other and final reaction? To this question we reply that feeling responses, just as well as perceptual reactions, can determine other responses in the same behavior segment. In other words, final reactions in behavior segments may be determined and conditioned by an affective precurrent response as well as by a knowledge or immediate manipulative action. It must be added, however, that in every case in which an affective response serves as a precurrent reaction it operates along with, and in addition to, a perceptual reaction.

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[8] Was it not the observation of the close connection between feelings and language which induced writers on language (Wundt for example) to find the origin of language in affective expressions?

We might illustrate the precurrent functioning of our affective behavior by pointing out that, just as our refusal to buy a picture must be determined by our perceptual appreciation of its nature or character, so the final action of refusing to make the purchase may likewise depend upon our affective reaction to that picture. Whenever the final act of the behavior segment is conditioned by an affective reaction in addition to the perceptual reaction, that behavior segment is of course complicated and becomes more intricate than if the perceptual action alone determined the final response. But this complication, no doubt, represents a common situation in our behavior circumstances.

Not inappropriate is it to add here that, besides the feeling reactions which we have called final and precurrent responses, affective reactions also occupy other positions in behavior segments in such a way that they must be called by a distinctive name. To such reactions we apply the term byplay responses. These byplay activities are incidental feeling reactions which are attached to behavior segments but which do not determine or condition a final response in the segment in which they are found; nor do they themselves constitute a final form of psychological adjustment; they merely operate along with other reactions, a condition made possible by their peculiar morphological character.

V. Are There Implicit Feeling Reactions? Because feeling reactions always comprise decided changes in the person's behavior (a fact which is symbolized in the statement that feeling reactions are definitely overt in their morphological character) the problem arises whether affective responses can be implicit in their functioning.

On the other hand, just because feeling responses are actions upon or in the organism, they can all the more operate without the presence of any original stimulus object, that is, they function through the stimulation of a substitute object. Now if we accept as our criterion of implicit action the operation of a given response to some definite object or person when that stimulus object is not present, then we are impressed by the fact that nothing is more common than the arousal of affective reactions by substitute stimuli.

How frequently do we observe a profound depressive affection produced in us by some person or object which substitutes-for, or is the symbol of, some past sorrow or perhaps some grief or disgrace situation! Such a response is a typical implicit reaction. Or again, what is more common than to be pleased or exhilarated by a letter, a friend, a garment or some other substitute stimulus for an adored person toward whom we have acquired affective or feeling responses? The range and complexity of implicit affective activity is revealed to us by the fact that to a considerable extent such behavior constitutes the psychological basis for religious, sex and other fetiches. To consider the bare character and use of fetiches is to appreciate immediately the implicit character of the reactions to them.

We might make the passing observation that implicit feeling reactions simulate much more closely the original overt affective reactions than do the non-affective implicit actions resemble the original overt non-affective responses. Morphologically speaking, the implicit feeling responses are always precisely like the original acts; but there may be a decided variation in intensity. We note, then, that implicit feeling reactions are never symbolized or symbolic in the sense that other types of implicit reactions may be; on the contrary, they are always incipient or vestigial. This latter fact, namely, that implicit affective reactions are very similar to the original overt responses, does naturally bring about a very decided limitation in the range of possible connections of such reactions with stimuli, but it does not in any sense interfere with their occurring at all. In other words, we have a great deal of implicit affective behavior, but none of it can ever be constructive in character. It can only be repetitive, that is, it is the repetition of previous reactions to stimuli, but not the construction of new forms of responses to absent stimuli.

The existence of implicit affective responses solves the problem of affective memory. Often has the question been propounded whether we can have affective memory, since it was supposed that memory consisted of the operation of images which were presumed to be cognitive and symbolic. Now from the standpoint of this present objective description of affective reactions we see that we can very readily recall or act over previous affective behavior.

VI. How Many Kinds of Feelings Are There? In the past history of psychology, when it was the tradition to make psychological facts consist of mental states called awarenesses of things, the whole series of affective reactions was reduced to a very small number. Most frequently it was to two, pleasure and unpleasantness. Sometimes other pairs were added, strain and relaxation, calm and excitement. Possibly we can discover two conditions governing the reduction of all kinds of feelings to so few kinds. One of them was that in the case of cognitive reactions the names of things were made into states of mind, and because there were so many different kinds of things to be aware of a great number of different states of cognitive mind were enumerated. Now having given the names of the qualities of things and of the things themselves to cognitive states it was presumed, apparently, they could not be further employed as affective terms.

The second basis for the view that there were only a few kinds of feeling responses lies in the fact that the psychology of feeling was developed mainly by ethical writers, who looked upon feelings as the springs of human action. The supreme categories employed by such writers were good and bad; consequently they attempted to find feeling qualities which could be correlated, with this good and bad. These they found in pain and pleasure. When the psychologists took over this conception, they of course did not connect pleasantness and unpleasantness with an ethical condition of goodness and badness, but with a furtherance and hindrance situation within the life of the organism. That is to say, they connected pleasantness with the kind of situations furthering the life activity, and unpleasantness with opposite sorts of situations.[9]

From the standpoint of the actual observations of human behavior we find that as a matter of fact the names historically employed for feeling reactions, such as calm, excitement, strain, pleasantness, unpleasantness, relaxation, etc., may be readily accepted as symbols for particular affective responses, as well as for types of affective reactions. No list, however, that is easily or quickly made can possibly exhaust the specifically different individual and class responses. That list must run into the thousands at least.

Impossible it is to solve the problem of the number of the kinds of feeling reactions that we perform unless we pay attention to at least a great many of the particular affective responses that we make to the great variety of stimuli arousing in us affective or feeling responses. Now the reactions induced by different stimuli conditions differ very materially in their morphological organization and specific psychological function. When we take the attitude that we are studying concrete facts of adaptation to specific objects and conditions we cannot for an instant be satisfied with any sort of limited list of affective activities.

Nor can the idea that there are only a very few kinds of affective processes be justified by asserting that after all only a few distinct affective qualities exist but that these can be mixed with cognitive and other processes to make a large list of complex feelings. For we submit that such an assertion obviously involves merely a logical proposition and is not based upon a descriptive analysis of specific behavior or reaction facts. Our suggestion is that even the simplest affective behavior phenomena comprise a large number of individually and typically different actions.[10]

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[9] It is without doubt the internal or organic character of affective behavior which gave plausibility to the furtherance-hindrance view of the biological psychologists (Spencer, Bain, etc.). And because of the element of truth in this view it must not be summarily rejected.

That the proposition to which we have just been objecting is purely logical and not descriptive is evident from the implication that these qualities are always direct opposites. Now the actual study of feeling activities in no sense supports the view that feeling responses all fall upon one or a few two-dimensional scales. An excellent argument against the logical view of the number of feeling types has always been contained in the problem which the upholders of that view had to face in the fact of mixed feelings.

If the feelings are made into simple states which are absolutely opposites, then logically it would appear that you could not have, say, pleasantness and unpleasantness, or strain and relaxation, in the same reaction. Such a logical conclusion of course clashes violently with the actual fact; for in our affective responses, at least sometimes, we have a few or many different kinds of feelings. When we look upon feeling reactions as definite adaptation responses to stimuli there is no fact militating against the inclusion in one behavior segment of both pleasantness and unpleasantness, strain and relief, and other kinds of reaction usually considered as opposed. Moreover, many of our simple feeling responses, each one of which involves only a single affective reaction system, are of such composite qualitative character that we may appropriately apply to them a number of supposedly opposite names. Certainly every one of our complex affective responses, say, to social or aesthetic stimuli, is constituted of a number of different sorts of qualitative reactions. We might describe our responses to aesthetic objects as pleasant with respect to some feature, and unpleasant with respect to some other, calming in some ways, exciting in others, and so on throughout an extended list of affective behavior.

A great obstacle to psychological analysis and description is the poverty of language. This condition is magnified in the case of feeling behavior. Whether this language difficulty is the cause of the intolerable affective classifications or whether it is the effect of it we cannot be sure, but the fact remains that we do not have enough names to describe or to indicate the multiplex and multiform affective responses. In view of the impossibility of estimating even the probable number of distinct feeling reactions we perform, we might suggest a few word symbols which represent unmistakeable simple affective responses and cannot possibly be classified under the heading of pleasantness or unpleasantness. Such names are elated, ravished, delighted, displeased, pleased, horror, disgust, disdain, satisfied, unsatisfied, dissatisfied, longing, libidinous, apprehensive, uneasy, catatonic, anatonic, manic, mirthful, downcast, disturbed, vexed, flustered, contented, etc.

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[10] We assume in this discussion that we may be allowed to interpret the term mental state, employed in the assertion referred to, as meaning responses.

VII. Simple and Complex Feeling Behavior Segments. Already we have indicated that we might look upon some of our affective behavior as comprising only single reaction systems, namely, in the case in which a feeling reaction determines a final response in a non-affective or affective reaction segment. On the other hand, the affective response may itself constitute a behavior segment, and so it may well be that the large number of qualitatively different feeling activities may indifferently comprise either simple reaction systems or behavior segments. In other words, the qualifying terms may apply to both the simpler and the more complex behavior units. Now we want to point out that when the affective phenomena constitute behavior segments they may be classified into two different sorts, namely the simple and the complex.

By simple affective behavior segments we mean the type of behavior in which the stimulus situation is comparatively simple and the response is a correspondingly uninvolved action. The perception of the stimulus merely brings about in the person some sort of immediately final feeling response. Probably the main characteristic of such a reaction is that the response is more subtle than in the more complex behavior segment and approximates more closely to a condition or tone of the person. Consequently, in many cases, the response appears to the observer as a less definite adaptation to a particular object or situation.

The principle of division between the simple and complex behavior segments (let us observe) refers to the simplicity of the total segment and not to the final reaction system which gives the segment its affective character. The point may be made therefore that the final reaction system itself need not necessarily be simple, but may on the contrary be very complex. Of course, no matter how complex the final reaction system is, the total behavior picture presented by the behavior segment in which this reaction system is found will be comparatively simple.

Complex affective behavior segments are characterized by the presence in them of one or more implicit or overt reaction systems besides the affective final reaction. In other words, the entire behavior segment is much more complicated owing to the more involved stimuli conditions. Not only must the individual perceive the stimulus but he must also do more than merely react with internal mechanisms. In listening to music one may perform merely single behavior segments to the obvious harmonies and single melodies; but, on the other hand, the definitive affective responses may be conditioned by an elaborate analytic and discriminative response to the intricate organization of the nuances constituting the musical stimulus.

Another way in which affective behavior segments are complicated and elaborated is by comprising many affective reaction systems in addition to the other precurrent responses found in the behavior segment. By this statement we do not mean to imply that some behavior segments as a matter of actual count contain only a simple affective reaction system. Rather we should be disposed to think in terms of numbers of different kinds of specific feeling reaction systems.

Further to differentiate between the various types of affective reactions we might consider a number of particular classes under each of the general headings of simple and complex action. These classes are arbitrarily chosen for illustrative purposes and it might be observed that the illustrations of our simple affective behavior segments will indicate a greater prominence of the reaction side of the segment while in the case of the complex behavior segment the illustrations will feature more prominently the stimuli conditions.

A. *Simple Affective Reaction Systems*

(1) *Expressive Behavior Systems.* These are situations in which the individual's behavior becomes more intense and on the whole he has an arousing feeling response to some specific thing in his surroundings. In other words, provided we have the coordinate reaction systems in our equipment, a sunset, a picture or some person may arouse in us a reaction of heightened tension, of increased affective inclination or of a changed internal and external organic posture, as contrasted with some previous condition. We have purposely considered here the simplest type of affective response, but we must stoutly assert that we are dealing with organized reactions to specific stimuli and not merely with hygienic conditions of the person.

(2) *Depressive Behavior Segments.* In these types of behavior situations the individual is reduced in tone or sensitivity to his surroundings. He does not feel as intensely as he did in some immediate previous condition or in some more remote prior situation. Particular situations such as accidents, the futility of our conduct, or some disappointment may induce in us an immediate withdrawing and slumping feeling. Likewise ugly or disagreeable objects, or things with which we have had some previous unfortunate or undesirable contact, arouse in us feelings of a cataphoric sort.

Every different stimulus situation, as we have so frequently repeated, calls out its own peculiar depressive affective response. In consequence, there are a great many of these depressive behavior segments. Especially important is this statement, for if we carefully heed its significance we may avoid serious error of interpretation. In the first place, we shall not overlook the fact that in every case of affective behavior we are dealing with a concrete response of the person to a stimulus. And in the second place, we may avoid the error of looking upon affective behavior as made up of opposites. For, as we have already indicated, to consider modes of affective behavior as opposites means to look upon them as logical or abstract conditions or kinds of reaction. And so, while it is true that we may arrange some types of our affective reactions in a two-dimensional scale, this must be considered as a classifactory scheme and not as a descriptive procedure. From the point of view of our actual behavior it can hardly mean anything to put expressive reactions over against depressive responses as opposites.

(3) *Euphoric Behavior Segments.* Quite different from the heightened or lowered tone of the individual's reaction to stimuli are feeling situations in which the person's contact with stimuli of various sorts induces in him reactions described as well-being, reactions which place him at ease, in a condition of calm, and withdraw him from any kind of intensification or depression. A number of different responses involved in sex gratification admirably exemplify these reactions. The English words pleased, satisfied, contented, placid, surfeited and serene may symbolize for us various reactions which can be placed in this class.

(4) *Cacophoric Behavior Segments.* Here we have a type of reaction describable as a lack of calmness or satisfaction. The stimuli disturb the person and remove his tranquility and serenity. Further descriptive terms would be displeased or unpleased, anxious, amazed, apprehensive, and others indicating disturbed relations between certain objects and the reacting person.

Despite the fact that the four kinds of affective responses we have just been describing may well be thought of as different each one from the others, yet it is true no doubt that in many cases a single affective behavior may better be described under two or more headings. Thus, some behavior segments may be elevating, depressing and pleasant all at the same time. The exact description in every case depends of course directly upon the stimuli objects and conditions.

B. *Complex Affective Behavior Segments*

Not only are the behavior segments of the complex form more complicated because of the presence in them of more and different systems, but also because the individual is in a more active contact with the stimuli objects. What is meant by being more in contact with stimuli we have already indicated in a former section. The responses in these cases are much more describable as adaptations to the stimuli than as effects which the stimuli have produced upon the organism. It would naturally follow that the stimuli conditions in the complex affective behavior segments are correspondingly more definite and complicated things or situations. In general, we might say that such stimuli are found in the more distinctly human situations constituting the milieu of our conduct.

For convenience of illustration we might suggest that such situations constitute what are conveniently referred to as the aesthetic, political, religious, social, sexual, economic, moral and intellectual situations. To illustrate, religious stimuli may produce in us complex feeling reactions which may be named dependent, hopeless, hopeful, awed, sacred, and purified. Aesthetic situations induce reactions of admiration, ravishment, appreciation, transport, etc. Moral and social stimuli are correlated with reactions of sympathy, antipathy, pity, while personal situations may induce love, hate, intolerance, sympathy. Intellectual and practical situations produce reactions of satisfaction, accomplishment, completion and numerous others.

Once more must we be on our guard in the use of our crude and inadequate linguistic tools in psychological description. In the first place, we should note that any name used for an affective reaction does not belong exclusively to a single one of the behavior situations we have mentioned, or to which we have made more general reference. Unfortunately, because of the lack of practice in the analysis of affective responses and the symbolizing of them by words, we cannot be sure that any particular term may not refer to responses which are common to many kinds of situations, even non-affective. For example, the term disgust may be used to describe a feeling reaction either to intellectual or to aesthetic stimuli. On the other hand, within the range of any one of these situations, say the moral, we shall find a large series of reactions to which we apply the same term. Our main concern is to be certain that in each case we are not overlooking the fact of a specific form of response to some specific stimulus.

Again, it is exceedingly important for us to make sure that each term we use does refer to some specific organized affective reaction to a stimulus and does not symbolize some sort of social conduct other than a feeling response. What we are describing or referring to now are exclusively feeling reactions and not other kinds of behavior which may be very closely attached to, even inseparably associated with, some feeling activity.

A further caution is not to confuse our specific affective responses with large behavior situations which may involve all kinds of behavior segments along with non-psychological facts. For example, we must guard against confusing a particular complex affective behavior segment with, say, the behavior sitituation of purchasing a picture. In this reaction there are many affective reactions, but also we find many others which are distinctly non-affective in character. Again, there may be in that situation a lot of facts which are not even psychological in character, for instance, facts relating to economic values, or aesthetic importance of aesthetic objects,—facts, that is to say, which are not directly involved in psychological action at all.

VIII. Degrees of Intensity of Feeling Responses. In all of our responses we may observe a variation in the intensity of action depending upon the degree of energy expended in their performance. That is to say, whenever we perform a specific response to a stimulus, the general hygienic and other conditions of the person may bring about a more or less intense contact with the stimulus object in question. Such modifications in the physiological or biological status of the person not only make him especially susceptible to certain stimuli but intensify or slacken his reactions. This is of course mainly observable in overt responses in which definite changes are produced in the object. This variation in the intensity with which any action is performed gives us in the case of affective responses a more striking picture of the behavior situation than is true in other kinds of activities. In fact, to a certain extent, when the same affective reaction system operates more or less intensively, there is a superficial appearance of an entirely different kind of behavior picture. We might immediately suggest that this apparent variation is due to the fact that the total affective response is much more intimately related to the person's biological status than is true of other types of behavior.

Such variations in the intensity of operation of affective responses are made possible, then, by the intimate organic character of affective responses. But what are the specific stimuli conditions which bring about these results? Quickly we observe that such changes of intensity depend upon (1) the way the stimulus object is presented, (2) the nature and condition of the stimuli settings, and (3) the length of time that the person has had the affective response included as a part of his behavior equipment.

(1) If the same stimulus object which ordinarily calls out some particular affective reaction is presented suddenly or more gradually than is usually the case, the affective reaction constituting the response to that stimulus may be so much more or so much less intense as to give on the whole a different behavior picture. Especially we must emphasize the point that the response, whether it is a simple action or an affective reaction pattern in a more complex situation, falsely seems to become a qualitatively different kind of activity, merely because of the greater or lesser intensity of the response. How many times has the young man preoccupied with business or other situations been accused by his beloved of not entertaining the same feeling toward her, whereas, in fact, there has been no difference in the quality of his reaction but only in its intensity? Such extreme variations of behavior picture are unique in the case of affective responses.

(2) According to the specific variations in the setting of objects which stimulate affective responses, they will be more or less intense in their operation. And here again, as in the previous case, an apparently different behavior picture is presented by the acting individual, although in actual fact the activity is in its qualitative description the same as in its customary operation. In general, we might indicate that in many cases an individual performing an affective reaction is much more sensitive to variations in the settings of objects than is true of the same individual while performing non-affective responses.

(3) The passage of time between the acquisition of some affective response to an object and the later successive performances of that reaction makes a difference in the intensity of that affective response. The feeling reaction becomes either more intense or less intense as the case may be. This phenomenon is very familiar to us under the heading of adaptation. It has long been known that our activities such as feeling behavior become adapted to situations so that the intensity becomes reduced. Now the other type of modification, namely, the increase in intensity of an affective response, may also occur with the passage of time, during which the acting person has changed organically or physiologically with respect to the particular action mechanisms which operate in this behavior system.

In effect, we have been pointing out in this section that affective reactions involve two sorts of intensity. One of these types constitutes a genuine qualitative difference in the affective reaction system, which displays to us a distinctly observable behavior picture, but which does not constitute a different type of reaction system. We must consider that the stimuli for these somewhat qualitatively different responses are the same. On the other hand, we have a type of intensity difference which represents a quantitative variation in behavior; that is, it is merely done with more energy and seems to indicate no sharp difference in the behavior picture. This second type of intensity of variation is common to both the affective and the non-affective reactions.

IX. The Stimuli for Affective Responses. Investigations of affective behavior indicate that the stimuli for such action must constitute an exceedingly large series of objects, situations and conditions. The truth of this observation appears in the following considerations. In the first place, it is unquestionably true that because of their character affective phenomena constitute responses to a more varied array of stimuli than is the case with other types of reactions. For is it not true that whatever objects or conditions can serve as stimuli for other kinds of reactions can also be stimuli for affective responses?

In the second place, we must add that the affective stimuli are far more numerous than the combined stimuli for several classes of reactions. For example, affective stimuli are more numerous than those for intellectual reactions, imagination responses, and all the activities of the person involving him in situations more or less detached from organically intimate behavior. It is a matter of common knowledge that most people less rarely perform thinking or problem solving reactions than feeling responses even to the same objects or situations. On the other hand, there are some objects which constitute stimuli peculiar to feeling reactions and do not at all serve to arouse other kinds of action.

Thirdly, because affective behavior segments can be both simple and complex, the number of stimuli within a definitely feeling domain must be larger than in a behavior domain in which no such division of activity is possible. All this means of course that the affective stimuli cover an exceedingly wide range of all the myriad objects and situations that prompt human beings to perform psychological conduct.

By way of enumerating some of the more prominent things serving as affective stimuli we will consider them according to their division into types.

(1) *Objects as Affective Stimuli.* Among the stimuli objects inducing affective responses we may suggest all types of things from the simplest natural objects to the most complex cultural objects, such as art works and social institutions. Furthermore, we find that certain qualities alone of both simple and complex objects can serve to arouse affective reactions. For example, the colors and tastes of fruits or flowers, as well as the whole objects, may induce pleasant, unpleasant or other kinds of feeling reactions. Similarly the specific sounds in music may produce exciting, depressing or other kinds of feeling reactions, as well as the total musical effects. Or possibly specific chords or discords may arouse an individual affectively as well as the entire composition. Again, the intensity and brightness of colors, as well as of tones, may produce varying forms of feeling activity. The same thing is true of the size and intensity of things. Contrasts of objects and of their qualities, such as their colors, and rhythms of all sorts in the visual and auditory objects, are fertile sources of affective stimulation. The same thing may be said for the non-rhythmic changes in the appearance of things and the alternate presence and absence of objects, such as light flicker, etc.

Including persons under this type, we need hardly mention the many qualities such as colors of eyes, slenderness or otherwise of figure, etc., which induce in individuals specific feeling behavior. Here we think at once of the sex factors, although we must not fail to observe that sex stimulation is unique and the affective responses are differential with respect to particular qualities and actions of persons. Almost infinite in number are the affective responses we perform to the human and cultural attributes of persons in their various capacities and in their interrelationships. Exceedingly prominent are our affective responses to persons which have as their stimuli the linguistic, religious, political, social and intellectual traits of people from different national groups or different strata of the same national groups.

An extremely interesting question arises here, namely, whether on the whole our most complex affective reactions are stimulated by complete objects such as a picture, a person, a statue or a symphony,—in other words, by some large aesthetic or non-aesthetic object,—or whether the several qualities of complex objects can just as well individually be stimuli for complex feeling responses. Possibly there is no definitely fixed correlation, but rather in some cases either the parts or the wholes of objects can be the stimuli for either simple or complex feeling responses.

(2) *Events as Stimuli.* Events of all sorts constitute a very prominent source of stimuli for feeling activity. For the simple affective reactions we find as stimuli the various happenings to physical things, such as injury to a book, or loss of some minor possession. Simple affective reactions are also called out by events involving happenings to persons who are not connected in a personal way with the reacting individual. For instance, an accident involving a stranger will probably call out a simple feeling response on the part of the onlooker rather than a complex one. The more complex and intense affective reactions are usually made to incidents involving persons near to us or disliked by us, and by the more complex affairs of national life, deaths, wars, strikes, famines, etc.

(3) *Conditions as Stimuli.* Whenever the person meets with a difficult or obstructing condition with respect to something that he wants to do, such as some unfavorable weather condition, then there is promptly induced in him some affective reaction. Any kind of hindrance in one's activities, or on the other hand the appearance of conditions favoring such activity, constitutes a stimulus for the operation of feeling reactions. These conditions are constantly present as affective stimuli. We have already had occasion to refer to the general stimulating character of change for affective responses. Now we may point out that the length of time that some event endures may be considered as a stimulus condition for affective behavior. Thus when one is looking at a picture which disappears too soon, or stays too long, the result is an affective response of an unique sort.

Changes in the individual's own condition are a common source of affective stimulation. Such changes may be direct, in the form of hygienic modification, or they may be indirect, through the indulgence or non-indulgence in food, drink, poisons, or by being touched, tickled, or perhaps injured or at least pained (modify tissues).

(4) *Actions as Affective Stimuli.* One of the commonest sources of affective stimuli are the actions of the person himself or of some other. Any successful act may be a stimulus for a pleasant reaction, perhaps, or a calming or exciting one, while unsuccessful action is of course accompanied by unpleasantness and other types of feeling responses. For instance, the successful or unsuccessful manipulation while solving a problem stimulates very decidedly a favorable or unfavorable feeling condition. The recalling of an unpleasant or pleasant situation that the person has participated in likewise produces such reactions, or the inability to remember what one wants to recall, for example, a name.

Especially important among the person's own actions which stimulate affective responses are the general implicit responses constituting the mediate adaptations to all sorts of objects and situations. Not at all negligible as affective stimuli are prior affective reactions of the acting person himself.

Among the more simple action stimuli we may name those induced by our own behavior, namely, eating, dancing, conversing, or reflex action of a sexual or general visceral sort. Here we find a very common and pervasive source of affective stimuli.

Among the affective stimuli constituting other persons' behavior is the action of individuals intimately connected with us, or upon whom we are dependent in various ways. Our affective responses to the acts of such closely related individuals would simulate very closely the responses to our own behavior. For instance, we are fearful for them when they do something harmful to themselves, or we become especially pleased when they do something favorable to their own advantage and social estimation. Again, the actions which disgust us when unrelated individuals perform them would not produce in us such negative affective responses when performed by related persons, especially if such actions arouse no negative affective response when we perform them ourselves.

These suggestions concerning the stimuli for feeling reactions must appear exceedingly fragmentary and incomplete, but they are here set down by way of emphasizing the importance of stimuli in affective conduct. While stimuli as components of behavior segments are always important, in the case of affective reactions especially, the specification of particular stimuli indicates the organized and directed character of feeling behavior.

X. Influences Upon Feeling Reactions. In common with all psychological responses affective reactions are subject to various influences which determine their precise operation aside from the stimuli and their settings. But unlike other sorts of activities the organic character of affective responses makes these influences much more pronounced. Of these influences there are many and all aside from those concerning the person's general physiological and hygienic conditions which we have already discussed in Section VIII.

Our present task, then, is to indicate some of the more prominent influences upon affective action arising from the individual's own reaction circumstances.

(1) To begin with, it is popularly recognized that the type of response a person undergoes immediately before the performance of an affective reaction will distinctly affect its nature. This influence is apparent both when the prior action is affective and when it is non-affective. For instance, if we have just been experiencing a depressing situation our following affective response to some other situation will not be nearly so expressive or so intense as it would have been had the immediately previous experience been a pleasant one. Or some types of previous affective situations may intenstify the following behavior. On the other hand, the same or similar effects upon affective responses may be produced by intellectual preoccupation or by some other contact with non-affective stimuli.

(2) Again, we readily see the determining influence that the individual's general behavior equipment would have on his affective responses. In the first place, the person who has in his action make-up feeling behavior of various kinds is liable to have affective reactions of a particular sort with respect to some specific happening, while the persons lacking those behavior systems in their equipment would have different or possibly no affective responses to that event. To illustrate, an in-

dividual who has been constantly stimulated to be tender with animals will have a decidedly more intense affective reaction when witnessing the maltreatment of some animal than another person who has not built up such feeling traits.

In the second place, the individual who has a large complex of different types of behavior systems in his equipment is more likely to have affective reactions connected with the objects correlated with those reactions than is the individual with a generally meagre set of behavior traits. Most certainly the person having information and art techniques is much more subject to affective reaction with respect to intellectual and aesthetic stimuli than another individual who lacks such techniques. This same fact holds good for all types of behavior equipment coupled with complex affective reactions.

Impossible it is to overemphasize at this point the cultural character of our affective behavior. Owing to the emphasis of the organic and biological factors of affective mechanisms we are prone to overlook the essentially conventional character of most of our feeling responses. But as a matter of fact, although in every reaction we must operate as animal organisms, yet probably ninety percent. of all our reactions are cultural or conventional in character. So far as our affective conduct is concerned we might suggest that since every individual is a product of a specific group, of a particular culturalisation process, his affective responses will inevitably be conditioned by the fact that he acquired his equipment in his particular group. Now since these groups or communities include varying social and intellectual centers, individuals will materially differ in their number and type of aesthetic, moral and other affective responses. Furthermore, since the different culturalising groups are not merely different strata of a given community but large cultural centers in actually different communities we have a basis for national and racial affective traits. The former affective reactions, namely those representing particular individual differences, may be illustrated by the popular notion concerning the physician's stoicism with respect to suffering as contrasted with the rest of the community. National or racial affective traits are represented by such characteristics as the insensitivity of the Romans toward death, compared with the individuals of other national or racial groups. Naturally we do not believe that the cultural differences and characteristics of affective behavior refer to the "expression" of feelings and not to the "feelings" themselves, for obviously upon the basis of our discussion no such differences exist. On the other hand, if our view concerning the cultural character of affective responses is acceptable, it reinforces the description of affective phenomena as concrete responses of the person to specific stimuli.

Briefly to sum up the influencing factors originating in the person's behavior we may call the two main modifying determinants (1) immediate influences, those resulting from behavior immediately performed previous to the affective behavior segment studied, and (2) permanent influences or those resulting from the deep-rooted behavior systems of the individual.

XI. The Conditioning of Feeling Reactions. Of all the reactions which we perform probably the affective responses are most easily conditioned. That is to say, the specific reaction systems can be detached from stimuli in contact with which they were originally developed and reattached to newer and different stimuli objects and conditions. This increased susceptibility to conditions obviously belongs to affective responses because of two of their primary characteristics. First, since affective reaction systems involve primarily the operation of internal mechanisms, they possess a sort of independence of the stimuli. Again, in performing our affective reactions we do not change our stimuli; and in consequence, while every affective response is a specific adaptation to a particular stimulus, the relationship between the responses and stimuli is such that probably they are more subject to be interchanged.

This entire matter may be otherwise stated by suggesting that a peculiarity of affective behavior segments is that they are capable of being interchanged in various ways. That is to say, the stimulus of one may become attached to the responses of the other and *vice versa*. Possibly this fact may be well symbolized by the statement that in the affective behavior segments the response and stimulus factors seem to be less rigidly related than is true in other types of behavior.

This characteristic of conditioning, while probably attaching to all affective behavior segments, is doubtless of more frequent occurrence among the simpler feeling activities. And so while it is very common for us to transfer the simple euphoric responses induced by the brightness of the day to various other stimuli objects and conditions, we probably less frequently similarly transfer any more complex affective actions. Illustrative of the transferred complex feeling response is the case of the person who is stimulated to love some individual and who transfers his affective reactions to objects belonging to this loved person or to others associated with him, even including a whole family, nation or race. Similarily, we carry over our antipathies and enthusiasms from one stimulus object or situation to another.

An individual who becomes enthusiastic about some painting may condition his affective response by connecting it with all the artists of that school although not all the members have the qualities to which he originally responded. Without doubt much psychopathic behavior consists essentially of the unadaptable conditioning of affective responses.

Affective conditioning processes may be considered to consist of two general types, one of which is not a genuine psychological phenomenon, though it has every superficial appearance of being such. We discuss each in turn briefly.

(1) Pseudo-conditioning processes are those in which there is no actual reattachment of a response to a new stimulus. On the contrary, the affective response just seems to persist and to be indifferently associated with various objects, as if it kept acting on its own momentum. Such pseudo-conditioning situations are exemplified by the actions of the person who allows his antipathies to be carried over from one individual to a whole group, without actually responding to each one of the group separately.

Now just here lies the criterion for spurious and genuine affective conditioning. In genuine conditioning some actual response to a particular stimulus object is reattached to another stimulus object. In spurious conditioning the actual reattaching process is absent, except in appearance. That is to say, there really is no new stimulus to which the old response could be attached. In actual practice the genuineness of the conditioning process is frequently far from being easy to determine. Especially must we be careful in situations in which the transfer is from some particular person to some abstract stimulus such as his profession. Such genuine but ostensibly spurious conditioning processes are common in various forms of insanity. These spurious affective conditionings may be very descriptively designated as feeling illusions or affective maladaptations.

(2) Genuine affective conditioning processes may be differentiated into two sorts, depending upon whether the reattachment is to a new object or to the same object. Any object may ordinarily of course constitute two or more specific stimuli for different reactions. Similarly the same reaction may become detached from one of the two stimuli which a single object or person constitutes, and reattached to the other. Or the reaction need not of course necessarily become detached from the old stimulus, but may be attached to the new stumulus in addition. A striking illustration is the pleasurable feeling induced in an individual by some person in one capacity, say as anatomically attractive, which same reaction may later be attached to the person as a friend, that is, may remain as the response to both.

Genuine affective conditioning, when the stimuli are different objects, is exemplified in the case of a woman transferring a particular affective response to her first child to the later ones, or in the transference of a love reaction from a child who has died to some adopted infant or even to an animal.

XII. The Place of Feeling in Behavior Situations. Such intimate behavior phenomena as feelings must inevitably play a very large part in all of our behavior situations. That is to say, they must, because of their peculiar character, be very pervasive in the various activities of the individual. What we have to point out here is that in addition to having affective reaction systems operate in both affective and non-affective behavior segments, affective processes whether reaction systems, reaction patterns, or complete segments, must and do play a very large part in our behavior situations. By the latter we mean a larger behavior unit than a segment; in other words, we mean a series of segments. In non-technical language we might say that practically every definitely human type of behavior involves some feeling reactions.

To illustrate, practically every one of our reactions to human beings comprises some sort of affective response, especially if it is a person of the opposite sex and if the stimulus individual is not exceedingly familiar to the reacting person. Also in aesthetic, religious, or sports situations we may distinctly observe a number of more or less complicated feeling activities. Possibly the observation we are making here has been the factual basis for the idea very frequently expressed in psychological literature that the feelings appear to be attributes or tones of other kinds of processes. Certainly, we find feeling activities exceedingly pervasive and also capable of thoroughly interpenetrating with other kinds of activity.

The operation of affective responses in what are essentially non-affective behavior situations may be illustrated by considering a few varieties of complex behavior. To turn first to an aesthetic behavior situation we will consider the constructive action of creating a work of art, a poem or a picture. Such activity we might consider as essentially imaginative in character; in different words, the primary response is the development of an aesthetic conception and its later execution. Such activity also may be designated as a type of social behavior, since the fact of making any particular object, and its execution in a particular kind of medium, are conditioned by a series of group ideas, sanctions and stands, as well as other sorts of specific group institutions. Never can such complicated constructive art behavior be carried out, however, without numerous correlated affective responses.

From the object which is painted or from the person and events about which the poem is created the artist receives many stimuli for the performance of many simple and complex affective reactions. Present here are the calm of admiration, the excitement of enthusiasm for the work or subject, and sexual feeling for the subject the artist is handling.[11] In addition to these there is present the general euphoric response with respect to the progress or success of the accomplishment.

The same or very similar kinds of affective behavior can be traced out in the appreciative or impressive types of affective conduct as well as in the constructive. Doubtless it is the operation of these affective responses, coupled with imagination reactions and others, which constitutes the behavior basis for the empathic or *Einfühlung* phenomena of aesthetic behavior.

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[11] The writer hopes it is unneccessary to indicate that this statement has nothing in common with the view that art is the sublimation of sex urges.

Much the same type of combination of affective and other forms of behavior can be traced out in the case of moral action. In the solution of moral problems, for instance, we also find these complex affective components giving character to the total behavior situation no matter how complex the latter may be. An essential difference between the moral and aesthetic situation may be pointed out in that the latter need not be so essentially imaginative in character but may involve more thinking and problem solving activity. Also in other cases, as well as in moral action, we can trace out a large number of memorial and habit activities of various sorts.

Especially important is the observation that the complicated meaning and understanding behavior situations which function to adapt the individual in his most complicated activity are veritably replete with affective elements. The meanings of things, the comprehension of objects and situations, are complicated behavior activities containing a goodly portion of affective reactions. And just as in the case of the aesthetic situation, color, shape, etc. condition the total behavior situation, so do such factors likewise influence the significance of things and persons from a reactional standpoint.

Just how the affective response factors operate in meaning activities may be pointed out in the following manner. Let us assume that, when we have our first contact, or perhaps even some later contact, with some object, this contact results in a pleasant or unpleasant feeling reaction. Now meaning responses are essentially behavior which conditions some other reaction connected with the first one. If the pleasantness or unpleasantness resulting from the contact of the person with some particular object or situation induces a particular kind of response action with respect to that object, which response will later condition other reactions, then we see how the pleasant or unpleasant affective response conditions the development of meaning. This development, of course, is merely preliminary to the future operation of this meaning activity. Our point is that in its development the meaning was greatly influenced by the affective response and the latter constitutes then a permanent factor in that meaning situation.

XIII. Varieties of Feeling Behavior Segments. Certainly by all relevant criteria, and especially because of possessing unique characteristics, affective behavior segments constitute a specific type of psychological activity. But, on the other hand, affective reactions are not thus excluded from participating in the qualities of some of the non-affective reactions. And so we might point out that the feeling behavior segment consists of reflexes, habits, volitional and other types of psychological behavior. Quite definitely do the barriers between different classes of behavior break down here in the sense that a particular reaction may at the same time be affective as well as volitional or habitual as the case may be. How then may we distinguish between the particular types of affective reactions? Most effectively can we do so by briefly describing each one.

(1) *Affective Reflex Action.* Reflex responses naturally constitute the simplest type of affective behavior. These responses consist of a simple unique reaction system correlated with a particular stimulus object or condition. Such comparatively simple responses depend upon the biological organization of the acting individual.[12] As a matter of detail we might point out that affective reflex reaction systems consist primarily of the operation of visceral mechanisms. Affective reflex actions then appear clearly to be classifiable as members of the general class of reflex responses. But here we must be on our guard lest we wrongly conclude that, because all affective reaction systems involve predominantly visceral mechanisms, all feeling responses are reflexes. Such is far from being the case. For, as we have abundantly seen, not all affective responses are so simple as to involve merely a single reaction system. Whenever we have a response pattern (more than one reaction system) we must place the reaction in a different class. We shall then have habits, volitions or other sorts of behavior. For example, even a fairly simple sex feeling response must always be connected with social or informational responses in the same behavior unit or segment. Consequently, even such comparatively uninvolved affective reactions must belong to another class of action than reflexes. As we have intimated in a previous section, the question whether we must classify sex feeling reactions as affective at all is determined by the fact that the primary element is the feeling factor rather than the social or informational ones which are also included in the behavior segment.

(2) *Feeling Habit Reactions.* Some of our affective reactions are definitely organized responses which are clearly integrated very closely with specific stimuli. Such an integration process brings it about that the mere appearance of the object constituting the integrative stimulus results in the practically automatic operation of the reaction. Since we call such integrated responses habit reactions, affective behavior of this nature will answer to this description and we will call them feeling habit reactions.

(3) *Affective Volitional Reactions.* By volitional reaction we understand a behavior segment in which the final or definitive response is conditioned, not only by its ordinary stimulus, but in addition by other stimuli objects directly, or indirectly by such additional stimuli objects through reactions of various sorts. Many of our affective responses constitute such multiply determined responses, and therefore they must be placed in the class of volitional reactions. The feeling reaction toward a picture is determined not only by the picture itself but also by the knowledge of its painter, the understanding of its technique, etc.

(4) *Affective Meaning Responses.* We have already indicated above that feeling responses serve to condition the operation of further and final reactions in behavior segments. Now such a conditioning function we call a meaning process. And therefore, the determining affective responses must be called meaning reactions in addition to feeling activity. Here in this case it will be observed that only some reaction systems in a behavior segment, namely the precurrent determining reactions and not the final action, bear this name of affective meaning response; in this sense this class of feeling reactions differs from the others we have been describing. But our point that affective reactions may at the same time belong to other types of behavior is here no less cogent than elsewhere.

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[12] Cf. The Psychology of Reflex Action, this JOURNAL, 1922, 19-42.

Similarly our affective responses may be placed in several other categories. We might merely suggest in addition that affective responses may also be considered either as individual responses, in the sense of representing the unique reactions of an individual who has developed in his particular behavior situations an unique type of affective reaction, or the activity may be classified as cultural, that is to say, as we have suggested above, action in which the person has developed part of his behavior equipment because of some group prescription.

XIV. Classification of Feeling Reactions. Impossible it is to classify feeling reactions in any sort of logical or adequate fashion. The various forms are too complicated and too individual in their operation to permit any satisfactory organization; and yet these very conditions which make classification so difficult make it all the more necessary to inject some order into the multiform facts of affective behavior. Even the most unsuccessful attempt at organization would undoubtedly add something to our understanding of feeling responses and supply some measure of descriptive control over them.

When we attempt, however, to plan such a classification we immediately face a serious problem. What criteria from all the possible ones that present themselves shall we adopt as a guide? Here is an incomplete list of them, which we will offer in the form of questions. (1) Temporal condition; or whether the feeling response is momentary or prolonged? (2) Smoothness of action; in other words, does the reaction operate consistently and on a level throughout, or is there a climax? (3) Directness of feeling; is the reaction definitely directed toward some stimulus object or person, or not? (4) Localization; is the feeling reaction unmistakably localized with respect to some organs or some segment of the organism? (5) Is the action simple or complex? (6) Does it involve great activity or not? (7) Does the reaction appear independent of other behavior at the moment or does it accompany some other reaction? (8) Does the affective action represent one of acceleration or of depression and slowing down of the organism? (9) Does the behavior segment involve implicit reactions of other sorts? (10) Is the feeling reaction itself totally implicit? (11) Is the reaction itself of a reflex sort or of a habit type? (12) What particular sort of mixture of behavior is involved in the feeling behavior segment? (13) Is there a preponderance of some one quality in the feeling action, for example, pleasantness or excitedness, etc.? (14) What is the type of stimulus? (15) Is the feeling reaction subtle or crude and violent in its operation? (16) Is the affective behavior suffusing or overwhelming? (17) And, finally, does or does not the reaction represent the operation of some permanent reaction system (behavior trait) in the person's equipment?

Any attempt to classify feeling reactions must obviously be based upon some one or a group of these criteria. Moreover, whatever series of classes we adopt, each class must necessarily include thousands of members. The impossibility then of the classifactory enterprise in the field of the affective responses is manifest. What we shall attempt to do is to select from the more common forms of feeling reactions a few (ten) representative types. For class names we adopt terms in ordinary use, specifying, however, that such names need not be considered as exclusively and irrevocably adhering to the type of behavior with which we connect it. Follows then our attempt at classification.

(1) *Passions.* Here we have a highly organized, usually intense and active form of feeling response which may be momentary or prolonged, overt or implicit. Passions are especially directed toward some particular kind of stimulus object or person. Again, a passion response may involve much implicit behavior of non-affective types such as information, as well as a critical attitude toward the person or object serving as a stimulus. These implicit components of passion responses would comprise of course the anticipatory or precurrent phase of the reaction. To point to examples, we may cite the actions ordinarily referred to as hating, loving, revenging, despising, loathing, jealousy and fits of anger.

(2) *Orgasmic or Irruptive Feelings.* In this class we put (*a*) the feeling reactions ending in a very definite climax after a more or less prolonged operation, and (*b*) the feeling responses which start with a sudden change of behavior. For these responses the stimuli are mainly situations which have abrupt endings or interruptions, or objects which change rapidly. Such actions are performed while hearing music that attains to a climax or which begins with great volumes of sound. Also these reactions occur in watching games, in which there are striking gains and losses, and the person's feelings are thus suddenly initiated or interrupted. Similarly, in observing theatrical performances we may have many sudden irruptions of feeling responses; but probably the best example of orgasmic type of feeling reaction can be found in the sex reactions. For the orgasmic and irruptive type of feeling reactions we cannot find very definite word symbols in our language, but the expressions striking disappointment, overwhelming surprise, sudden indignation suggest the kind of behavior under discussion. In contrast to the passions, the orgasmic or irruptive feeling responses are more turbulent, fitful and less organized or well-knit, and in this respect they are closer to the emotions than any other type of feeling behavior.

(3) *Affective Sentiment Reactions.* Sentiment reactions represent very highly organized activities which typify acquiescence or readiness to do certain things or to refuse to do them. They are usually exceedingly complex and very intimately connected with complicated knowing and informational behavior. They are also for the most part developed and function as cultural or social modes of action. In each case sentiments are unmistakably directed toward some particular kind of stimulus object or situation. Probably most frequently the stimuli for sentiment responses are social institutions of the group behavior variety. Names symbolizing sentiment reactions are shyness, altruism, patriotism, modesty, shame, decency, indecency, cleanliness, charitableness, etc.[13]

(4) *Affective Dispositions.* The main characteristic of disposition reactions is the fact that the feeling activity when it occurs in response to its

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[13] None of these names refers to social conduct, but only to the particular affective responses of persons, but these affective responses may be social in character.

particular stimulus represents a practically permanent behavior factor of the person's equipment. Thus the dispositions may be considered to a certain extent as giving character to the person. The stimuli for these reactions may be objects, situations, and persons. Such dispositions are tenderness, hopefulness, optimism, pessimism, etc.

(5) *Affective Attitudes.* These reactions are contrasted with dispositions in representing an action of the moment and do not necessarily reach back to definite elements in the personality behavior equipment. By feeling attitudes we mean more or less prolonged responses with respect to particular stimuli which are usually immediately present. They constitute reactions, then, that are organized with respect to some particular kind of situation object or person which is usually present. These reactions may be exceedingly intense, such as some extreme happiness over some situation or a hopefulness concerning some event. Very good examples of affective attitudes may be found in aesthetic situations; to be enraptured in a work, inspired or repelled, represents such behavior and in ethical situations to be disconcerted or astonished by some one's action. When these expressions represent definite affective responses they may be considered as typical feeling attitudes.

(6) *Affective Moods.* Moods constitute feeling reactions consisting mainly of a simple type of slightly organized conduct. Qualitatively the mood reactions are in effect conditions of depression or elevation of the person, but these depressions and elevations are none the less organized and directed towards a stimulus object or condition, since they comprise a number of varieties differing with the arousing conditions. Because of the simple character of these reactions, however, they have for the most part been considered as merely conditions which color other types of action. Much plausibility attends this observation, since the comparative simplicity of mood responses makes them especially subject to conditioning. In consequence, they seem to belong indifferently to various kinds of behavior situations. Also moods appear as not directed and autonomous responses because probably for the most part they are stimulated by hygienic conditions of the person himself. Moods may be symbolized by the popular color-terms, blues, browns and rosy moods.

(7) *Continuous Feeling Responses.* A very prominent type of feeling reaction we are constantly performing are the affective responses of an unlocalized sort which accompany other actions and which to a considerable extent may be said to have their stimuli in other actions of the person. In these cases we have definite pleased, displeased, satisfied or unsatisfied, disappointed, apprehensive or other affective responses which parallel and accompany other actions. The essential characteristic of these continuous feeling reactions is that they are not independent responses; that is, they depend upon and are closely connected with other reactions. To illustrate the operation of these actions we might indicate that during a conversation we may have definite pleasurable, excitable or other sorts of feeling responses, depending upon the nature of the conversation and the sort of person with whom we are conversing. Such forms of feeling reaction may frequently amount to a suffusion of the individual with pleasantness or other sorts of feeling. Continuous feeling reactions are naturally not connected with objects as stimuli, but rather with series of objects or continuing action (conversation) and in general with events and conditions.

(8) *Diffuse Feelings.* In this class we place a large series of complex organized reactions the members of which have a longer or shorter duration in their specific situations. Although the responses of diffuse feeling segments are directly related to some stimulus object or condition, still the segment as a whole appears to feature the response indefinitely more than the stimuli objects and conditions. In other words, the reference to the stimuli objects is not very marked on the surface, especially when the total segment is of considerable duration.

Diffuse feelings usually constitute responses of a serious or important nature. They operate in situations which greatly affect the person. It is for this reason that the response appears so much more in evidence than the stimulus. Thus the responses are diffuse and apparently undirected. We might point out, too, that the most typical of these diffuse feelings have as their stimuli past or future situations or present conditions which are inevitable and cannot be controlled by the individual. This fact adds also to the diffuseness of these reactions. Well symbolized can such behavior be by the responses we call regretfulness, remorse, gladness, elation, dread, anxiety, ecstasy and resignedness.

The diffuse feelings may be differentiated from the continuous feelings chiefly by the fact that the latter are not independent responses but are always related to some other kind of response. Again, the continuous reactions represent slighter forms of behavior.

(9) *Localized Feeling.* By localized feeling we mean definite forms of direct feeling responses to stimuli which are immediately present and in close contact with the individual. These stimuli, however, may be simple or complex, with a simple or complex response on the reaction side. Here we refer to the pleasantness or calming affect produced by touching, stroking or hurting the skin, or the kind of feelings that accompany such actions as eating in which we can localize a pleasant or unpleasant feeling on the tongue.

(10) *Tender Affective Responses.* Classed here is a type of highly organized feeling directed toward a person or some other living thing. The specific qualities of these responses are correlated with these types of stimuli. That is to say, these reactions involve implicit responses to the suffering and the joy of the persons or other living things that constitute the stimuli. To a certain extent the person reads himself into the situation of the stimulus individual, and imagines his trials and triumphs, and himself suffers accordingly. For names of these reactions we might use the terms sympathy, meltingness, pity, forebearance, etc.

Specific differences between the tender feeling responses depend upon the character of the person and upon the relationship of the stimulus person to the reacting individual. Of course in every case we are ruling out any sort of intellectual compassion or pity and insisting that the reaction when it occurs must answer to the particular characteristics we have specified as belonging to the feeling reaction.

Summary

In this paper we have attempted to suggest a naturalistic description of feeling phenomena. By this we mean that we describe feeling reactions as actual ways in which the person adapts himself to his affective stimuli. Upon analysis we have found that a feeling reaction system consists of adaptations which involve primarily the visceral mechanisms, facial movements and such external skeletal mechanisms as do not produce any direct effect upon the object. As a consequence the most characteristic feature of affective adaptations is that they constitute changes or effects in the person himself, and not effects in or upon the stimuli.

Striking superficial similarities appear to align affective responses with emotional, desire and pain reactions. In consequence, feeling responses are very frequently confused with one or more of the latter. Because of this situation we have essayed a differentiation of the three latter forms of behavior from the affective reactions.

Assuming that affective phenomena are concrete psychological adaptations of a specific sort, it follows that because of their intimate personal character and frequency of occurrence there must be a great number of qualitatively different types. These qualitative differences constitute essentially different reactional characteristics which are not reducible to any sort of logical abstractions.

Concerning the stimuli for affective reactions we find that these are extremely numerous and very frequently are conditions, events, modifications of things, acts and qualities rather than composite objects.

Among some other results of our study is the observation that the typical affective reactions partake of behavior characteristics of other psychological phenomena. Accordingly, we find that affective behavior comprises reflex, habit and volitional reactions besides a great many varieties of cultural conduct.

In lieu of the impossible task of classifying feeling behavior we have selected from the more familiar types of affective reactions ten classes which we have attempted to distinguish from one another. As names of these classes we have retained whenever possible the terms usually connected with them in so far as this was practicable.

BOOK REVIEW

Behaviorism and Psychology. By A. A. ROBACK. University Bookstore, Inc.: Cambridge, Mass. 1923. Pp. 284.

In the present volume, Dr. Roback sets himself the task of evaluating Behaviorism as a point of view in psychology. He handles the problem under the following main headings: (1) history and exposition; (2) criticism of the principles of behaviorism; (3) practical applications of behavior in such fields as ethics, law, medicine, religion and society; and (4) the future of behaviorism and psychology. The book closes with a chart which presents twenty-one varieties of behaviorism and which lists some forty individuals. The varieties are grouped into four main classes: prebehaviorism, behaviorism proper, psycho-behaviorism, and nominal behaviorism. The subdivisions of behaviorism proper are given: (1) as structural (gross, Holt; physiological, Watson and Weiss; neurological, S. B. Russell, Meyer, et al.; formal, Tolman); and (2) as functional (biosociological, Parmelee; physico-social, G. DeLaguna; philosophical, Bode, Perry, et al.; and dynamic, Holt, Kempf and Kantor). The organization of the chart is such that practically every member of the American Psychological Association is eligible to a place in it! The omissions which strike me as the most conspicuous are those of Angell (as a pre-behaviorist), Judd (in connection with motor theories), Thorndike, and Woodworth.

The historical section relates behaviorism to materialism and realism in philosophy, to the views of Comte and Cournot, and to other more recent philosophical and psychological writings. McDougall is given credit for first defining psychology behavioristically, although the author points out that McDougall has never carried out the definition. The author's presentation in this section and throughout the book is too polemical and sketchy to be a good example either of historical analysis or of dispassionate criticism.

In putting the case against behaviorism, Roback emphasizes the following criticisms, which I shall list with what I conceive to be their possible answers.

Behaviorists criticize the introspective psychologists for their nebulous theories and yet they present hypotheses of the same character,—as witness Watson's treatment of affection and the image and the physiological constructions of Meyer, Russell and others. And yet, except for the philosophers, the behaviorists have avoided the issues and objections raised by the opposite side.

This criticism can hardly affect the value of the behavioristic position. Undoubtedly there have been extravagances and mistakes as there are in all revolts; but the behaviorists have done well to let most of the talking be done by their opponents. Most of this talking has been done with the purpose of showing that consciousness does exist and that the behavioristic position involves that assumption. (Lovejoy is perhaps the most glaring example.) If consciousness exists, then psychologists should study it because no other science does so. Now I gather, from what Watson (whom Roback calls an "enfant terrible" and other undignified names) has written, that even he would not deny the existence of consciousness. To do so would be to enter upon the discussion of philosophical problems, and this the behaviorist refuses to do. What Watson does do is to doubt the possibility of making a scientific study of what others call "consciousness." My own views would be in harmony with this position to this extent:

although I believe that a scientific study can be made of conscious states' as has been done by the introspective psychologists, I do not think that these studies bulk large relatively in the history of scientific psychology or that those studies which have been made are of particular value. This last point is, of course, the expression of personal taste, but I suspect that a similarity of such tastes is at the basis of much of the behaviorists' activities. When I say that introspective studies do not bulk large in scientific psychology, I mean that few studies have been devoted to the description and analysis of consciousness, in spite of the fact that practically all studies have used the terminology which descends from the Greek philosophers. Many studies can be interpreted in either of two ways. Thus studies of sensory processes may be regarded as of value because they indicate the nature of conscious qualities or because they get at certain sensory conditions of behavior. So far as psychologists take the former view, so far are they subjectivists tending to reduce the science to what Yerkes has called a field of "pure psychics." Now I submit that a world of "pure psychics" may be interesting, but that it is of value only for aesthetic contemplation. It is decidedly worth while from the human standpoint to assume that one's sweetheart is not an "unconscious" automaton; but that is partly because such interpretations are intimately bound up with behavior, and partly because of the aesthetic satisfaction involved in such theories.

I now come to a second criticism stressed by Roback. The behaviorists have no experimentation to show on lines divergent from introspection (p. 71). Undoubtedly the author has in mind some other meaning than that which would ordinarily attach to such a statement, but I can not discover it from the context. All of the studies in animal psychology, infant psychology, and mental tests, and most of the studies of learning and memory, have been accomplished on lines divergent from introspection in the sense that this method has not been used. Many of these studies have been made by men with behavioristic leanings. In many cases the authors have felt it necessary to *add subjective interpretations to their observations in order to convince themselves that they are working in psychology*, but such interpretations are irrelevant to the nature and scientific character of the experiments and as such are definitely opposed by behaviorism.

A third criticism of behaviorism which the author urges is that the term behavior is too indefinite to serve as a name for the subject-matter of psychology. Even the physical sciences speak of the 'behavior' of organic and inorganic material, while physiology and the biological sciences are largely preoccupied with the 'behavior' of animals. An examination of the studies made by behaviorists shows that even the distinction between "whole and part" behavior is not adhered to, while it also shows that these students are unable to describe responses correctly because they ignore the intentions of the subject which alone serve to distinguish many responses one from the other.

The main issue raised here seems to me a fruitless one. Psychologists need concern themselves no more about the definition of their science than do the physicists, chemists, biologists, and others with the definitions of their respective fields. Those who are devoted to clear-cut definitions deductively arrived at are horrified of course by the statement that psychology is the science studied by psychologists, that its subject-matter is very diverse and is determined largely by academic and other social conditions, but that its goal is the scientific understanding of human nature in which it emphasizes both "consciousness" and "behavior." Numerous philosophical questions can be asked about such a definition, as they can be about the terms employed by the other sciences. Indeed who can discourage philosophic meddling with any question? And yet I conceive these problems to be no more the concern of the psychologist than are similar ones the concern of the physicist. The behaviorists will in time be able to offer a clearer picture of what psychological behavior is in terms of the behavior which has been studied by men known as psychologists. Any definitions or hypotheses proposed in advance of this historically accumulated information must be justified by their significance for the control of observational procedures.

The critics of behaviorism, including Dr. Roback, make much of the philosophical implications of that point of view and chide the behaviorist for not looking better to his philosophical groundwork. The behaviorist, I believe, intends to let the philosopher make any examinations and speculations to which he may be inclined, while the behaviorist applies himself to his experiments like any other good scientist. (And yet Dr. Roback says (p. 32) that behaviorism is merely a philosophical attitude applied to the subject-matter of psychology!) Let us not scold the behaviorist for his indifference to philosophical problems. Let us rather give thanks that some psychologists are so innocent in philosophical matters that they may attack their problems with the same single-mindedness that characterizes the approaches made by other scientists upon their problems. I do not mean to say that the behaviorist is the only psychologist who tries to keep away from philosophical questions; but I do insist that he succeeds better than the structuralist, if for no other reason than because of the novelty of his terminology. I think, when Watson claims that he doesn't know what the terms 'imagery', 'consciousness' and 'affection' mean, that he is influenced by the long philosophical controversies summed up in each term. It seems impossible to define the classical terms in psychology in such a way as to insure acceptance, and this situation seems certain to persist just so long as psychologists are steeped in the philosophical history of these terms. It would be psychologically unexpected if men of markedly different philosophical tastes and training should attach the same meanings to the terms which they use. Terms like sensation, affection, image, emotion, etc., will always be productive of verbal controversies for this reason, and progress demands their elimination. If we are to study consciousness, let us get some terms which are new and which do not invite differences of opinion by virtue of the various meanings attached to them since the days of Aristotle. The behaviorist himself has still some housecleaning to do. Even Watson uses such terms as instinct and emotion! One needs only to read through the recent discussions to realize that it has become impossible to secure agreement on the meaning even of the term *instinct*. Let us give up this term also, then, rather than spend precious energy in verbal battles. Agreement can be reached concerning the observable phenomenon, and this can be given some colorless name such as *alpha* behavior. Behavior which has a different genetic history in the individual can then be termed *beta* behavior, and so habit and instinct as terms may be eliminated. If such action seems necessary in *behaviorism*, how much more imperative is it for the structuralists to free themselves from the Idols of the Market-Place, which have obscured genuine scientific issues since philosophy began! A good example has been set for all of us by the mental testers. Their success is in proportion to their avoidance of subjective terms which would draw them away from the real business of the day. One term, however, has threatened to hold up experimental work because of its subjective significance and philosophical history, the term 'general intelligence.' These men, however, have been wise enough to make but little use of the term and to emphasize the concrete attack upon their problems, although every now and then a pathetic wail goes up, from some one who is hurried along by the procession, to the effect that nobody knows *what* is being tested. And of course no one does know, in subjective terms, and few care! I venture the assertion that most of the opposition to the use of tests which is current today is due to the ambiguities of the term *general intelligence*.

Dr. Roback has two other criticisms, to whose consideration we must turn. The first of these is that the behaviorists refuse to use the introspective method and hence do not have access to all of the data. This I believe is an important systematic question, quite irrespective of whether introspection be thought to give information concerning consciousness or not. Roback overlooks the fact that Watson would not exclude verbal responses and the testimony of the subject from the methods of psychological investigation. Watson, writing in the symposium on thinking, explicitly provides for what most psychologists have termed introspection, but he has never developed the implications of such an admission. Does he not thus let in at the back door all of the subjective problems which he has so unceremoniously bundled out at the front? The position can be so interpreted, but no one who knows Watson will believe that to be his intention! There is here an important phase of his position which he should develop, not only in the interests of behaviorism but also, what is much more important, in the interests of unity within the psychological field. My impression is that the subjectivist and objectivist can get together only when the former will accept a terminology which will discourage the philosopher and yet which will refer to those "experiences" which all can observe. My suggestion intends to have little in common with the earlier attempts by Bethe and others to establish an objective nomenclature, inasmuch as the present suggestion seeks new terms for subjective material and not for objective material only.

We now come to the final argument against behaviorism which we are to consider. It may be formulated as follows. Inasmuch as ethics, jurisprudence, medicine, religion and the social sciences are written in terms of subjectivism, behaviorism is incompatible with them. If psychology is to be conceived in terms of behavior, it must lose all contact with these fields. Dr. Roback must attach primary importance to this criticism, for he devotes one-fourth of his book to its exposition. Again I find myself unable to agree with the author. In the first place I wish to dissent most emphatically from the assumption that any science should so regulate its conduct as to satisfy some other field. The business of the psychologists is to define their terms and to conduct their investigations in the manner best suited to solve their problems. This is the fundamental requirement. If psychology gets the facts, other disciplines which rely upon it will be only too ready to incorporate the material. Physics does not conduct its work so that the results will be suited to engineering. Rather engineering takes whatever the physicist can offer which is relevant to its own problems. So biologists seek for the facts in their field in the best way possible, and medicine must use the material or go without. One need not read psychologists a lecture upon this moral! If they find themselves talking in the same vein as that adopted in various related disciplines, the situation is only the result of the incorporation into those disciplines of the only psychology offered them. Psychologists will set their own pace and study their own problems in the ways most likely to bring reliable results. And we need not fear that the social sciences and other disciplines will reject the data because of their newness or because of technical terminology. What is wanted in these fields are the facts about human nature, and it is the psychologist's problem to furnish this material in an unbiased and scientific manner. I am therefore much more optimistic concerning the future of behaviorism than is Dr. Roback, because I feel that this point of view keeps closer to the verifiable phenomena of human nature than does "mentalism."

The University of Kansas WALTER S. HUNTER

NOTES

Relearning after Forty Six Years

On my tenth birthday, in January 1877, I recited without mistake Milton's *Hymn on the Morning of Christ's Nativity*, a poem of twenty-seven eight-lined stanzas. I recall nothing of my method of learning, except that it was piecemeal; I had learned a certain number of stanzas (three?) as a week-end task for what seem in retrospect to have been many school weeks.

As it happens, I can say with assurance that I had never reread the poem until March of the present year, when I undertook the experiment which is the subject of this Note. It occurred to me that it might be worth while to see how easily, after an interval of forty-six years, I could relearn the hymn. I must premise that I am accustomed to learning by heart. Also, when I looked about for a book to learn from, I was lucky enough to find on my shelves my old school edition of Milton, so that I could count on the revival of memories of place.

The record of relearning is as follows. The abbreviation a. m. means the first thing in the morning, just after breakfast, and p. m. means the last thing at night, just before going to bed. All readings recorded are complete readings.

Saturday, March 17, p. m.	(1) One silent reading. Notes made afterward.
	(2) One loud reading.

The first reading could hardly have had its full value, since I was interested to see how much of the poem was familiar, and made mental notes as I went along. I recognized verbally three lines of Stanza XX.; two of XIV. and XIX.; and one of Stanzas II., V., VIII., XXIII. These eleven lines had, for one reason or another, become a permanent part of my mental furniture. I found a vague familiarity in Stanzas I. to V., VIII., XI., XXVII. Certain stanzas—VII., X., XVI., XXVI.—struck me as definitely strange. The rest were blank: many phrases and word-groups fitted together more smoothly and easily than would be natural at the first reading of a poem; but there was no positive recognition, however vague. My general impression was that of discursiveness; the stanzas seemed to be linked rather by their author's current associations than by logical plan; and I was a little afraid that I had undertaken a burdensome task.

March 18, a. m.	(3) One silent reading.
March 18, p. m.	(4) One silent reading.
	(5) One loud reading.
March 19, a. m.	(6) One silent reading.
March 19, p. m.	(7) One silent reading.
	(8) One loud reading.
March 20, a. m.	(9) One silent reading.
March 20, p. m.	(10) One silent reading.
	(11) One loud reading (recitation with promptings).

Hitherto I had read slowly and carefully (the silent reading took about 9 min., the loud reading a little longer), with keen attention to words; I had never read any line or phrase twice over, but the reading was slow enough to permit me to note, *e.g.*, the shift of tense in VI., the shift of gender in XIV., the repetition of *in vain* in XXII., XXIII., XXIV. After the tenth reading I felt that I had some grasp of the whole; and at the eleventh I tried to recite,—covering the printed page with a card, and exposing the lines one by one at the least check of memory. I have no record, but I read a very great deal more than I recited.

March 21, a. m. (12) One silent reading.
March 21, p. m. (13) One loud reading (recitation with promptings).
(14) One loud reading (recitation with promptings).
(15) One recitation.

The thirteenth reading showed marked improvement; and the fourteenth went so readily that I was tempted to try a recitation without book. So far as I know, the recitation was correct.

March 22, a. m. (16) One silent reading.
March 22, afternoon (17) Recitation to a colleague.

This recitation—with one exception, which can hardly be accounted, in the ordinary sense, an error—was correct. In IX. I gave *warbling* for *warbl'd*. After the recitation was done, the friend who had been good enough to 'hear' me remarked on the mistake, and I bade him look again at his text; I was very sure that he had himself misread. Only when I took the book into my own hands was I convinced that, all along, I had read the wrong word for the right. Whether I had seen the apostrophe as the dot of the *i*; whether I was 'attracted' by the *answering* of the next line; whether my nervous system inhibited the double rhymes *warbl'd voice* and *stringed noise*; I do not know. I am pretty sure, however, that such a misreading could occur only in the case of material already once learned. And indeed, now that I have recovered the poem, the whole seems so absolutely familiar that, were it not for the notes jotted down after the first reading of March 17, I could not believe what I have said about its original blankness.

E. B. T.

A DEMONSTRATIONAL QUALITATIVE SERIES

In order to show, in a striking manner, how from a mixed medley of colors may be derived a qualitatively regular series we have prepared the following demonstrational apparatus. A frame about 1 x 5 ft. with a solid back carries three sets of colors. One set is fastened to the back of the frame. The other two are in slides with windows. The two slides move independently of each other and of the back. The fixed series is composed of rectangles of color in the order (1) YG; (2) R; (3) V; (4) OR; (5) G; (6) RO; (7) dark BG; (8) rO; (9) B; (10) yO; (11) RV; (12) YO; (13) verdigris; (14) OY; (15) P; (16) Y. It will be noticed that the odd numbers in the series vary irregularly in quality, but that the even numbers form a series from R through degrees of O to pure Y. The two slides carry colors and blank windows alternately. In the front slide the colors form a R-O-Y series. In the back slide the colors are duplicates of those in the front slide, but are arranged out of order.

In the first position for demonstration the *O* sees the colors of the front slide and, through the windows of both slides, the odd-numbered members of the fixed set. This set of 17 colors has 9 members in a regular order; but between them the other 8 members are chaotic. In the second position, the back slide is drawn along until its windows are covered by the colors in the front slide, and its colors cover the odd, irregular colors of the fixed series. The *O* now sees 17 colors (9 from the front, 8 from the back slide), all of which are members of a R-O-Y series; but they are out of order. In the third position, the front slide is moved until its colors cover the colors in the back slide, and the windows of both slides expose the regular series of the even numbered members of the fixed set. The *O* now sees *an orderly arrangement of 17 colors*, starting with R and passing by regular steps of increasing yellowness to pure Y. There are stops to prevent drawing the slides too far. This feature of the demonstration assures coincidence of the windows and colors in the several positions.

The colors of the odd numbers in the fixed set are of colored paper. All the members of the R-O-Y series are papers covered with ordinary wall-kalsomine, a form of pigment which is easily mixed, easily put on, gives a dead surface, and is sufficiently permanent. By a few empirical trials we determined what geometrical series of increments of R should be added to a constant amount of Y to give the proper qualitative steps.

H. G. BISHOP

Cornell University

A METHOD OF OBTAINING RECORDS OF CONSTANT BLOOD PRESSURE

In a current experiment on emotional reactions, it was desirable to have some means of taking systolic blood-pressure changes both quickly and easily. Also a method where the pressure-change could be closely followed over a space of some 5 min. was sought. Reference was found to Kolls'[1] method of taking constant records; but this involves a rather complicated apparatus which would have to be specially built.

The laboratory equipment included a "Tycos" sphygmomanometer of the office type; a compressed air tank and a hand pump[2]; and a "Bowles Patent" blood-pressure bracelet stethoscope. The air-pressure tank was substituted for the small hand-bulb as a method of inflating the armband, while the systolic pressure was taken stethoscopically. This method was found to give a quick and easy reading of either systolic or diastolic pressure. The main advantage, however, is that a series of readings which are close enough together to form practically a continuous record of changes in systolic pressure during a period of 5 to 10 min. may be easily taken.

Say, for example, that the blood pressure is 120 mm., and that owing to emotional excitement it starts mounting; by a slight manipulation of the air-tank outlet-valve the pressure can be raised in the arm-band to follow up the rise in blood pressure. With a decreasing blood pressure the closing of the tank outlet-valve and the opening of the escape-valve will allow a fall in the arm-band pressure. Both the rise and the fall may be followed very closely. This method has been found to give records of form of the systolic blood pressures in a very satisfactory manner.

University of Minnesota CARNEY LANDIS

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[1] A. C. Kolls. Continuous blood pressure tracings in man, *Jour. of Phar. and Exp. Theraputics*, 15, 1920, 433-41.

[2] Supplied by the Standard Scientific Co., E200, Catalogue 3, 1920.

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A MODEL FOR THE DEMONSTRATION OF FACIAL EXPRESSION

By E. G. BORING and E. B. TITCHENER

Ever since Wundt brought apparatus from the laboratory to his desk in the lecture-room it has been clear that a general course in psychology should, so far as possilbe, be made demonstrational. The best demonstrations, as Wundt saw, are those that invite the members of the audience to observe for themselves; the worst are those that merely display and describe some instrument of research. Even these worst, however, are very much better than nothing; they enable the lecturer to give a concrete account of procedure; and now and again they persuade some mechanically-minded student to enter the laboratory.

For the introductory courses given at Cornell University we have worked out an extensive set of demonstrational pieces, some of the older of which have been described in this JOURNAL.[1] The various topics handled in these courses present varying degrees of difficulty to the demonstrator; and we have found Feeling to be the most difficult of all. In the lectures on Simple Feeling we have been obliged, so far, to content ourselves with two demonstrations: a demonstration of the method of impression, by way of paired comparisons of colors, and a demonstration of the method of expression, by way of the pneumograph.[2] Emotion has long baffled us. We tried, first, to construct a series of charts by means of drawings and photographs of postural and facial expression; but the series remained woefully incomplete, and the demonstration also consumed too much time. We next thought of an artist's manikin; but we found none on the market whose articulation met our psychological requirements; and in any case the impossibility of facial change makes the lay-figure an unsatisfactory performer.[3] Finally, we decided on an attempt to realise, in demonstrational form, Piderit's 'geometry of expression,'—not by way of the plaster heads which he himself suggests, but more simply by way of an 'articulated' profile.[4]

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[1] E. B. Titchener, Class Experiments and Demonstration Apparatus, this JOURNAL, xiv., 1903, 439ff.

[2] Neither of these arrangements has been described.

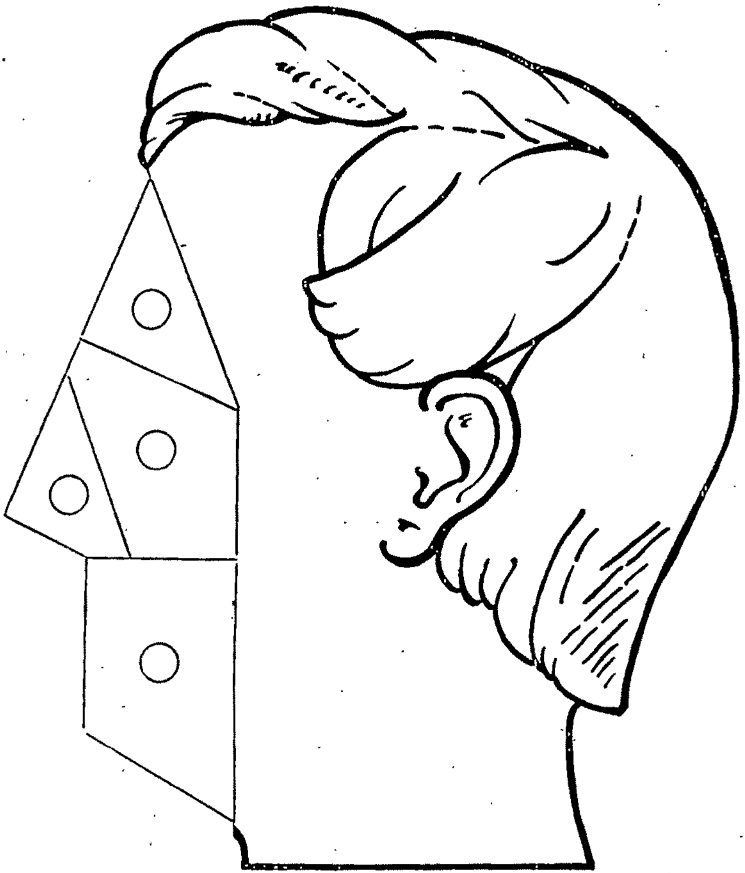

FIG. I

The model consists of the profile of a head, about 18 in. high, with various brows, eyes, noses and mouths, that can be fitted into place after the manner of a puzzle picture. The basic outline is shown in Fig. I., and the pieces in Figg. II. and III. The 18 in. head is satisfactory in a room seating about 250; for larger audiences the model should be larger.

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[3] Cf. E. Toulouse et H. Piéron, *Technique de psychologie expérimentale*, ii, 1911, 85 ff. (measure of memory of attitudes and expressions). We have not altogether given up the idea of a demonstrational manikin or human phantom.

[4] Th. Piderit, *Mimik und Physiognomik*, 1867, 1886, 1909. The third edition was reprinted in 1919. The full title of the first edition was *Wissen-*

The original model at Cornell is made of white cardboard with the outlines drawn in India ink. On the back of every piece is glued a rectangular piece of wood which fits into a rectangular socket in the square wooden base upon which the head is mounted. The base-board is supported on a tripod, inclining slightly backward from the vertical. This arrangement works perfectly: the parts stay in place and, being independent, remain in position even when pieces below them are removed. The contrast between the white cardboard and the black lines greatly assists the visibility of the picture. The parts need, however, to be kept carefully, in order that the cardboard shall not curl up or get soiled or become battered at the corners.

A wooden model was later constructed for the Clark Laboratory with the intention of obtaining a more durable demonstration that could easily be transported for lecture purposes. This model was made of several plies of white wood. The head was raised from the base-board the thickness of one ply (⅛ in.). A large socket ¼ in. deep of the shape shown in Fig. I. was made by cutting out the central ply (¼in.), and into this socket four blocks, ¼ in. thick, for the four features, exactly fitted, as shown by the outlines in Fig. I. The profile is carried on every piece by an additional ply (⅛ in.), which is glued to the block and which projects, in different amounts for the different expressions, beyond the outer line of the block. The pieces are removed by pushing with a finger through a hole at the back of the model; these holes are shown as circles in Fig. I. The Clark model has been less satisfactory than the Cornell model because sufficient attention was not given to balancing the plies one against another, and the model has consequently warped. This model also shows less contrast than does the white cardboard. In it the white wood was filled with white shellac and the outlines were drawn in India ink directly upon the wood. The resultant pieces are not all of the same color, because of differences in the wood, and the yellowish surface gives less visibility than the white. A better wooden model could be constructed, if more attention were given to the principles of multi-ply construction, and if the surface of the wood were painted white.

The outlines of the model are all copied directly from Piderit-except for a few minor modifications and slight changes neces, sary to secure perfect registration in any combination of pieces.[5]

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schaftliches System der Mimik und Physiognomik. It was preceded in 1858 by a smaller work, *Grundzüge der Mimik und Physiognomik,* which we have not seen. There is also a French translation of the second edition (1888): *La mimique et la physiognomie.* The dates for Piderit, 1858 and 1867, are interesting when taken in connection with the dates for Darwin, 1859 and 1873. In the first edition of Piderit there is no mention of Darwin; the second edition discusses specifically *The Expression of Emotions in Man and Animals* (1873) and refers repeatedly to Darwin.

[5] We wish to acknowledge the assistance of Mr. W. S. Hulin of the Harvard Psychological Laboratory in the preparation of the drawings for Figg. I.-III. of this article.

The principle, upon which the expression of the emotions is demonstrated by the model, consists in showing that given meanings, often biologically explicable, can be attached to given simple expressions, and that the complication of expressions by mere addition leads to a total expression which is intelligible as a complex of the correlated meanings. The approach in lecture is best made by way of Darwin's principles of remnants of originally serviceable habits and of antithesis.[6] It is necessary to add to these principles the corollary that contradictory expressions do not always lead to paradoxical expressions, since a total expression may properly and simultaneously mean contraries. Thus contempt involves at once both attention and inattention (*cf.* Fig. VII., 23). One may even introduce here the notion of the Freudian conflict.

When the expressions to be compounded pertain differentially to the brow, eye, nose and mouth, the addition is obtained simply by combination of the different pieces of the model. When two expressions pertain to the same feature it is necessary to have a new piece showing the combination of the two expressions (*e.g.*, the combination of the open mouth, Fig. II., 7, with the bitter mouth, Fig. II., 4, as in Fig. II., 8).

A very large number of features could be made. The present list does not exhaust Piderit, and Piderit does not exhaust the possibilities. Some indication of promising additions will be given below.

On the other hand, the present set permits a very great number of possibilities. There are here 360 possible combinations, and most of these turn out to be sensible, largely because the combination of contraries often gives a natural expression for a conflicting emotion. Some expressions are, however, unnatural or impossible (*cf.* Fig. V., 12, and possibly Fig. VI., 15).

One begins in lecturing by listing (preferably on a blackboard) the simple expressions with their corresponding meanings. For the pieces of the present model these correlations are as follows.

Mouths

The mouths are shown in Fig. II., and will be referred to as M1, M2, etc. The mouth, according to Wundt,[7] is fundamental to the expression of the emotions, because taste qualities, like sweet and bitter, are naturally correlated with pleasantness and unpleasantness and hence with acceptance and rejection, and are further correlated physiologically with regions of the tongue, which is most sensitive to sweet at the tip and most sensitive to bitter at the root.

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[6] C. Darwin, *Expression of the Emotions in Man and Animals*, 1873, 27-65.

[7] W. Wundt, *Physiologische Psychologie*, iii., 1911, 264 ff.

M1. Normal mouth = passivity. Piderit does not give this inexpressive mouth. It is necessary, however, as the neutral point of departure for other expressions (*cf.* Fig. IV., 1).

M2. Sweet mouth = pleasantness. The lips are pressed together and drawn in and the tongue is pressed forward, so as to keep the sweet substance in the mouth and to get the greatest effect from it. M2 is the basis of the pleasant expressions.

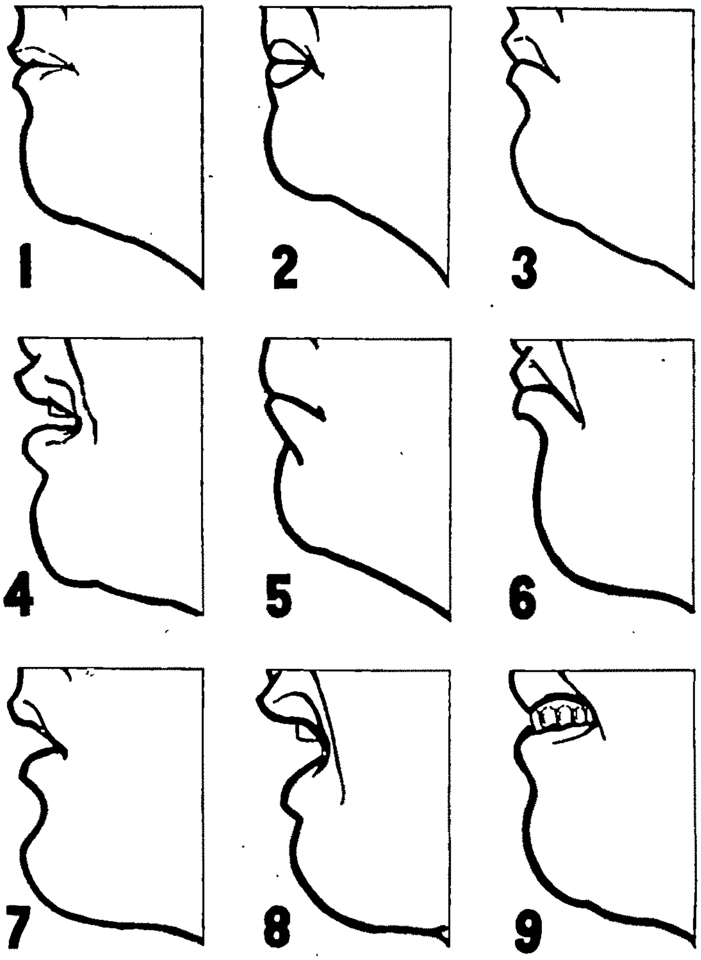

Fig. II

M3. Bitter mouth = unpleasantness. The root of the tongue, most sensitive to bitter, is drawn down to avoid stimulation by the bitter substance, thus changing the contour of the neck. The lips are thrust out in incipient spewing (the pout). M3 is the basis of the unpleasant expressions.

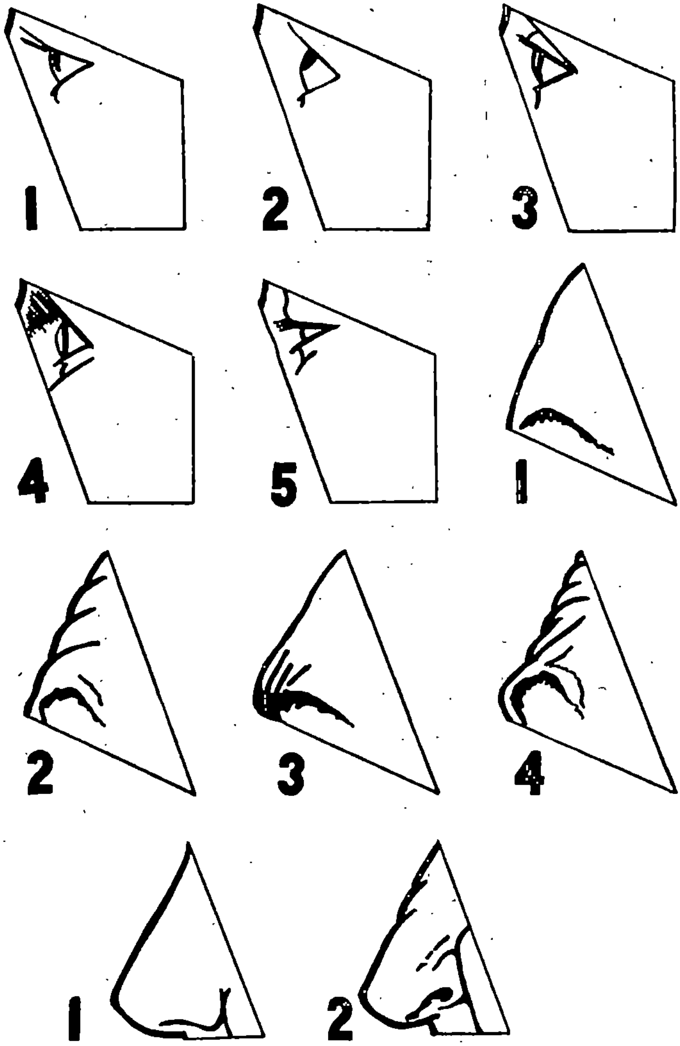

Fig. III

M4. Extremely bitter mouth = great unpleasantness. This expression is simply an exaggeration of M3. The tongue is drawn further down, the lips are protruded further, and the mouth is a little open: a more extreme attitude of rejection.

M5. Set mouth = stubbornness. *Der verbissene Zug* (Piderit) is only imperfectly translated by the word "set." The biological explanation is not obvious, unless one turns to Darwin's third principle, the spread of nervous energy.[8] The lips are protruded, but, otherwise than in the bitter expression, are pressed tightly together; thus the corners are drawn down. The expression fundamentally means determination, but the compressed lips give it also the meaning of rejection. Thus it is correlated with obstinacy or stubbornness.

M6. Bitter and set mouth = unpleasant stubbornness. This expression is not new, but is a combination of M4 and M5, which can not be made by combining two pieces since the two expressions pertain to the same feature.

M7. Open mouth = attention. The biological explanation is presumably better breathing rather than alimentary readiness. The present outline has been changed from Piderit by enlarging the chin to what M1 would be when opened. The open mouth in Piderit is especially vacuous because of the receding chin; but in general attention belongs to the eyes and brow and is not shown well by the mouth alone (*cf.* Fig. IV., 6).

M8. Bitter and open mouth = unpleasant attention. This expression is a combination of M4 and M7. It is part of horror (*cf.* Fig. VII., 19).

M9. Clenched teeth = rage. The mouth is meant to show the bared canines of the snarl or sneer.[9] It occurs in anger (*cf.* Fig. VII., 20), and is too extreme an expression to appear alone.

The sour mouth of Wundt might be added.[10] Piderit does not give it, and Wundt, copying Piderit's sweet and bitter mouths in profile, found it necessary to draw the sour mouth in full-face.

There are also the possibilities of adding smiles of various degrees, and the laughing and weeping mouths. Piderit draws these in full-face, and it may be that they could not be shown adequately in profile. It is not impossible that the present model could advantageously be supplemented by a second in full-face. If laughter and weeping are to be represented, it would be necessary to add corresponding eyes, brows and noses.

Eyes

The eyes are shown in Fig. III., and will be referred to as E1, E2, etc.

E1. Normal eye = passivity. This eye is the neutral point of departure for other expressions.

E2. Raised eye = exaltation. The expression is symbolic and as such is distinctively human. It tends to imply 'introversion,' since reality is mostly upon the ground.

E3. Wide open eye = attention. Human attention is primarily visual. Ordinarily the brow is raised also (B2). This expression is fundamental to the meaning of 'extraversion.'

E4. Very wide open eye = great attention. This eye is simply more extreme than E3, and is always combined with the raised brow, B2 or B4. Properly it belongs only to the brow that is both raised and contracted (B4), since it is the contraction of the brow that produces the shading above the eye, copied from Piderit's original drawing of horror (*cf.* Fig. VII., 19).

E5. Lowered lid = inattention. This expression is basal to 'introversion' or to voluntary inattention. It can be referred to Darwin's prin-

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[8] Darwin, *op. cit.*, 66-82.

[9] *Ibid.*, 249ff.

[10] Wundt, *loc. cit.*

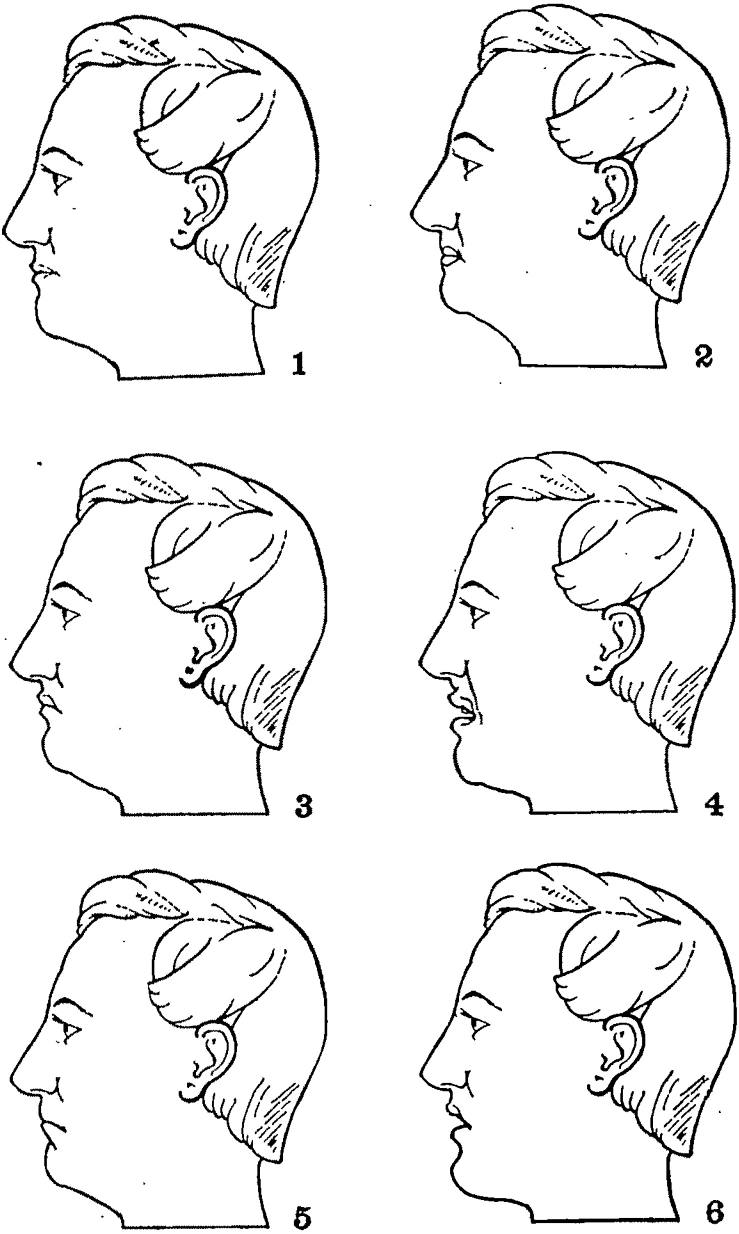

Fig. IV

No.	M	N	E	B
1.	1	1	1	1
3.	3	1	1	1
5.	5	1	1	1

No.	M	N	E	B
2.	2	1	1	1
4.	4	1	1	1
6.	7	1	1	1

ciple of antithesis (E5 is the opposite of E3), or to some more fundamental biological principle, allied to the conditioned reflex, which explains why animals, including, it is said, the ostrich, refuse to look facts in the face. Most facts that have to be met are predominantly visual, and are thus partly removable by removing the visual perception.

The wide open eye (E3), especially when combined with the raised brow (B2), may mean memory as well as attention. It is not at once obvious why attention and memory should be similar. As an invariable correlate of memory Piderit lists the moving eye (not easily incorporated in a model), thus anticipating the experimental finding of Perky forty-three years later.[11] The moving eye, the wide open eye, and the raised brow are all the imitative kinaesthesis of remembering.

Brows

The brows or foreheads of Fig. III. are referred to as B1, B2, etc.

The brow goes properly with the eye and should ordinarily be treated with it. Normally the wide open eye and the raised brow are part of the same expression. They may, however, contradict each other in a complex expression (*cf.* Fig. VII., 22-24).

B1. Normal brow = passivity: the neutral point of departure for the other expressions.

B2. Raised brow (horizontal furrows) = attention. The expression belongs naturally to the wide open eye (E3, *q.v.*), but may give the meaning of attention apart from the eye.

B3. Contracted brow (vertical furrows) = unpleasantness and thought. The assignment by Piderit of two such apparently different meanings to the same expression seems odd at first, although one notes that active thought of the contracted brow is usually supposed to be unpleasant. The common term, on the side of meaning, seems to be 'introversion.' The unpleasant is avoided; thought is subjective. In both the reality of visual perception is avoided (*cf.* E5).

B4. Raised and contracted brow (horizontal and vertical furrows) = unpleasant attention. There is nothing incompatible in the meanings of B2 and B3 to prevent their combination, except that, since the raised brow means attention ('extraversion'), the contracted brow with it can not mean thought ('introversion') and must mean unpleasantness. There is here, nevertheless, a genuine conflict positively expressed. The organism meets an unpleasant situation either by attending to it (raised brow) or by withdrawing from it (contracted brow), and the two contrary tendencies are simultaneously registered, as, for example, in horror (*cf.* Fig. VII., 19), where the organism tends to withdraw and yet is compelled to attend.

Noses

There are two noses, N1 and N2, in Fig. III.

N1. Normal nose = passivity. The nose is the least mobile part of the face and ordinarily can not be innervated alone. N1 is therefore the nose of all mild expressions. Only in extreme emotions need N2 be substituted for it.

N2. Dilated nostrils = attention and unpleasantness. It seems probable that the biological ground for this expression lies in olfactory attention and sniffing, although the dilatation of the nostrils in anger must be related to the deeper breathing which is part of the total picture of preparation for attack. The assertion in olfactory attention of the unpleasantness of the object presents the same puzzle which confronts us when we try to explain the taboo upon smells in conversation and literature.

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[11] C. W. Perky, this JOURNAL, 21, 1910, 422-452.

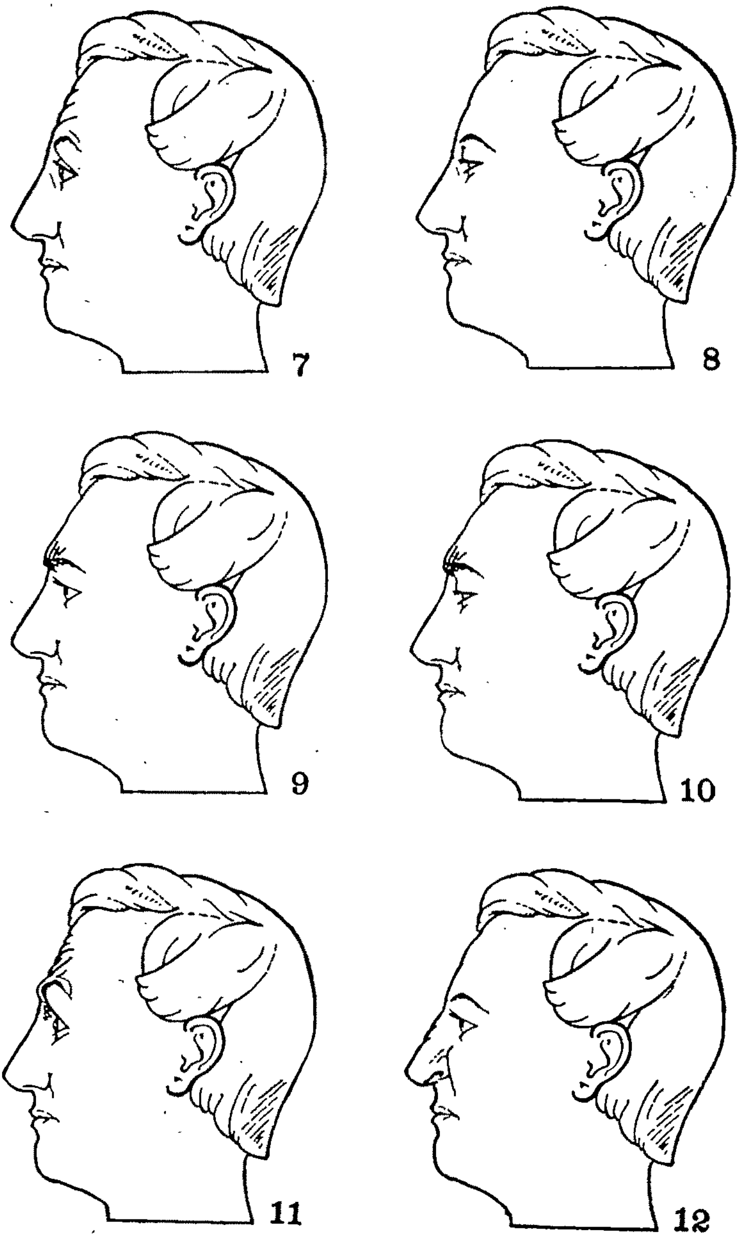

Fig. V

No.	M	N	E	B
7.	1	1	3	2
9.	1	1	1	3
11.	1	1	4	4

No.	M	N	E	B
8.	1	1	5	1
10.	1	1	5	3
12.	1	2	1	1

Henning indicates that most smells are potentially pleasant; and yet when the nose of Fig. VII., 24, asserts, in contempt or disgust, "How you smell," the expression inevitably takes on an uncomplimentary meaning.[13] (*Cf.* the sniff of contempt, thumbing the nose, etc.)

It has been suggested that an intermediate nose between N1 and N2 would be desirable in the model. If one were, however, to multiply the number of pieces in the model by the discrete degrees of expression, the demonstration would become impossibly complicated.

If laughter and weeping were added, two more noses would be needed. According to Piderit the nose is almost the only *differentia* between these two expressions. It is slightly dilated in weeping and spread broadly in laughter.

The foregoing expressions may be recapitulated under their meanings as follows.

Meanings

Pleasantness is shown by the sweet mouth (M2).

Unpleasantness is shown by the bitter and extremely bitter mouths (M3, M4), which are attitudes of rejection; by the contracted brow (B3), which is an attitude of withdrawal; and by the attentive nose (N2),—which may mean that unpleasant odors are the odors most often attended to by most persons. Since thought is also a withdrawal, it is likewise indicated by the contracted brow.

Attention is the most important attitude of the organism, and is shown by the open mouth (M7), the wide open eye (E3, E4), the raised brow that belongs to the wide open eye (B2), and the dilated nostrils (N2).

Inattention is indicated by the lowered eye-lid (E5), which may be enhanced by the contracted brow (B3).

Stubbornness is the set mouth (M5). *Rage* is the clenched teeth (M9). *Exaltation* is the raised eye (E2).

Total Expressions

Of the 360 possible combinations of the pieces, 24 are shown in Figg. IV-VII. These cases are selected as showing well the range of the model and the principles of demonstration. The cuts for the figures were photographed from diagrams made by pasting together parts from prints of Figg. I-III. The profiles of Figg. IV-VII. are therefore not redrawn, but are actually obtained by mechanical addition. In the model itself the cracks between the pieces (*cf.* Fig. I.) show, but they are not nearly so prominent as the lines of the profiles.

The following numbers refer to the total expressions of Figg. IV-VII. The words describing the expressions are intended merely to be suggestive; such words do not admit of rigorous definition. The expression is really defined by its analysis.

1. *Passive.* The relaxed inexpressive face. M1+N1+E1+B1.

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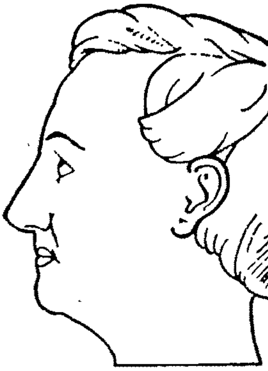
[13] H. Henning, *Der Geruch*, 1916, 177-183. As Henning divides thee smell prism there would be only one-half to one-third as many unpleasant odors as pleasant. Actually the proportion of examples that he gives is even more in favor of the pleasant odors. On the fallacy of the olfactory taboo, see E. B. Titchener, *Text book of Psychology*, 1910, 116; *Beginner's Psychology*, 1915, 50 f.



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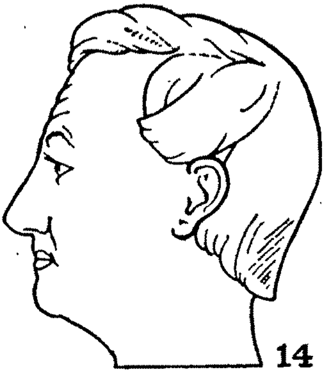

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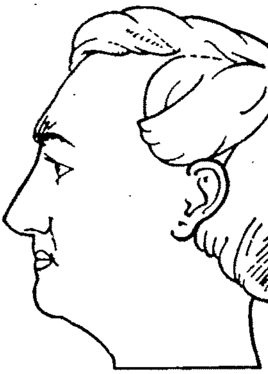

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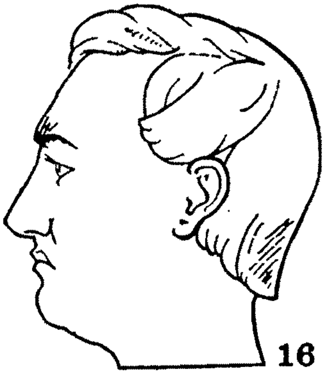

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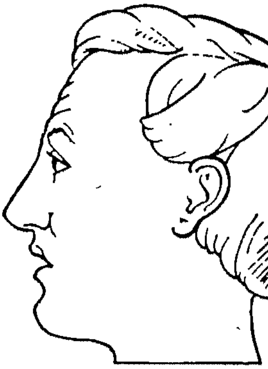

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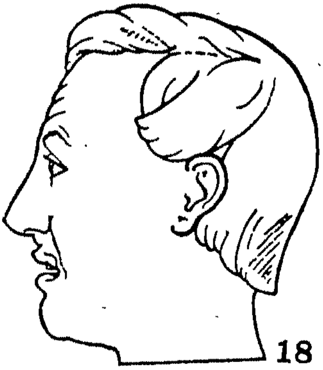

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Fig. VI

No.	M	N	E	B
13.	2	1	2	1
15.	2	1	1	3
17.	7	1	3	2

No.	M	N	E	B
14.	2	1	1	2
16.	3	1	1	3
18.	4	1	3	2

2. *Pleased.* Pleasantness (sweet mouth). M2+N1+E1+B1. A very mild expression; the sweet mouth is not a smile.

3. *Displeased.* Unpleasantness (bitter mouth). M3+N1+E1+B1.

4. *Very displeased.* Great unpleasantness (extremely bitter mouth). M4+N1+E1+B1. The expression is not striking, because the contracted brow should be added, as in no. 16.

5. *Stubborn.* Refusal and determination (set mouth). M5+N1+E1+B1.

6. *Attentive.* Acceptance (open mouth). M7+N1+E1+B1. Oral attention is, however, much less expressive than visual attention; *cf.* nos. 7 and 17.

7. *Quizzical.* Attention (wide open eye and raised brow). M1+N1+E3+B2.

8. *Inattentive.* Withdrawal or 'introversion' (lowered eye-lid). M1+N1+E5+B1.

9. *Displeased, thoughtful.* Withdrawal or 'introversion' (contracted brow). M1+N1+E1+B3.

10. *Deeply thoughtful.* Still greater withdrawal; combination of nos. 8 and 9 (lowered lid and contracted brow). M1+N1+E5+B3.

11. *Dismayed.* Displeased (contracted brow) attention (wide open eye and raised brow), showing a conflict between the tendencies for withdrawal (contracted brow) and acceptance (raised brow and wide open eye). M1+N1+E4+B4.

[12. *Unpleasant olfactory attention.* M1+N2+E1+B1. This expression is practically impossible and is presented here to emphasize the point that the nose is a feature that, unlike the mouth, eye and brow, contributes to expression only in combination with changes in other features.]

13. *Reverential.* Pleasant (sweet mouth) exaltation (raised eye). M2+N1+E2+B1.

14. *Affable.* Pleasant (sweet mouth) attention (raised brow). M2+N1+E1+B2.

If the factor of attention in no. 14 is enhanced by the wide open eye, we get Piderit's drawing for *sweet memories*: M2+N1+E3+B2. A drawing is inadequate, however, since the eye should rove.

15. *Pleasant unpleasantness,* a contradiction between the sweeth mouth and the contracted brow. M2+N1+E1+B3. *O*s differ as to the meaningfulness of this expression. It is anatomically possible, and one *O* characterized it as the passage from pleasant to unpleasant thought. The case illustrates the difficulty of finding any combinations that are meaningless simply because they are contradictory.

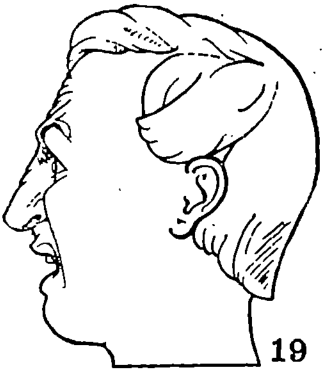
16. *Displeased and thoughtful.* Unpleasantness (bitter mouth, contracted brow) and withdrawal (contracted brow). M3+N1+E1+B3.

17. *Bewildered.* Very great attention (open mouth, wide open eye, and raised brow). M7+N1+E3+B2.

18. *Amazed.* Unpleasantness (bitter mouth) and great attention (wide open eye and raised brow). M4+N1+E3+B2.

19. *Horrified.* Great unpleasantness (bitter mouth, dilated nostrils, contracted brow) and great attention (open mouth, dilated nostrils, wide open eye, and raised brow). Really a meaningful contradiction, since it is an attentive withdrawal (brow raised and contracted). M8+N2+E4+B4.

Anger is based upon the clenched teeth, M9. When the clenched teeth, are combined with the wide open eye and the raised brow, one gets a pleasant anger: M9+N1+E3+B2. The contracted brow, however, gives an unpleasant anger: M9+N1+E1+B3. Great anger, as the model shows it, is unpleasant, though the brow is both raised and contracted: M9+N1+E4+B4. In very great anger the nose is also dilated, and this expression is shown as no. 20.



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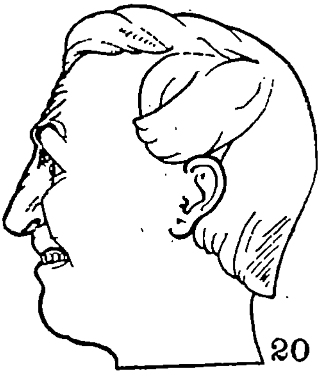

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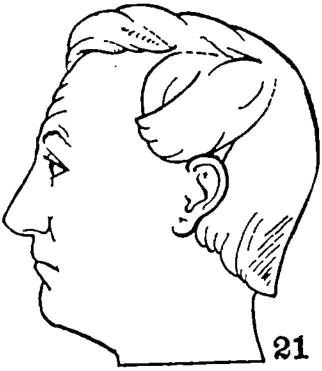

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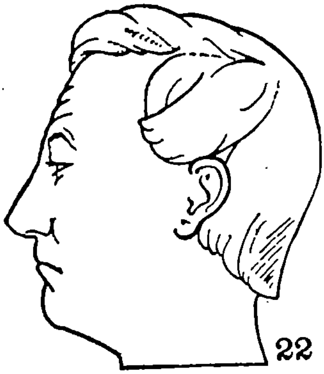

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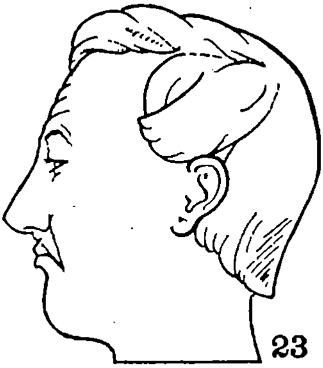

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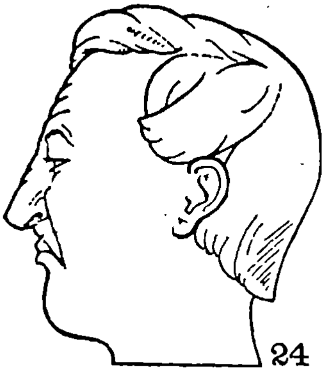

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Fig. VII

No.	M	N	E	B
19.	8	2	4	4
21.	5	1	3	2
23.	6	1	5	2

No.	M	N	E	B
20.	9	2	4	4
22.	5	1	5	2
24.	6	2	5	2

20. *Raging.* Unpleasantness (dilated nostrils, contracted brow), great attention (dilated nostrils, wide open eye, raised brow) and aggression (clenched teeth). M9+N2+E4+B4.

21. *Disapproving.* Resistant (set mouth) attention (wide open eye, raised brow). M5+N1+E3+B2.

No. 21 has been described as "Mr. Grundy." A related expression is that of the "extreme Puritan," where the set and bitter mouth is combined with the raised eye: M6+N1+E2+B1. The mouth gives obstinate disapproval, and the eye a withdrawal from the mundane.

22. *Disdainful.* Refusal (set mouth), inattention (lowered lid) and attention (raised brow). M5+N1+E5+B2. The contradiction between the raised brow and the lowered lid is meaningful. Disdain is both attentive and inattentive; it points to an object and denies interest in it.

Disdain is the first member of a related group of expressions. The next are *amazed disapproval*, M6+N1+E3+B2, where the conflict of disdain is partly eliminated and the bitter mouth is introduced, and *mild contempt*, M6+N1+E5+B1, where there are the bitter stubborn mouth and the attentive eye. There is still a trace of conflict here, for the mouth refuses while the eye attends. Then follow nos. 23 and 24.

23. *Contemptuous.* Unpleasantness (bitter mouth), rejection (set mouth), withdrawal (lowered lid), and attention (raised brow). M6+N1+E5+B2. This figure develops the meaningful contradiction of disdain, no. 22.

24. *Disgusted.* An enhancement of no. 23 by the addition of the dilated nostrils, which heighten the meaning of attention and point unpleasantness to the object. M6+N2+E5+B2.

In view of the fact that many contradictions are meaningful, it is of interest to note that there are some besides no. 12 and possibly no. 15 which appear to be senseless. No *O* has yet read sense into the following combinations: the attentive (open) mouth and the inattentive eye (lowered lid), M7+N1+E5+B1; the aggressive (clenched) mouth and the reverential (raised) eye, M9+N1+E2+B1; the sweet mouth and the turned up (dilated) nose, M2+N2+E1+B1.

To ensure a smooth-running demonstration, we arrange the 20 movable pieces as follows. A squared wooden rod is laid horizontally in V-shaped rests on the lecture-desk, below and in front of the tripod which carries the model; and the pieces are 'hooked' over this rod by the rectangular bits of wood glued to their backs. The audience thus sees the whole series of mouths, eyes, brows and noses, laid out in that order; and the lecturer, who has made his guiding marks upon the backs of the pieces, removes and replaces them as need arises. Some such arrangement is necessary, to prevent confusion and consequent loss of time; and the demonstration loses nothing by the show of dexterity with which the lecturer manipulates his facial keyboard.

IMAGERY IN THE WAKING AND DROWSY STATES[1]

By HULSEY CASON, University of Kansas

I. *General Statement*

The claim has been made by certain writers that the most prominent varieties of imagery present in the waking state are not the most prominent varieties in the drowsy state. Other writers have maintained that the particular kind of imagery predominating in the waking state also predominates in the drowsy state. My purpose in the present experiment was to investigate this matter further by a new method and with a larger group of subjects than had hitherto been used, and to determine whether or not this transformation of type is a typical experience. That is to say, I wished to determine to what extent the *non-verbal* (2,9)[2] imagery present in the *waking* state is different from that present in the *drowsy* state. I was concerned primarily with the *presence* rather than with the *function* of imagery.

At the beginning of *The Experimental Psychology of the Thought-Processes*, after proposing to turn out his mind for inspection, Titchener states that he lectures from three main cues under different circumstances: he says he reads off what he has to say from a memory manuscript, follows the lead of his voice, or trusts to the guidance of kinaesthesis (13, 8). He claims that he may look to all three kinds of prompting in the course of a single hour, but that some one method is followed exclusively at times: "thus, when I am tired, I find that vision and audition are likely to lapse, and I am left alone with kinaesthesis" (13, 8 f.). Ordinarily, however, visual and auditory imagery are superior.

H. L. Hollingworth and L. S. Hollingworth made a study of their own imagery in drowsy states during the two years prior to 1911. Hollingworth found that "modes ordinarily vague and feeble become here dominant and vivid, even tending to replace customary imagery habits." He states further that "along with this emphasis of unusual modes goes the subordination of dominant modes, so that in the drowsy state, as in dream life, images even of these unusual types seem to exceed by far in intensity the clearest images of the waking state" (7, 104 f.).

Another very interesting observation has been made by Colvin. "I find not the least difficulty in recalling, mentally, rich colors, and I have often experienced, just before falling asleep, visual hallucinations of a most pronounced character. I have elaborate visual dreams and have been able to get a complementary image from a memory image. Yet I seldom employ imagery in my thinking. In learning a word series I seldom see it as printed or written, my thinking being almost exclusively motor. Further, I have on several occasions experienced auditory hallucinations, but have, as far as I can discover, never had an auditory image. I recently experienced a most vivid taste dream, but I am reasonably certain that I have never used taste imagery in any of my thinking" (4, 235).

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[1] This experiment was performed at Columbia University in 1920.

[2] The first number in parenthesis refers to the appended bibliography, and the other number, separated by a comma, refers to the page.

Martin gave imagery tests to three Stanford students who were in the habit of seeing apparitions (8). She claims the tests showed "most clearly that whether we will see ghosts at all and what their appearance will be depends upon whether we normally project our visual images into space or are able to do so under the influence of a strong emotional stimulus" (8, 251); but "the ability to project one's image is but one of the factors involved in the seeing of ghosts" (8, 257). Two of her subjects (Mr. M. and Miss T.) were predominantly visual in the hallucinatory state, and tests in the laboratory showed that they were also predominantly visual in the "normal" state. She does not give details for the third subject, but claims that he furnishes further evidence for the same conclusion.

The following statements by Binet (which are somewhat aside from the main thesis of the *Psychologie du Raisonnement*) have a bearing on our problem: "Ajoutons que très probablement les visuels sont spécialement prédisposés aux hallucinations de la vue, et conséquemment aux genres de délire dont les hallucinations de la vue sont le symptôme" (3, 20-21). Again: "Il est possible que les hallucinés de l'ouïe et les individus atteints du délire de persécution appartiennent au type auditif; et que la prédominance d'un ordre d'images crée une prédisposition à un ordre correspondant d'hallucinations—et peut-être aussi de délire" (3, 26, also 41, 118-125).[3]

These citations show that there is considerable disagreement on the subject.[4]

I do not attempt to deal with any "complex of images." On certain difficulties with the term 'mixed type' the reader is referred to Segal and Fernald (10, 124-132, 175-179; 6, 130-132).

The present investigation was begun with a group of 20 normal subjects, but I have included in this report only the results of the 8 subjects who became especially interested in the experiment and who served over an extended period. All 8 subjects had had some previous training in psychology, and most of them had had special training in introspection. Six of the subjects—A, B, C, D, E and F (Table I.)—were women, and two—G and H—were men.[5] I first gave each subject a series of "subjective" tests to determine to what extent his imagery was visual, auditory, kinaesthetic (cf. 11; 1, 77-79, 95-96), tactual, organic (cf. 1, 79-80, 97), olfactory, gustatory (cf. 1, 80 and 97), thermal and pain imagery.[6] I later checked up these results by certain objective tests (6; 1).

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[3] In the *Psychologie du Raisonnement* the term hallucination does not have exactly the same meaning as in more recent times.

[4] Cf. Peillaube's *Les Images*, especially 1-100.

[5] None of the subjects was spiritualistically inclined.

[6] My classification of imagery is obviously somewhat arbitrary, but it should be stated that it does not differ from other classifications in this respect. Some of the classes mentioned are rather unimportant introspectively, but each one of even the last 4 varieties named may occasionally predominate for a short time. I have not attempted to deal with verbal (or, some-

I attempted to evaluate the factors of frequency and vividness, in spontaneous as well as in voluntary imagery. Furthermore, I made an effort to determine the nature of the imagery present in the drowsy state. All 8 of the subjects, whose results I have included in this report, had drowsiness hallucinations rather frequently. Although first dependence was placed on the introspective method, I think the very general conclusions regarding the differences in imagery in the waking and drowsy states, which are stated at the end of this report, are fairly reliable.

II. *Imagery in the Waking State*

Any test whose purpose is to determine imagery types quantitatively is of course liable to various errors. Shall we judge a person's visual imagery to be superior to his auditory imagery, for example, because it is more vivid or intense, "more profuse and more readily summoned," or "more accurate and reliable" (6, 18)? In the absence of well-defined desiderata, the most wholesome attitude to take is to pay some attention, where practicable, to *all* of these qualities, realizing that any criteria are sure to be arbitrary to some extent. All of the various criteria which have been used from time to time rest on *a priori* reasoning, and the present study is no exception. We have relied for the most part on frequency and vividness (cf. 10, 214-218, 230-231). The judgment which I formed of each subject's imagery in the waking state is a composite of (*a*) his spontaneous imagery (see 6), (*b*) his reactions to a large number of "equivocal" stimuli which were designed to furnish a good opportunity for each kind of imagery being evoked at different times (cf. 10, 222-228), and (*c*) his ability to call up imagery of each variety under very favorable conditions (voluntary imagery: see 2). The results obtained by these three tests were then averaged, and the percentage-scores for the 5 most prominent classes of imagery are shown in the left-hand portion of Table I. Each of these three tests now deserves a separate description.

(*a*) *Spontaneous Imagery.* After each subject had been questioned and interviewed personally about his imagery, he

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times, articulatory) imagery, partly because of the possibility that this so-called verbal imagery is nothing but sensations derived from actual muscular movements. I readily grant the possibility that a similar explanation might be made for several other kinds of imagery which are to be found in my classification (cf. 5, 105-107). Wherever it can be demonstrated that this is the case, then to that extent the study had to do with resulting sensations rather than with images. It is quite well known that it is next to impossible to distinguish accurately between an image and a sensation on all occasions.

was supplied with a small blank-book in which he was to record his imagery from time to time by a variety of the "interrupted thinking" method. The subject "interrupted" himself in a variety of mental operations over a period of several weeks, and made a record of the kind and vividness of the images (if any) which were present. Vividness was rated on a scale of five.[7] Some of the tests devised by Betts (2, 54-87) and Fernald (6, 24-31) were used as ancillary aids in obtaining fairly representative mental operations. The final records obtained from several hundred interruptions take account of both frequency and vividness.

In this test, visual imagery was the most prominent class for all subjects except E, who had more kinaesthetic imagery. Visual, auditory and kinaesthetic imagery included 86 percent. of the total score made by all of the subjects. Visual, auditory, kinaesthetic, tactual and organic imagery included 94 percent. Olfactory, gustatory, thermal and pain imagery was relatively unimportant.

(*b*) *Test with Equivocal Stimuli.* The second test used in determining the imagery present in the waking state consisted of some 200 words and phrases, selected with the idea of giving the various kinds of imagery about the same opportunity of being evoked (10).[8] These 200 stimuli, which consisted of such expressions as "the motion of a boat," "laughing," "electric shock," "peeling an orange," "door knob," "fire, fire," "horse galloping," etc., called up different kinds of images with different subjects. They were exposed consecutively to each subject, who was instructed to record the *first* image (if any) which he experienced; but no image was recorded if it did not clearly appear at once. In order to determine the consistency of this test, it was given a second time to each subject one month later. There was a very close agreement between the scores made on the two trials one month apart. I have used the average of the two trials in the calculations.

In this test, visual imagery was easily the most prominent variety in the case of every subject. Visual, auditory and kinaesthetic imagery included 73 percent. of the total score made

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[7] The scale used in grading these images was as follows:

As clear and vivid as an actual sensation	5
Almost as clear and vivid as an actual sensation	4
Moderately clear and vivid as an actual sensation	3
Rather dim and vague	2
Extremely dim and vague	1

The subjects were instructed to pay particular attention to the 1st, 3rd, and 5th grades, using the 2nd and 4th only on occasion (cf. 1, 101-104).

[8] I am indebted to Professor R. S. Woodworth for this list.

by all of the subjects. Visual, auditory, kinaesthetic, tactual and organic imagery included 89 percent.

(*c*) *Voluntary Imagery.* The third test used in determining the imagery present in the waking state was a questionary, the purpose of which was to test the subjects' *ability* to call up each kind of image under very favorable circumstances. In this test I took advantage of the work of Betts, and followed his questionary (2, 20-24) in many details.[9] After various stimuli had been tried out on a group, I selected the 10 stimuli, for each of our 9 categories of imagery, which seemed to offer the best opportunity for evoking that particular kind of image. Clearness was again graded on a scale of five, the final score thus taking account of both frequency and vividness.

The most marked characteristic of the results obtained with this voluntary imagery test is the greater distribution of imagery.[10] The scores made in visual imagery range from 11 to 50 percent. for different subjects (median at 15.5); auditory imagery ranges from 4 to 18 percent. for different subjects (med. at 13); kinaesthetic from 2 to 17 percent. (med. at 10); tactual from 8 to 23 percent. (med. at 14); and organic from 6 to 18 percent. (med. at 13.5).[11] Visual and auditory imagery is much less prominent relatively than in the two other tests. Touch and organic imagery is relatively more prominent.

(*d*) *Average Score of the Three Tests.* The final judgment of the general nature of a subject's imagery was obtained by averaging his percentage scores in these three tests.[12] The average result is shown in the left-hand portion of Table I. The figures in the table give only a very general idea of each subject's imagery (cf. 6, 130), and are not to be considered as very accurate. It is fairly safe, however, to assume from these figures that subjects A, C, D and G are predominantly visual. In no case does an average percentage score for a subject's olfactory, gustatory, thermal or pain imagery reach the figure 10, and such data are not given in the table.

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[9] The following directions will illustrate the type of questions used in Betts' questionary: "Think of seeing each of the following, considering carefully the picture . . . ; and classify the images suggested by each of the following questions as indicated by the degrees of clearness and vividness specified in the Key: . . . . The sun as it is sinking below the horizon . . . The stars on a clear winter night . . . " (2,22).

[10] This conclusion is in agreement with the work of Betts, who states that "most persons can command a far wider range and greater profusion of imagery than they normally employ in thinking" (2, 94).

[11] In his *Psychology* (4th ed.), 218, Angell writes that "the imagery which most of us find we can command with greatest accuracy and flexibility is that belonging to the perceptual process with which we are most intimately familiar, *i.e.*, vision, hearing, movement, and touch."

[12] We thus take account of the images which are *ordinarily* present, of the predisposition for certain kinds of imagery, and of the subject's *ability* to call up images of various kinds (cf. 10, 214).

If any changes should be made in weighting the three tests described above, I should be inclined to weight spontaneous imagery higher, because this type of test seems to fulfill the ideal conditions of a diagnostic imagery test better than any other. However, the general order of prominence would remain practically identical in every case. Weighting the percentage scores made in the spontaneous imagery test higher would simply *emphasize* the differences shown in Table I.

III. Imagery in the Drowsy State

We are dealing here with a group of phenomena which are allied with both the waking and dream states, belonging, as Ellis says, to the "ante-chamber of sleep." The subjects, whose records I include in this account, had drowsiness hallucinations rather commonly, and seemed able to distinguish fairly well between the drowsy and dream states. The plan which I adopted was to have each subject make a record of his drowsiness hallucinations over an extended period. The subject was supplied with a small blank-book and was given specific instructions in regard to the way these records were to be made. He was instructed to write out an accurate account of the hallucination immediately after he experienced it, regardless of whether it seemed individual, commonplace, or attractive,—and he was also cautioned not to *look for* any particular kind of imagery. He was especially cautioned to make a record of *all* of his hallucinations *immediately* after he experienced them, and to use much descriptive language. Judging from the assurances given me by all of the subjects, I am inclined to believe that the hallucinations reported are fairly representative.[13]

I later read all of the hallucinations recorded, and listed the kind, and sometimes the vividness, of the images which had probably been present. If a subject saw things in his drowsy states, this fact would find its way into his report. While language is sometimes a symbol for meaning (2, 8), such expressions as "I saw clearly," "I could barely hear," "I felt a whirling sensation," "My muscles strained as I jumped," and similar expressions, give some indication of the kind of imagery present. All of the subjects were interviewed by me every three or four days; and each subject continued to record his drowsiness hallucinations until I was reasonably certain that I knew the general nature of the imagery which he experienced in this state.

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[13] One advantage in this procedure is that no effort is made to direct the subject's thinking; and whatever may be lost by not being able to "set a definite problem" is more than compensated for by the factors of spontaneity and naturalness.

Since none of the subjects knew the purpose of the experiment, I think the factor of suggestion, commonly encountered in experiments of this nature, had practically an equal opportunity of operating for all kinds of imagery.

I was not interested in those images which were only implicitly present (cf. 2, 7-8).

The right-hand portion of Table I. shows the distribution of imagery in the drowsy state. Visual imagery is in the lead for most subjects. The five categories of imagery shown in the table represent 97 percent. of the total score made by all of the subjects. Pain, thermal, gustatory and olfactory imagery is practically absent.

TABLE I.

Imagery in the Waking and Drowsy States

Subject	Percentage Score									
	Waking					Drowsy				
	V	A	K	T	O	V	A	K	T	O
A	67	8	2	9	3	47	16	14	12	4
B	39	24	15	5	6	50	39	7	2	1
C	57	10	5	11	3	93	7	0	0	0
D	33	15	16	9	11	30	37	11	5	12
E	30	9	27	9	12	19	42	23	1	14
F	31	23	13	10	9	47	29	18	6	0
G	36	11	12	5	14	35	15	23	17	4
H	31	10	13	11	5	44	50	6	0	0

The *letters* V, A, K, T and O in the above table refer to visual, auditory, kinaesthetic, tactual and organic imagery, respectively. The *figures* in the table represent percentage scores. For example, subject B has 39 percent. visual imagery in the waking state and 50 percent. in the drowsy state. The left-hand portion of the table gives the average score made in the three tests for imagery in the waking state. These three tests are described in Section II. In visual imagery, for example, subject B made percentage scores of 42 in spontaneous imagery, 58 in the test with equivocal stimuli, and 18 in voluntary imagery, giving a final average score of 39 in visual imagery in the waking state. The percentage scores for the 4 types of imagery not shown in the table (pain, thermal, gustatory and olfactory) constitute a very small percentage of the whole. The figures in the table possess only a very general value.

IV. Results

The data of Table I. are represented graphically in Figure I. The 5 varieties of imagery shown in the figure represent a higher percentage of the total score in the drowsy state than in the waking state. It is possible that this is a spurious result, however, because of the disturbing memory factor. In the case of several subjects, auditory imagery especially is more prominent in the drowsy state.[14]

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[14] Ogden makes a similar observation in his *Introduction to General Psychology*, 81. It is interesting in this connection to observe that auditory hallucinations are prominent in several abnormal conditions (9, 30).

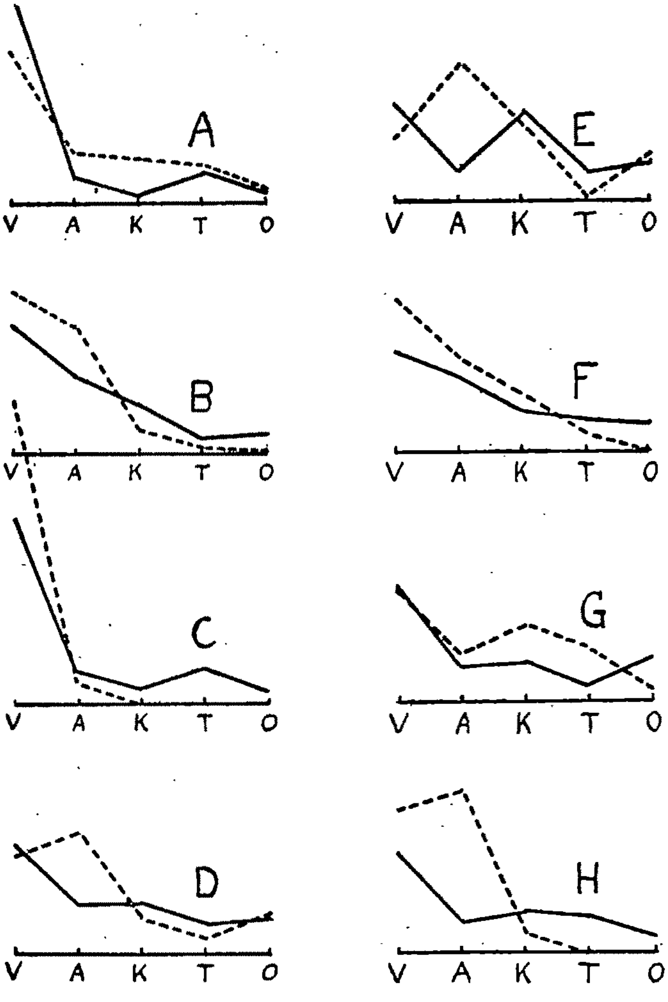

FIGURE I.

Imagery in the Waking and Drowsy States

The letters V, A, K, T and O in the accompanying figure, as in Table I., represent visual, auditory, kinaesthetic, tactual and organic imagery, respectively. The distances above these letters, on the base lines, show the percentage scores (1 mm. for every 2 per cent.). The subjects are designated by the large capitals (A, B, etc.).

There is a well-marked shifting of certain types when passing from the waking into the drowsy state in the case of subjects E and H. Subjects B and F, on the other hand, maintain about the same order of prominence in the two states. We should naturally not be justified in concluding from these data either that there *is* a common transformation of the dominant types or that there *is not*. The most marked characteristics which we observe are the individual differences between the various subjects and a general irregularity of the results (cf. 6, 20-21; 10, 218-222).

Most of the drowsiness hallucinations which the subjects reported were composed of the subject-matter of every-day life, with recency as the principal determining factor. A few of the hallucinations were grotesque in character, and some were like a "flash in the pan." There were no veridical hallucinations. The following may be given as a fairly common type of hallucination: "11:30 p. m. Just before falling asleep; in bed. Imagined myself stepping backwards off the subway platform. Reflex jerk of entire body. Image distinctly kinaesthetic, accompanied by visualization of the situation. I was not asleep, but nearly so."

The following hallucination illustrates the irrelevant variety: "12:30 a. m. Saw quite plainly a group of 3 men and 3 women standing around a mahogany table, discussing something. Then I heard one of the women say very distinctly, 'That being the case, I think I shall get drunk too.' The picture then faded completely." [15]

V. Imagery in Dreams

An effort was also made to study the dream state,[16] which is characterized by little power of co-ordination and often by a flight of images, but lack of time kept both subjects and experimenter from continuing the investigation. The method by which I attempted to determine the kind of imagery present in the dream state was the same as that described in Section III. The following data are admittedly not very reliable, because so few observations were made.

IMAGERY IN DREAMS[17]

Subject	Percentage Scores					Number of Observations
	V	A	K	T	O	
A	38	29	5	19	5	21
B	45	29	12	7	2	41
D	44	22	22	3	6	36
E	29	21	29	0	21	14
F	56	17	0	11	0	18

It might be pointed out that, with the exception of a few irregularities, there is a very general similarity between the nature of the imagery present in the dream state and in the other two states, particularly in the case of subjects B and D.[18]

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[15] Many examples of hallucinations (of very unequal value) may be found in the lengthy English *Report on the Census of Hallucinations*, Proceedings of the Society for Psychical Research, 1894, 10, 25-422.

[16] A very interesting account of this state is given by B. Sidis: An Inquiry into the Nature of Hallucination, *Psychol. Rev.*, 11, 1904, 104-137. Some good examples of hallucinations are given by H. B. Alexander: Some Observations on Visual Imagery, *ibid.*, 11, 1904, especially 324, 328 and 329.

[17] The distribution of imagery shown in this table is in close agreement with that obtained by Thompson, who "dissected" 190 dreams of 5 sub-

VI. *Conclusion*

1. The "transformation of imagery type," when passing from the waking into the drowsy state, is not a typical experience; and no general formula or rule can be stated which will hold for all cases. There are marked individual differences in this respect.

2. There is often a very noticeable increase in the relative prominence of auditory imagery when passing from the waking into the drowsy state.

3. There is a slightly greater similarity between the imagery present in the waking and dream states than between that present in the waking and drowsy states.

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jects (12). Thompson concludes that "that class of imagery is most frequent in dreams which is most frequently employed in waking life" (12, 305).

[18] Olfactory, gustatory and pain imagery was practically absent. Subject F had a score of 17 percent. in thermal imagery. So few observations were made by subjects C, G and H that I do not give their results.

THE APPROXIMATION OF ACTUAL DATA TO THE PHI-GAMMA HYPOTHESIS

By F. M. Urban

L. B. Hoisington[1] published some time ago a short report on the results of a determination of the limen of dual impressions upon the skin. The data were fractionated in groups of 25, 50, 100, and 200 series. After calculating the constants *h* and *c* for every group, Hoisington determines the sums of the squares of the deviations of the actual percentages from the theoretical percentages. These numbers show the amount of agreement between the observed and the calculated results and are, therefore, a test of the degree in which the $\Phi(\gamma)$-hypothesis is realized in practice.

The results are given in two tables, one for each observer. The averages of the sums of the squares of the deviations show a highly uniform ratio of decrease. The mean variation of these sums also tends roughly to vary inversely with the number of series. These results seem to indicate that we approach the theoretical function as we increase the number of experiments. This is particularly true of Hoisington's observer D, and we must conclude that in his case the $\Phi(\gamma)$-hypothesis is approached with practice. The departure of the second 100 experiments from theory is only about one eighth of the first 100, and the last 50 fit theory over five times more closely than the first 50.

The $\Phi(\gamma)$-hypothesis can not fail to attract the interest of every worker in psychophysics, who will be grateful for any light thrown on this question. Hoisington's observation is all the more important, since it agrees with one made by Thomson[2]. In his research on the psychometric functions Thomson analyzed the results of my experiments on lifted weights and found a better agreement between theory and observation in the results of the more practised observers. Should further experience bear out this observation, we should be in the possession of a fact of far reaching practical and theoretical consequences.

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[1] L. B. Hoisington, An Example of the Fractionation of Data from the Method of Constant Stimuli for the Two-Point Limen, this JOURNAL, 28, 1917, 588–596.

[2] W. Brown and G. H. Thomson, *The Essentials of Mental Measurement*, 1921, 90 ff.

It is not likely that an investigation will be undertaken with the express purpose of testing Hoisington's observation. The amount of experimentation for settling this question would have to be very large, so large indeed that it might seem out of proportion to the point to be decided. At the present stage it is perhaps not even advisable to undertake such an investigation, since the possibility remains that Hoisington's and Thomson's observations are erroneous. A purely negative result is always disappointing, and it is particularly so if the investigation required a large outlay of time, money, and energy. At present it seems to be a case of incidental observations, and for this reason I wish to call the attention of investigators to this problem with a view to induce them to make similar observations.

I also wish to point out that a large material is at hand on which Hoisington's observation might be tested. Fernberger's doctorate thesis contains the data of two very extended series of experiments on lifted weights. The data are fractionated and the constants *h* and *c* are given partly in Fernberger's thesis, partly in my paper on the effect of practice in lifted weights. All that remains to be done is the calculation of the sums of the squares of the deviations, and this is a comparatively simple affair. A competent computer might do it in a day or two. My present circumstances prevent me from doing this work myself, but I hope that this suggestion may induce some person to undertake this small piece of research. The result is sure to be interesting. It is true that both of Fernberger's subjects were highly practised observers, in whom the effect of practice is less noticeable. One may expect to trace this influence, nevertheless, since the influence of practice on the value of the constants *h* and *c* is quite indisputable.

THE APPROXIMATION OF ACTUAL DATA TO THE PHI-GAMMA HYPOTHESIS

By Samuel W. Fernberger, University of Pennsylvania

In accordance with the suggestion made by Urban in the article just preceding, the calculations were made of the theoretical values of the curves of the 'greater' and 'less' judgments. The values *h* and *c*, the constants of the psychometric functions, are to be found in Tables 18 and 19 in Urban's article on practice.[1]

Table 1

Number	Lighter Judgments	Heavier Judgments
10	0.00528165	0.00569811
11	0.00206896	0.00579367
12	0.01018162	0.00275773
13	0.02570003	0.00439892
14	0.01102116	0.02515375
15	0.00309432	0.00806441
16	0.03949521	0.00661291
17	0.02073429	0.00225738
18	0.01522867	0.00881963
19	0.01508111	0.00991014
20	0.01071539	0.01572607
21	0.01239068	0.02139374
22	0.01329113	0.01770968
23	0.01522534	0.03618851
24	0.00434233	0.02198976
25	0.06417478	0.00495409
26	0.00189263	0.00458539
27	0.00026153	0.00115018
28	0.00888697	0.00055554
29	0.00143610	0.00500059
30	0.00364235	0.00033702
31	0.01978015	0.00582996
32	0.01312453	0.00199937
33	0.00771604	0.04510175
34	0.00660163	0.00859111
35	0.00862567	0.00356635
36	0.00960937	0.01359313
37	0.01315930	0.03513948

The observed relative frequencies are to be found in Tables 16 and 17 of the same article. The results of Urban's Observer VIII were calculated in full. This involved 28 groups of 50 judgments on each of 6 comparison pairs. Only the last 28 groups for Observer II were calculated. The first 9 groups, the constants of which are given in Table 18, were made by this observer in an earlier investigation, a number of years prior to his subsequent practice. The group numbers in our tables, given below, conform to the group numbers in Tables 18 and 19 of the Urban article.

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[1] F. M. Urban, Der Einfluss der Uebung bei Gewichtsversuchen, *Arch. f. d. ges. Psychol.*, 29, 1913, 271–311.

TABLE 2

TABLE 19 NUMBER	LIGHTER JUDGMENTS	HEAVIER JUDGMENTS
1	0.02081909	0.00587067
2	0.00504132	0.01469981
3	0.00332283	0.00641695
4	0.03364268	0.01863544
5	0.01477663	0.00671550
6	0.00772576	0.00706596
7	0.01695835	0.01060964
8	0.00263027	0.00084990
9	0.01515844	0.00817609
10	0.03513519	0.04134780
11	0.00878956	0.00605952
12	0.00659333	0.00783328
13	0.01396908	0.01723052
14	0.00613113	0.00494092
15	0.00930283	0.00621563
16	0.00377251	0.00607283
17	0.00376331	0.00201803
18	0.00823965	0.00551722
19	0.01447011	0.00856419
20	0.00596861	0.01182627
21	0.00689667	0.00263735
22	0.02143435	0.00386525
23	0.01367870	0.00781873
24	0.01053391	0.00299834
25	0.00531802	0.01850315
26	0.00934552	0.04598085
27	0.00536269	0.00223346
28	0.00965603	0.00407375

We had the theoretical curves calculated for the greater and less judgments for the stimulus values corresponding to those used in the investigation. These calculated values were then compared with the observed relative frequencies of judgment, and then the sums of the squares of the deviations were calculated. These sums of the squares of the deviations are given in Tables 1 and 2. The results of Observer II are given in Table 1, and these correspond to the values found in Urban's Tables 16 and 18. Table 2 contains the values for Observer VIII and corresponds to Urban's Tables 17 and 19.

The results seem to verify the observations of Hoisington[2] and Thomson[3] in that there is a closer approximation of the observed to the theoretical curves of the psychometric functions as a result of practice. The difference seems to be more marked for Observer VIII than for Observer II. This result might have been anticipated because Observer II was relatively more highly practised at the beginning of experimentation, having participated as observer in a former investigation. This also is evident from a study of the magnitudes of the constants of the psychometric functions which we have made previously for these same two observers.[4]

The decrease in the size of the sums of the squares of the deviations is far from regular but the general tendency seems evident. This tendency is more marked than the values indicate, as an analysis shows that a large sum of the squares in the earlier series is invariably due to large deviations on at least three or four of the six observed frequencies. Large deviations in the later series, such as that for Group 26, Heavier Judgments, Observer VIII (Table 2), are almost invariably due to a large deviation (frequently an inversion) for one of the observed frequencies only.

In the results of Thomson and especially in those of Hoisington the decrease in the size of the sum of the deviations is very much more marked than in the present results. This is because Hoisington used entirely untrained subjects and calculated from the first judgments obtained. For the present values, the observers had reached a fairly high degree of practice before the judgments were recorded.

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[2] L. B. Hoisington, An Example of the Fractionation of Data from the Method of Constant Stimuli for the Two-Point Limen, this JOURNAL, 28, 1917, 588-596.

[3] W. Brown and G. H. Thomson, *The Essentials of Mental Measurement*, 1921, 90ff.

[4] S. W. Fernberger, On the Relation of the Methods of Just Perceptible Differences and Constant Stimuli, *Psychol. Mon.*, No. 61, 1913, 41-45.

ALCOHOL, TOBACCO AND TEA:
THE EFFICIENCY UNDER DRUGS AS DERIVED FROM THE LEARNING EQUATION

By MAX F. MEYER

Let no one get from the title the erroneous impression that this report is concerned with the problem whether a person's learning ability may be changed by drugs. That is not the problem here. The *learning* equation enters into the problem only incidentally, but *unavoidably, as we shall see.* The purpose of the experiments was to determine *the relative efficiency in doing work* under the influence of various drugs.

Not a few people can be found who have the habit of taking, about half an hour before going to work, a particular drink. Some of them expect no special consequence therefrom. Some, on the other hand, take that drink intentionally as a "tonic." That is, they expect that that drink will either increase the efficiency of their work or at least make them more happy and contented during the performance of the task.

Others have the habit, not of taking a particular drink as a tonic for their work, but of smoking tobacco before or rather during their work.

Still others have, not the habit, but what they would call the unfortunate fate, of being compelled now and then or frequently to work in rooms where the air is filled with tobacco-smoke. If they are not smokers themselves, they feel then very uncomfortable during their work and naturally expect from such a condition, in addition to other consequences which do not concern us here, a diminished work-efficiency.

The present study was undertaken in order to throw more light on the questions which are suggested by the contents of the preceding paragraphs. No attempt was made to solve or even only to formulate any specific problem in neurology or pharmacology. The problem and the procedure adopted to solve it resembled a problem of ordinary business. But the procedure had nevertheless its scientific aspects. First, a kind of work was chosen which permitted a large number of repetitions under virtually identical conditions. The repetitions permitted the elimination of the influence of the (known as well as unknown) chance factors. Secondly, the conditions under which the work was done were held as identical as possible except where they were intentionally varied. Thirdly, a record was regularly taken of the time required for the work. Fourthly, the procedure and the computations made were based on a previous analysis of the learning equation as discussed in another paper.[1]

The "business" whose efficiency was to be observed consisted in writing shorthand exercises. The "worker" did not write these exercises in order to please somebody interested in some scientific problem. Neither did he write them because he was paid for the work by the hour. He wrote them in order to do himself a favor. He did not have to whip himself by suggestion, loyalty, gratefulness or other artificial means into an artificial state of attention and interest. He was actuated by the most reliable motive, sheer selfishness. He *wanted* to do that very work for his own well-understood benefit, and to do it with the least loss of time.

The first worker, whose records were taken in the summer of 1921, was a middle-aged man, who had used weak alcoholic drinks now and then in his youth, but so rarely that even in that period of his life he could be said to have been an abstainer. He had been a total abstainer during the last twenty years. He had never smoked in his life. He did not customarily drink either tea or coffee or "cokes" and similar drinks.

He had learned a certain system of shorthand, a little over a year before, to the extent of knowing all the rules of the system, and of being able, therefore, to read and write anything in that system. But he could do that generally only very slowly, and he was anxious to improve his reading and writing speed by systematic exercise. He procured a list of either most frequently occurring or (with respect to the rules) most typical words, well illustrating that whole system of shorthand. This list he copied just once at each sitting. To be more exact: he looked at each word (or later, with increasing practice, whenever possible, small groups of words) as it appeared in shorthand in that list until he had "read" it, then turned his eyes to his copy book and, from memory or aided by his knowledge of the system of rules, "wrote" it *with amply sufficient neatness* to feel sure that he could again easily read it if that might be necessary at any future time.

The unit of time in which (in this first experiment only) the duration of each work-period was recorded, was the rather large unit of .5 min. It was thought that, in work of this kind, where a pencil may have to be exchanged, a page may have to be turned, a readjustment of the sitting posture may have to be made, that is, in work which is so much like ordinary business, a very small time unit would not commend itself to reason. The measuring stick need not be finer than the tools with which the work is done. However, experience showed that a smaller unit, say, .1 min., would have been better; and seconds might then as well have been read off the watch, even though the accuracy of the experiment does not go as far as that. (Seconds were used in the later experiments.)

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[1] This JOURNAL, April, 1923.

The worker had the habit of eating only two meals a day, breakfast and supper. Being thus without occupation during the lunch period, he chose that time of the day for his practice period. Since everybody else who might otherwise have disturbed him was at that time engaged elsewhere, he was able to do his work without any interruptions.

From 20 to 30 min. before starting his work he took his "tonic." He used four different kinds, sugar, tobacco, alcohol, tea. He used them on successive days in the order named; on the fifth day he started again on sugar, and so forth. He tried to work regularly on all the successive days of the week; but now and then (not often) he had to drop a day on account of duties taking him elsewhere. The work seemed to progress on the days following such an intermission so nearly in the same way as if that intermission had not occurred, that no attention is paid in the following discussion to these intermissions.

If the intermission could have had an unfavorable influence on the efficiency of the work of the day immediately succeeding, it would have been unfavorable about equally to the showing made by the different "tonics." Maybe the sugar, and even more the tea, should have made a showing a trifle better than our following records.

In the numerous experiments after this first—let it be said at once—there were virtually no intermissions, and this matter will therefore not be referred to again.

The tea was prepared by placing 5 gr. of green tea leaves in about a pint of boiling-hot water together with 20 gr. of sugar, and removing the tea-leaves after 6 min. by filtering.

The alcohol was prepared for drinking by mixing 10 cu. cm. with about a pint of cold water together with 20 gr. of sugar.

The tobacco was administered by placing 30 gr. 30 min. before the beginning of the work on a hot iron plate, where it turned to ashes in about 20 min., filling the room with smoke. The worker, of course, remained in the room from the moment he put the tobacco on the plate, and did not enter the room only for the beginning of his work 30 min. later. The size of the room was 22 by 16 ft. and the height 18 ft. The windows were closed in order to confine the fumes, but the door was kept open all the time so that there was at least that amount of ventilation which one is apt to find in rooms where men smoke and work at the same time. The burning tobacco was about 12 ft. from the chair on which the worker sat occupying himself with reading and the like while waiting for the proper time for the beginning of the "business." The room was still very smoky after the business was done, an hour later.

The sugar was taken 20 gr. in a cup or two of water. Of course, no particular effect was expected from the sugar; but since the worker "took something" always 30 min. before starting his work, it was thought that sugar would be about the most innocent thing that he could take and still call it "something."

The worker did not like (or rather, greatly disliked) either the tea-drink or the alcohol-drink or the tobacco-fumes. He called all of them "vile stuff." He felt uncomfortable all the time while being in the tobacco fumes. And under the influence of any of these three substances he complained of being slightly nauseated for an hour or two.

The selection of the particular kind of work, a shorthand exercise, was not purely the result of the fact that this worker regarded this as very useful for him and was therefore very eager to do it,—a fact which does not hold true for all psychological tests described in the literature. The shorthand work in this case was truly useful work. But, in addition, any shorthand work done for practice is truly intellectual work. It is not so exclusively memory work as, for example, adding or subtracting or multiplying numbers.

The shorthand work requires not only a knowledge of a number of rules (to be applied mechanically like the multiplication table), but also a considerable amount of "choice" in the application of each rule whereby to obtain in each individual case (of a word or group of words) a greater or lesser amount of economy, whichever the writer may be satisfied with. These intellectual processes play their rôle both in reading and in writing shorthand.

May it be repeated here what has been said on another occasion?[2] One serious deficiency of the work done hitherto (usually under the pressure of somebody wanting immediate results) on efficiency under drugs consists in largely or totally avoiding those processes which are "subject to improvement during continued testing. They are the very processes that interest us most. An artist drinking a bottle of wine before going to work does not intend thereby to improve his knee-jerk. And the student who drinks a cup of tea before a mathematics examination does not intend thereby to improve his use of the multiplication table. The highest intellectual processes ought to receive the chief attention of the experimenters of the future."

Since the improvement popularly called "learning" plays its *foremost* rôle in the highest, the *intellectual* processes of human life, it is *prerequisite* to develop a method of describing in accurate (mathematical) terminology the *efficiency in successive stages of the learning process under normal or average conditions,* before we can make a comparison of the efficiency under drugs. This prerequisite the writer has fulfilled in another place, in his article on the learning equation, to which reference has already been made and will further be made.

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[2] Max F. Meyer, The Psychological Effects of Drugs, *Psych. Bulletin,* 19, 1922, 173–182, esp. 178.

The procedure in this experiment has for the most part already been explained. A few details still have to be added to what has been said. At first, 30 min. were consumed in writing the list of selected shorthand words. But after a week that time had been reduced to nearly one-half of the original time. A second practice with different material was therefore taken up immediately on completing the first. The first is in the following tables referred to by A and the second by B.

The second practice, B, consisted in copying, not a logically disconnected list of words as in the first, but a few commercial letters found in the shorthand text-book. After a few more days a third practice, and again after a few days a fourth practice, were added. These are in the following called C and D. They also consisted in typical, brief commercial letters.

Toward the end of the whole experiment, lasting about 6 weeks, each group of the 4 practice periods A, B, C and D combined with short intervals for the necessary adjustments filled a little over an hour of the worker's time.

The constant L, defined as the individual learning ability, has been taken in all the computations concerning work of this same individual as 20.1. No attempt at any extraordinary accuracy has been made in determining the other three constants, T, p and a. None of these constants is wanted for its own sake in the experiment under discussion. They are wanted only in order to make possible the substitution of a computable curve for the experimental curve.

TABLE A

$L = 20.1$, $T = 21.0$, $p = 2.4$, $a = 1.3$.

Sugar			Tobacco			Alcohol			Tea		
n	t c'p't'd	t exp'l	n	t c'p't'd	t exp'l	n	t c'p't'd	t exp'l	n	t c'p't'd	t exp'l
0	60.7	60	1	49.4	52	2	43.1	46	3	39.1	36
4	36.3	34	5	34.2	30	6	32.7	32	7	31.4	30
8	30.4	32	9	29.6	30	10	28.9	32	11	28.3	29
12	27.8	34	13	27.4	28	14	27.0	34	15	26.6	26
16	26.3	28	17	26.1	27	18	25.8	27	19	25.6	27
20	25.4	29	21	25.2	27	22	25.0	27	23	24.9	24
24	24.7	28	25	24.6	25	26	24.5	26	27	24.3	23
28	24.2	25	29	24.1	22	30	24.0	24	31	23.9	23
32	23.9	27	33	23.8	24	34	23.7	25	35	23.6	23
36	23.6	23	37	23.5	24	38	23.4	25	39	23.4	20
40	23.3	24	41	23.3	23	42	23.2	24	—	—	—

The above Table is immediately clear to anyone familiar with my previous article on the learning equation, in which the definitions of all the terms are given. With the actual computation I hesitate to bore the present reader. If he wishes to "audit" my computation he may, if he is a teacher of psychology, give the computation to some of his students. It is good practice for somewhat advanced students. Good practice for them, too, it will be to plot both the computed curve and the experimental curve and to discuss whether one should vary slightly p and a from the values given above in order to obtain a closer approximation of the computed curve to the experimental curve. As was said before, however, for our present purposes no very close approximation is needed.

Let it be pointed out to those who wish to make the computation, that they must first find a suitable value of K. We proceed as follows. First we plot the experimental curve and make a guess with respect to its probable fall until *n* becomes very large. It looks like a fair guess that 21 would be about the asymptotic limit; that is, that T is about 21. Since K is equal to the product of (L–1) and T, divided by 180°, and since we found for this individual in our previous article L equal to 20.1, K must equal about 19.1 times 21 divided by 180, or 401.1 over 180, or about 2.23. Let us accept this provisionally. Table A proves that T equal to 21 was indeed a fair enough guess.

In order to utilise the four columns of the Table for comparative purposes, we *compute now each experimental value as a certain per cent. of its computed value.* That is, we divide each experimental t by the corresponding computed *t* and multiply the quotient by 100. For example, we divide 60 by 60.7 and get 99%, we divide 52 by 49.4 and get 105%, we divide 46 by 43.1 and get 107%, and so forth. I do not wish to bore the present reader with the enumeration of all the results per cent. I give below at once the average for each of the 4 columns of the table, that is, for each of the 4 drug conditions.

Summary of Experiment A

Alcohol	107	(98 to 126)
Sugar	106	(94 to 122)
Tobacco	100	(88 to 107)
Tea	96	(86 to 106)

I have also given the extremes (in parenthesis) for each of the four drug conditions. *No meaning at all is conveyed by the averages (or the extremes, either).* They are not intended to convey any meaning by themselves. But compared with each other they have some significance. The values 107 and 96, for instance, can be said to mean this: on an average "tea day" the shorthand writer worked about 11% more efficiently than on an average "alcohol day." The "tobacco days" have an intermediate position. Little difference is apparent between the "alcohol days" and the "sugar days." Nevertheless, the alcohol may have placed the worker at a slight disadvantage in comparison with the sugar.

TABLE B

L = 20.1, T = 26.0, p = 2.8, a = 1.2.

Sugar			Tobacco			Alcohol			Tea		
n	*t*	t	*n*	*t*	t	*n*	*t*	t	*n*	*t*	t
0	71.8	68	1	60.1	60	2	53.2	54	3	48.5	48
4	45.3	52	5	42.8	46	6	40.9	40	7	39.4	42
8	38.2	43	9	37.2	35	10	36.3	41	11	35.5	34
12	34.9	43	13	34.3	32	14	33.8	35	15	33.4	31
16	33.0	34	17	32.7	31	18	32.3	33	19	32.0	31
20	31.8	32	21	31.5	29	22	31.3	33	23	31.1	32
24	30.9	34	25	30.7	29	26	30.6	32	27	30.4	28
28	30.3	29	29	30.1	28	30	30.0	31	31	29.9	27
32	29.8	29	33	29.7	28	34	29.6	30	—	—	—

Summary of Experiment B

Sugar..............	106	(95 to 123)
Alcohol.............	104	(98 to 113)
Tea................	97	(90 to 107)
Tobacco............	96	(92 to 107)

In order to have a suitable K for the above computation, we first had to make a guess as to what T might equal. We guessed at T equal to 26. K is then 19.1 times 26 divided by 180, or 496.6 over 180, or 2.76. The actual computation of the Table proves that the guess was fairly good,—good enough for our present purpose. In order to utilize the four columns of the Table for comparative purposes, each experimental t has again been divided by the corresponding computed *t*. The Summary gives us again the average of these values per cent. for each column of the Table, and, although these averages are quite meaningless for themselves, as already said, they permit us to measure the relative efficiency.

Here, in Experiment B, the sugar days make a showing slightly poorer than that of the alcohol days. And tea and tobacco also have exchanged places. But there is no doubt that tea and tobacco belong to the same class as compared with sugar and alcohol, which belong to a different class, the former two representing greater, the latter two lesser efficiency.

TABLE C

L = 20.1, T = 34.0, p = 2.1, a = 1.8.

Sugar			Tobacco			Alcohol			Tea		
n	*t*	t	*n*	*t*	t	*n*	*t*	t	*n*	*t*	t
0	87.4	86	1	70.6	73	2	61.9	59	3	56.5	54
4	52.8	55	5	50.1	49	6	48.2	54	7	46.6	43
8	45.4	47	9	44.4	44	10	43.5	44	11	42.8	43
12	42.2	46	13	41.6	43	14	41.1	44	15	40.7	39
16	40.4	37	17	40.0	40	18	39.7	39	19	39.5	36
20	39.2	40	21	39.0	35	22	38.8	39	23	38.6	35
24	38.4	37	25	38.2	36	26	38.1	39	27	37.9	37
28	37.8	38	29	37.7	33	30	37.6	38	—	—	—

Summary of Experiment C

Alcohol.............	102	(95 to 112)
Sugar..............	101	(92 to 109)
Tobacco............	97	(88 to 103)
Tea................	95	(91 to 100)

Having guessed T in Experiment C to be about equal to 34, we find K to be 649.4 over 180, that is, we find K to be equal to 3.61. The Table proves that our guess at T was good enough for our purpose. The summary of Experiment C gives us the average of the ratios per cent. for each of the 4 columns of Table C. The 4 drug conditions have again the same relative positions as we found them in Experiment A. The advantage of tea over alcohol, however, here amounts only to 7%.

TABLE D

L = 20.1, T = 27.0, p = 3.6, a = 1.3.

Sugar			Tobacco			Alcohol			Tea		
n	*t*	t	*n*	*t*	t	*n*	*t*	t	*n*	*t*	t
0	61.6	60	1	54.2	52	2	49.4	54	3	46.1	46
4	43.6	43	5	41.7	40	6	40.1	41	7	38.9	37
8	37.9	42	9	37.0	39	10	36.3	38	11	35.7	33
12	35.1	34	13	34.6	32	14	34.2	35	15	33.8	36
16	33.5	34	17	33.2	33	18	32.9	33	19	32.6	31
20	32.4	31	21	32.2	33	22	31.9	33	23	31.8	30
24	31.6	32	25	31.4	29	26	31.3	31	—	—	—

Summary of Experiment D

Alcohol..............	103	(99 to 109)
Sugar..............	100	(96 to 111)
Tobacco............	97	(92 to 105)
Tea.................	97	(92 to 107)

In D the value of T was provisionally taken as 27. And K was therefore 515.7 over 180, that is, 2.87. We see in the summary of Experiment D that the 4 drug conditions have again the same relative positions as we found them in Experiments A and C, except that tobacco and tea are tied. It is also worth calling attention to, that Experiment D consists only of 27 measurements, from 6 to 7 for each drug, and that the small efficiency-difference of only 6% between alcohol and tea may partly be due to the drug-effect being already on the decline, since about 90 min. had passed from the moment of taking the drug to doing the work.

Summing up the results of all four experiments, A, B, C, D, we can very well draw the conclusion that tea and tobacco belong to one class and sugar and alcohol to another class (or, maybe, the latter two to two other classes); that even greater efficiency results from tea than from tobacco; and that there is a certain suspicion that even this small dose of alcohol diminishes the work-efficiency in comparison with sugar.

Of course, the alcohol dose in these experiments must be pronounced exceedingly small. This suggested the taking of a stronger dose in a future experiment.

The worker, when asked if he was not anxious now, after seeing the results, to take to tea or tobacco or both, replied in the negative. One must remember that the general hygienic, not to mention the social, problem of the use of drugs is in no way touched upon in the performance of these experiments. It is highly probable that the increased efficiency is somehow paid for (not in money,—that is not the question) physiologically. And it is not unlikely that the bargain is not worth the price paid.

A very remarkable fact is this, that the worker, during and after virtually every tobacco or tea work, said to himself something like this: I certainly am working slowly; a mysterious weight seems to force me to drag it along, and thus seems to keep me from going forward as fast as I might otherwise. And then, when he looked at the outcome, he found to his surprise that he had worked much better than he expected. On the other hand, the work of the alcohol days seemed to fly ahead and pull the operator after it. And then, when he looked at the outcome, he found to his surprise that his efficiency was much poorer than he had expected. It seems clear that a worker who lacks means of objectively measuring his efficiency can not trust his own feelings in the matter.

Let it be recalled in this connection that all these drugs except the sugar were nauseating to the worker, not only at the moment of taking them, but until the end of the time spent in the work. Especially the tobacco fumes were very disagreeable to him. But no such experiences, obviously, can be regarded as indicators of a worker's efficiency. This is entirely in accord with Tinker's remark[3] that "intended distractors may fail to distract" and with my own teaching[4] as expressed, for example, in the following quotation: "He who thinks for the first time of the deflection, in the nervous system, of a current by another current, may be struck by the effect which it has on the deflecting current. Is not the deflecting current strengthened thereby? . . . A little noise and a little glitter on the side do not distract, but help." Let us remember that the worker eagerly wanted to do those shorthand exercises and had made up his mind to do them, "rain or shine." It may even be that the "helpful" effect of the "distraction" accounts (although the present writer does not hold this view very strongly) for the unexpectedly high efficiency (*i.e.*, unexpectedly small inefficiency) of the alcohol days.

Some remarks about the behavior of the constant "a," the practice value of the task, and incidentally about the other constants, may not be entirely out of place here, although they have no relation to the drug effects, but only to the method of interpreting the drug effects on the basis of the learning equation. It is most plausible to assume that a drug affects of all the variables and constants of the learning equation only the variable t and the constant T.

There is no reason for assuming that a drug could affect the constant L, the individual learning ability, which is (or, which I regard as) an inherited physiological (functional) property of the "conductive" (nervous) tissue of that particular individual, called by me the "susceptibility" of his nervous tissue,[5] especially of his so-called higher centers.

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[3] This JOURNAL, October 1922, 580. See there other bibliographical references.

[4] *Psychology of the Other-One*, 98–116.

[5] *Op. cit.*, 130. Do not confuse "susceptibility" with "conductivity"! They are constants entering into the individual endowment which are entirely independent of each other. Either of these constants may be low while the other is high.

No influence of the drug is imaginable on the constant p, since this refers only to the individual's past, both his ancestral past (heredity) and his individual past (experience). No influence of the drug is imaginable, either, on the constant a, the practice value of the task.

It has already been mentioned that no attempt has been made to determine the constants p and a with any extraordinary accuracy relative to each other. The reason appears in the preceding paragraph and may also be stated thus: actual computation shows that, if "p" is taken a little smaller, this can *virtually* be compensated for by taking "a" a little larger, provided we remain within a limited range of n, as we naturally do, since we should not desire to repeat the same task thousands of times. By "virtually" we mean that the shape of the curve *within these limits of n* would vary very slightly,—not enough to trouble our interpretation of the drug effects. While therefore a was not determined with any great accuracy, the 4 values of a in the experiments A, B, C and D nevertheless agree fairly with a certain conclusion that seems possible *a priori*. By this statement is meant that this conclusion is possible *from the definitions* of the constants a and T. Is it not plausible that, when the practice tasks are made up of quantities of rather homogeneous unit materials, such as average words or syllables, the practice values are simply proportional to these quantities? If that is so, one may also say that the T's should turn out proportional to those quantities and then also proportional to the a's.

This indeed is nearly so, in the experiments B, C and D. The exercises here were ordinary commercial letters. The longer the letters, the longer T, and the greater then the value of a. The T's appear as 26 in B, 27 in D and 34 in C; and the a's appear as 1.2 in B, 1.3 in D and 1.8 in C. That looks like proportionality, considering that the a's were not determined very accurately and that the material was not entirely homogeneous. The largest a belongs to the largest T and the smallest a to the smallest T.

On the contrary, no such proportionality is found in experiment A with its T equal to only 21 and its a of the relatively high value 1.3. The material used in A was not homogeneous with the material used in B, C and D. It did not consist of ordinary sentences with their random aggregations of words, but exclusively of words for which the inventor of the shorthand system had devised special methods of abbreviation. It appears that this fact increased the practice value of the task.

The significance of these remarks about the constant a consists in being an answer to the question whether the definition which I adopted for a in the learning equation was a proper definition. The choice of an equation and the definition of its variables and constants is never a matter arbitrary, speculative and metaphysical, but one that has to justify itself by being directly or indirectly serviceable for practical purposes.

This, however, is not the place to enter further into a discussion of the learning equation as such and of the problems of learning. Let us be satisfied here with the observation that the definition adopted for the parameter a has thus far justified itself.

The results of further experiments on working efficiency under drugs will be published in a following article.

THE PSYCHOANALYSTS' THEORY OF THE CONFLICT-NEUROSIS

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Of the many theories developed by the psycho-analysts, as an outgrowth of the psychotherapeutic methods of Freud, that of the conflict-neurosis is probably the best known and the most valuable to science. Having conflicts and complexes about things has come to be almost a fad among those who are in want of something exciting. But the conflict is also a serious fact of psychopathology, and has to be dealt with seriously by the worker in medical psychology and sociology. Neuroses and psychoses are particularly likely to develop in an over-complicated and hyper-stimulated society, where regimentation is in excess of rational social organization. Sociology and the social sciences generally have not yet discovered the method of smoothly taking care of the complicated adjustments in our modern world. Possibly, as some of the psychoanalysts believe, we shall never be able to learn this art. They hold that the breach between acquired culture and instinctive human nature has become so great that it never can be bridged. To a certain class of psychosocial theorists civilization has come to be synonymous with repression, or a disease. They believe that we are already too highly civilized, and would have us return to nature —or to the guidance of instincts—instead of moving farther in the direction of an artificially constructed and controlled civilization.

In the past century and a half—especially since the coming of the industrial revolution—there has been an abundance of social theorists to urge a cure for civilization. Rousseau, Fourier, Bakunin, and their kind, merely voiced a social philosophy for which Freud and his disciples have offered a psychological justification. I believe that the fundamental character of this conflict between instinct and culture has been much exaggerated. I have attempted to show elsewhere that the psychologists have overstressed the importance of instinct as a factor of control in our lives.[1] In this and in two other articles on the concepts of psychoanalysis[2] I have sought to offer some proof that our conflicts are not alone between instincts and culture, but are also in large part—perhaps primarily—between contending cultures or acquired attitudes and interests. Perhaps, after all, we have more need to worry about the control of habit-formation under the influence of environmental pressures or education in the large than about instinct. The neuroses, coming as a result of conflict, are realities, and they must be dealt with; but possibly they are in the last analysis as much or more a problem for the sociologist than for the psychologist and the geneticist. It may be possible to remove a very large part of the conflict-neuroses by controlling the environmental pressures which build our human natures, and instinct may prove to be a flexible and malleable instrument in the service of civilization instead of an obstruction in its way.

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[1] The Misuse of Instinct in the Social Sciences, *Psych. Rev.*, March, 1921.

[2] Instinct and the Psychoanalysts, *Journal of Abnormal Psychology and Social Psychology*, Jan.-Mar., 1923; A Criticism of the Psychoanalysts' Theory of the Libido, *Monist*, April, 1923.

The main contention of the conflict-neurosis theory is, to put it briefly in its most orthodox form, that "the neurotic symptoms are substitutions for sexual satisfaction."[3] Freud further states his theory of the causation of the neuroses as follows: "First, the most generalized factor, self-denial, then the fixation of the libido, by which it is forced into certain directions, and thirdly, the tendency to conflict in the development of the ego, which has rejected libidinous impulses of this kind."[4] It is at this point, according to Freud, that substitute activities or emotions are taken on, in compensation for the denied sex satisfactions. Sometimes these substitutions are in the nature of sublimations; but "the measure of unsatisfied libido which the average human being can stand is limited. The plasticity and freedom of movement of libido is by no means retained to the same extent by all individuals; sublimation can, moreover, never account for more than a certain small fraction of the libido; and finally, most people possess the capacity for sublimation only to a very slight degree."[5] Thus, where people are not able to find normal substitute satisfactions or to sublimate the "libido" they "become neurotically ill . . . ill with 'denial' . . . and . . their symptoms are the substitutes for the denied gratification."[6] These pathological substitutions which constitute the neuroses are too numerous to be detailed in a brief article.

It will be observed from the foregoing that Freud identifies the "libido" with sexual impulse.[7] He does not, however, deny that theoretically other impulses may play a part in the causation of psychoneuroses, but he maintains that he has not found any such cases:[8] "the essence of these maladies lies in disturbances of the sexual processes, in those processes in the organism which determine the formation and utilization of the sexual libido."[9] The conflict which leads to the neurotic satisfaction is never between sex-impulses or between ego-impulses as such. "The pathological struggle is waged between ego-impulses and sexual impulses."[10] This passage must be interpreted to mean that the conflict-neurosis arises out of the struggle of the sex-impulses for satisfaction against all other controls, which are here termed the ego-impulses; because Freud, having been educated in the strict hedonistic metaphysics and the subjective psychology of the Viennese school, does not recognize social or herd-impulses. He furthermore makes an artificial distinction between the sex-impulses and the ego-impulses on the ground that the former often seek pleasure at the expense of the organism's welfare and safety.[11] The psychologist with a social point of view would tend rather to group the sex-impulses as a subdivision of the ego-impulses, and to set both over against a class of social or ethical impulses. These last, of course, would be considered mainly as acquired, at least in their functioning forms; but apparently Freud does not hold that even the sex-impulses are wholly inherited.[12]

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[3] S. Freud, *General Introduction to Psychoanalysis*, 266. See also 260, 311, 373.

[4] *Op. cit.*, 305. [5] *Ibid.*, 300. [6] *Ibid.*, 299.

[7] See also *op. cit.*, 357–8.

[8] Freud, *Selected Papers on Hysteria and Other Psychoneuroses* (tr. A. A. Brill), 126, 141.

[9] *Ibid.*, 192 f.

[10] Freud, *General Introduction to Psychoanalysis*, 303. See also 304, 358.

Freud has an interesting justification of his theory of the limitation of the conflict-neurosis to the struggle of the sex-impulses for satisfaction against the censorship of the ego-organization. "It is worth noting that sexual instincts and instincts of self-preservation do not behave similarly when they are confronted with the necessities of actuality. It is easier to educate the instincts of self-preservation and everything that is connected with them; they speedily learn to adapt themselves to necessity and to arrange their development in accordance with the mandates of fact. That is easy to understand, for they cannot procure the objects they require in any other way; without these objects the individual must perish. The sex-instincts are more difficult to educate, because at the outset they do not suffer from the need of an object. As they are related almost parasitically to other functions of the body and gratify themselves auto-erotically by way of their own body, they are at first withdrawn from the educational influence of real necessity."[13] It is apparently true that the conflict of sex-impulses with other interests does cause a very large proportion of the psychoneuroses, but this result is scarcely due to the cause which Freud assigns. The main reason is that the sex-impulses are more firmly rooted in actual organic instincts, while the ego or self-preservative tendencies are in much larger proportion acquired. The objective unity of action here is imputed rather than real, and the subjective unity of concept is abstract rather than concrete and motor.[14] Consequently there is not such a strong tendency for the ideas connected with most of the other ego-impulses and desires to go over into action, and, being in the main acquired, they have more associations with other action-patterns into which they may be transferred. Doubtless, also, the fact that sex-desire and impulse can end subjectively much more easily than the ego-tendencies which are dependent upon (and usually derived from) the organization of the external psycho-social environment, which has in large measure given them form and content, has considerable influence here. The greater educability of the so-called ego-tendencies is due to the fact that they are primarily, at least in their "higher" and less vegetative forms, the product of environmental pressures and adaptation or education. Thus the distinction which Freud draws between these two sets of tendencies or impulses is seen to rest on the difference in degree of innateness which they possess.

Freud's is the orthodox view of the conflict-neurosis, and is concurred in by a very large portion of the psychoanalysts.[15] White's theory of the conflict-neurosis is somewhat different. For him the conflict arises between two "libidos," instead of between the libido (sex-impulse or desire) and the self-preservation values (interest), as maintained by Freud. It would be difficult to imagine a more metaphysical and obscurantist statement of the theory of conflict than that made by White in the following passage: "The libido has only two pathways open for it and these pathways lead in diametrically opposite directions. One leads forward and upward, it is the pathway of constructive tendencies; the other leads backward and downward, it is the pathway of destructive tendencies. The former leads to fulfilment, life, immortality; the latter to dissatisfaction, failure, death. . . . The goal of immortality leads by the way of development, progress, evolution and is expressed by the conservation of personal life by means of health and the prolongation of personal life in children and of personal influence as expressed in a material way through the passing on of our personal qualities by way of the germ plasm or our spiritual tendencies by way of our works in our influence upon those about us, upon our own times, and upon the future by the record of our achievements that survive our individual existence. The death goal is by the way of the path of regression, the retracing of the path by which we have come, and leads to failure in the conservation of our individual existence, illness, invalidism (physical and mental) and to failure to hand on our influence either by way of the germ plasm or spiritually as a result of our works. These are the two pathways represented on the one hand by the drag back of our unconscious instinctive tendencies and on the other by those tendencies sublimated and applied to constructive conscious ends, the ambivalent goals of which are death and life motived respectively by fear and desire. This is the conflict, the path of opposites, along which somewhere, specific tendencies clash and cause that splitting of the psyche, so clearly seen in psychotics, which divides the energies of the individual and leaves him torn and broken upon the rocks of indecision, with his consciousness raised to an acuteness which is painful (fear, anxiety) in its terrific efforts to effect an adjustment."[16]

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[11] *Ibid.*, 309. [12] *Ibid.*, 314 f. [13] *Ibid.*, 308.

[14] See my article, Instinct and the Psychoanalysts, above referred to.

[15] See, *e.g.*, E. Jones, *Treatment of the Neuroses*, 167; *The Psychoanalytic Review*, 8, 361–374; also Brill, Coriot, Jelliffe, etc., as cited in this article.

As long as the "libido" flows uninterruptedly into successful and satisfying action in the real world there is no conflict.[17] But if for any reason expression is blocked through the normal activity-channels, the "libido" flows backward within the individual himself, through the deepened channels of individual or racial past experiences, "reanimating old experiences." This is introversion of the "libido," and "disorder of the mind is the result."[18] This avenue of escape of the "libido" by means of introversion or regression may prove to be quite harmful. White regards it as the most characteristic element in dementia praecox.[19] "Perhaps the libido can get out of older levels but in doing so it offends the ideals which the individual has acquired in his upward strivings and so the result is illness."[20] This might properly be called the secondary conflict, the primary conflict being between two tendencies to action, one of which is inhibited on the plane of everyday reality and driven back upon older and more primitive avenues of expression. If these primitive modes of expression are in turn blocked, this time by ideals or values which censor the primitive impulses, a secondary conflict arises, and apparently, according to White, it is at this point that the true psycho-neurosis arises. The "libido" now attempts a third avenue of expression, that of subterfuge or symbolization or phantasy formation.[21] "The object of phantasy may be considered as twofold. The first, and less important object, I think, is the object of finding an outlet for the libido at an older level when faced with a situation to which adjustment is difficult. It is a way of letting off steam or, as the phrase goes, of emotional catharsis."[22] The dream may serve this function in some minor degree.[23] But "the more important function of phantasy is coupled with its . . . function of portraying conflict, that is, its picturing of the two opposed tendencies that are battling for supremacy . . . The object of the symbolization of the conflict is, therefore, in general, to bring the whole matter into consciousness but in particular to bring that particular element into consciousness which is interfering with the progress of the individual."[24]

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[16] *Mechanisms of Character Formation*, 271f. See also his *Outlines of Psychiatry*, 9f.

[17] *Mechanisms of Character Formation*, 305.

[18] *Ibid.*, 285.

[19] *Ibid.*, 304. See also White, Some Considerations Bearing on the Diagnosis and Treatment of Dementia Praecox, *Psychoanalytic Review*, 8, 417–22.

[20] *Ibid.*, 305. [21] *Ibid.*, 305. [22] *Ibid.*, 305f. [23] *Ibid.*, 306.

Thus it would appear that White, like Freud, regards the psychoneurosis as a substitute expression and as a symptom. But he does not maintain that it is dominated exclusively by the sex-impulses. Apparently it can arise from any type of repressed striving. We saw that Freud's ego or self-preservative tendencies, which produced the psychoneurosis by coming into conflict with the sex-impulses, in large measure really represent the censorship of the psychosocial environment, under the direction of which the former impulses are formed. White practically acknowledges this when he speaks of the conflict (which we have classed as secondary) as arising between regressive expression of the "libido" and "the ideals which the individual has acquired in his upward strivings."[25] These ideals could come only from his contact with the psychosocial environment, or the environment of institutions and social values. It is obvious that White is not so much interested in the structure of the psychoneurosis or phantasy as is Freud. He views it primarily from the standpoint of the diagnostician who is searching for a symptom which will enable him to discover the conflict and to remove the cause.

Jung has made considerable emendations of the theory of conflict-neurosis as held by Freud and his followers. He does not agree that the neurosis arises from some infantile sex-traumatism which recurs in the conduct of the individual and is based on the subconscious dispositions. All children, practically, undergo sex-shock, but only a few develop neurosis. Many of the stories of shock are invented by the patients,[26] probably as wishes rather than as facts. Jung does agree, however, that the neurosis is accompanied by an infantile fixation. "The aetiological secret of the neurosis, therefore, does not consist in the mere *existence* of infantile phantasies, but lies in the so-called *fixation*."[27] Again; "The psychological determination of a neurosis is only partly due to an early infantile predisposition; it is due to a certain actual cause as well."[28] The infantile phantasies merely determine the form and further development of the neurosis.[29] The neurosis is based on conflict and this conflict is primarily in the present.[30] "Whenever the libido, in the process of adaptation, meets an obstacle, an accumulation takes place which normally gives rise to an increased effort to overcome the obstacle. But if the obstacle seems to be insurmountable, and the individual renounces the overcoming of it, the stored-up libido makes a regression. In place of being employed in the increased effort, the libido now gives up the present task and returns to a former and more primitive way of adaptation."[31] The neurosis is the consequence of the overflow—is the overflow—of the stored-up energy or libido. "The psychological trouble in neurosis, and neurosis itself, can be considered as an act of adaptation that has failed."[32] "Even when we find perverted sexual phantasies of which we can prove the existence in childhood, we cannot consider them of aetiological significance. A neurosis . . . is not a primary phenomenon, based upon a perverted sexual disposition, but merely secondary, and a consequence of a failure to apply the stored-up libido in a suitable way."[33]

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[24] *Ibid.*, 306f.

[25] *Ibid.*, 305. For further discussion of this censorship of the herd, see 326–329.

[26] Jung, *Analytical Psychology*, 227.

[27] *Ibid.*, 228f.

[28] *Ibid.*, 230, 234.

[29] *Ibid.*, 233; *Psychology of the Unconscious*, 463.

[30] *Psychology of the Unconscious*, 192; *Analytical Psychology*, 227–235.

[31] *Analytical Psychology*, 231f.

[32] *Analytical Psychology*, 234; *Psychology of the Unconscious*, 463.

The neurosis stands, then, as a substitution in action or thinking for reality.[34] It is a form of illness arising from self-denial,[35] as Freud said. On this point Freud and Jung are fully agreed. It takes on the infantile form because the subconscious is full of stored experiences or memories which can be called upon to serve as channels for the overflow of substituted actions or thoughts when the hardness of reality blocks the way for the normal response of the conventional or expected kind.[36] The infantile experiences or memories are like a publisher's morgue, from which can be drawn almost any action-pattern to take care of the overflow of dammed-up energy. The same sort of substitution takes place in everday life. When we find some objective too difficult of attainment we often change our aim to something which we regard as more feasible. Ordinarily we do not consider this action pathological, for we select some other normal objective as a substitute; it is an exchange of a more difficult for a less difficult reality. It is only when the substituted act is not conventional, not a "reality," is in the nature of a phantasy and is shocking to other people, that we classify it as a psychoneurosis. There are also border-line cases, where we speak of the person making the substitution as "queer" rather than as strictly neurotic. Jung says: "He who has missed the happiness of woman's love falls back, as a substitute, upon some gushing friendship, upon masturbation, upon religiosity; should he be a neurotic he plunges still further back into the conditions of childhood which have never been quite forsaken . . . Every psychoanalysis carried out at all thoroughly shows this regression more or less plainly."[37] Thus the neurotic differs from the hypothetically normal person only in degree. He is an intensely disappointed person, while everyone is more or less disappointed. It is clear also that to both Jung and Freud the hall-mark of neurosis is the infantile fixation, although to Jung this fixation is not the cause of the neurosis. The neurosis usually occurs under great immediate stress, "at the moment when a new psychological adjustment, that is, a new adaptation, is demanded."[38] To the critic it may seem quite doubtful whether infantilism is a necessary correlate of neurosis.

Although Jung does not accept the Freudian view that neurosis is necessarily the result of sex-conflict and holds that the apparent sexual character of the neurosis is often assimilated to it along with its infantilism,[39] he does admit that the field of sex-conflict is in a high degree productive of the neurotic condition.[40] In this assertion and in the statement that the neurosis arises out of a present situation Jung, by implication, assigns great importance to the social environment in the production of the neurosis. In one connection the author seems to imply that conflict with the external environment does not tend to produce neurosis. "If it were an external difficulty, then, indeed, the libido would be violently dammed back, and would produce a flood of phantasies, which can best be designated as schemes, that is to say, plans as to how the obstacles would be overcome. They would be very concrete ideas of reality which seek to pave the way for solutions," but not substitute-phantasies aiming at the escape from reality by means of a neurosis which protects the sufferer from his environment.[41]

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[33] *Analytical Psychology*, 233f.

[34] *Psychology of the Unconscious*, 339, 432f., 460–5.

[35] *Ibid.*, 259 ff. [36] *Ibid.*, 421, 461–5.

[37] *Analytical Psychology*, 156.

[38] *Ibid.*, 229.

[39] *Analytical Psychology*, 227, 231, 277, etc.

[40] *Ibid.*, 367 ff.

[41] *Psychology of the Unconscious*, 196 f; *Analytical Psychology*, 297 f.

On the other hand, Jung specifically states the conflict in terms of the struggle of the inner impulses (largely the sexual, because this aspect of life is most repressed in our civilization[42]) and the pressures of the social environment. "Instinct thirsting for liberty thrusts itself up against the yielding barriers of morality: men are tempted, they desire and do not desire. And because they will not and cannot think out to its logical conclusion what it is they really desire, their erotic conflict is largely unconscious; whence comes neurosis."[43] He means to say here that repression from the social environment has driven the conflict into the subconscious, from which it emerges as a neurosis. He further adds that this conflict-neurosis is primarily concerned with the problem of repressed sex-expression. "Neurosis then is most intimately bound up with the problem of our times and represents an unsuccessful attempt of the individual to solve the general problem in his own person. Neurosis is a tearing in two of the *inner self*. For most men the reason of this cleavage is the fact that their conscious self desires to hold to its moral ideal, while the unconscious strives after the amoral ideal, steadfastly rejected by the conscious self."[44] Again he emphasizes the social nature of the conflict-neurosis: "We always find in a patient some conflict, which, at a particular point, is connected with the great problems of society; so that when the analysis has arrived at this point the apparently individual conflict is revealed as a universal conflict of the environment and the epoch. Neurosis is thus, strictly speaking, nothing but an individual attempt, however unsuccessful, at a solution of the general problem . . . The 'question' which troubles the patient is—whether you like it or not—the 'sexual' question, or more precisely, the problem of present-day *sexual morality*."[45]

It does not necessarily follow, however, that this conflict between our inner tendencies and the external social reality can have no issue except in neurosis. To be sure, the modern world is getting farther and farther away from the primitive world in which the two primal impulses—nutrition or self-preservation and reproduction or the race-preservative—find direct and uncensored satisfaction (at least, uncensored as far as public opinion is concerned). But along with the growth of opportunities for conflict there develops also an expansion of the "libido" or forms for energy expression. The "libido" (man's impulses) becomes more and more socialized. It develops from the primitive nutritive interest over to the relatively social sex-interest;[46] from this it expands, or is sublimated, into the religious interest,[47] which is at first very decidedly mingled with the sexual,[48] but finally comes to have largely a rational and refined emotional character;[49] and finally, at least in the minds of the more philosophic of our population, the "libido" (or impulses) is suffused largely with the spirit and data of science.[50]

Strictly speaking, perhaps, to be perfectly orthodox in our definitions, religion and science or philosophy might be described as neuroses, at least insofar as they contain infantile elements. Anyway, from the psychoanalytic standpoint, they are substitutions for primal satisfactions, but they are primarily substitute-interpretations of reality rather than substitutions for reality. However, it is difficult to distinguish between substitutions for reality and substitute-interpretations of reality. Possibly no definite distinction can be made in this connection. The expansion or socialization of the "libido" is in the nature of prophylaxis. When the conflict has already arisen and a neurosis is formed or in process of formation cure may be effected by removal of the factor damming up the "libido."[51] Applied on a wide scale, this method might range in content all the way from a widespread education of all in the principles and facts of science to a return to savagery or to a higher animal level of existence, where there would be no artificial civilization to censor the native instincts of man. But this remedy is probably worse than the disease.[52] Jung warns against the attempt to "help the neurotic patient by freeing him from the demand made by civilization; we can only help him by inducing him to take an active part in the strenuous task of carrying on the development of civilization. The suffering which he undergoes in performing this duty takes the place of his neurosis."[53] This strenuous method can usually be avoided by sublimation, as an alternative to letting down the bars.[54] Sublimation is doing for the individual what has been or is being done for the race by the socialization and objectification of the "libido" or impulses through religion, art, philosophy and science. In a sense the neurosis is itself an attempt at self-cure,[55] a struggle to find a less burdensome solution to conflict, one which will work. Where the attempt is successful it is a cure; where it fails we have neurosis.

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[42] *Psychology of the Unconscious*, 460–2; *Analytical Psychology*, 374 f.

[43] *Analytical Psychology*, 369 f.

[44] *Ibid.*, 370.

[45] *Ibid.*, 374. Compare *Psychology of the Unconscious*, 258 ff.

[46] *Psychology of the Unconscious*, 146–9, 160 f.

[47] *Ibid.*, 258, 528. See also *Analytical Psychology*, 224.

[48] *Ibid.*, 256. [49] *Ibid.*, 257 ff. [50] *Ibid.*, 262.

MacCurdy[56] accepts three fundamental instincts (ego, sex and herd) and maintains that there are four types of conflicts: those between ego and sex-tendencies, between the ego and the herd-tendencies, between the sex and the herd-tendencies, and between one type of sex-ambition and another.[57] Many writers on psychoanalysis do not allow for this last type of conflict. Freud includes it only when one type of sex "libido" has been assimilated to the ego-tendencies, as in the case of narcissism.[58] Serious conflicts occur only when the opposing tendencies are pretty evenly matched,[59] and all true conflicts must be between instincts, one of which must be repressed or transformed in its expression before the conflict can be removed.[60] The severest conflicts are between the sex and herd groups of instincts,[61] and the most marked psychoneuroses arise out of this type of conflict. "The next most important conflict is between the ego and herd groups,"[62] but the psychoneuroses arising hence are not particularly striking. The reason is that the individual adjusts to the demands of society because of the advantage he reaps from doing so. "When these (social or herd) conventions cramp individual ambition, the ego accepts the restriction because it gets a *quid pro quo*. This compensation takes the form of protection from danger, a share in material prosperity not achievable by the individual alone and, above all, a feeling of comfort derived from unity with the group that is a direct emotional expression of the herd instinct."[63] Furthermore, the herd does not repress all ego-tendencies. Consequently pathological expressions or psychoneuroses resulting from this type of conflict arise in only two possible situations. "One is where there is a constitutional predominance of ego tendencies such as seem to exist in epilepsy; the other is under exceptional circumstances when the ego is denied normal expression. These circumstances would be episodic and therefore produce only episodic abnormalities enduring only for the period of the emergency."[64] There does not seem to be much permanent conflict between the ego and sex-instincts, "for there is an alternation of attention to one type of stimulation or another, at least in so far as the pleasure aspects of the sex instincts may conflict with other pleasures of the ego."[65] He further adds: "The probability is that important pathogenic conflicts would not result from the interaction of sex and ego instincts alone."[66]

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[51] *Analytical Psychology*, 232.

[52] *Ibid.*, 221. [53] *Ibid.*, 224 f. [54] *Ibid.*, 375 f. [55] *Ibid.*, 234,374.

[56] J. T. MacCurdy, Synthetic View of Ego, Herd and Sex Instincts, *Journal of Abnormal Psychology and Social Psychology*, 16, 1921, 249–268.

[57] *Ibid.*, 258.

[58] Freud, *General Introduction to Psychoanalysis*.

[59] MacCurdy, *op. cit.*, 259.

[60] *Ibid.*, 251. [61] *Ibid.*, 260. [62] *Ibid.* [63] *Ibid.*, 253.

[64] *Ibid.*, 254.

The explanations offered by MacCurdy for the stronger conflicts between the herd and sex-instincts and the more pronounced psychoneuroses arising from these conflicts are interesting, but they omit the essential factor. Three explanatory factors are enumerated. First, "an important cause for continuous conflict between the sex and herd instincts is that sex is not a periodic function in the races of man we know about . . . A second and all-important point is that the channels for sex stimulation are, many of them, identical with the channels of social contact."[67] This is true of organic structure as well as of social intercourse. "The community of interest, the moral and physical propinquity which draw people into groups may draw two individuals into sex relationship. It is therefore difficult for one to receive a social stimulus without at the same time being subject to a sex stimulus. The latter falls, almost invariably, under social tabu, hence a constant conflict between the synchronously stimulated herd and sex instincts is inevitable."[68] Thirdly, the relatively freely expressed auto-erotic tendencies of childhood begin to be objectivated as the child grows more mature, and they must take cognizance of the social environment which is largely antagonistic to them. Hence conflict arises and phantasies and psychoneuroses result.[69] The strongest social censorship is directed against those sex-practices which inhibit marriage and propagation, such as inversion, perversion and incest. In fact, the writer thinks that there is an instinctive opposition to incest, because of the harmfulness of inbreeding.[70] On the other hand, adultery, prostitution and concubinage are lightly condemned because they do not conflict with the "fundamental urge for propogation."[71]

The thing which is constantly overlooked in this account of the superior strength of the sex-psychoneuroses, although it is literally calling for recognition from the context, is the environmental factor. As pointed out above, the sex-impulses contain a much larger instinctive element than the ego and herd-impulses, which are largely acquired. The herd-impulses are almost wholly acquired. Consequently, the sex-tendencies are too deeply rooted to give way easily under environmental pressures, and if repressed in one direction they break through in some other direction. If this variant outlet be through an activity which is not functional in a socially useful way, such as tics, hysterical paralysis, etc., it may be characterized as a neurosis. If the alternative expression is through unconventional or socially disapproved or "unreal" mental processes or ideas, it is called a psychosis. The ego-tendencies being more commonly instinctive than the herd-tendencies in the individual, but less so than the sex-tendencies, yield with a medium resistance to the demands of the social environment and produce weaker neuroses and psychoses. But since the herd-tendencies in the individual are mainly acquired from the pressures of the psychosocial environment, and are but little rooted instinctively, they find little occasion for conflict with the social environment and rarely produce neuroses and psychoses. In fact, these tendencies are often so weak that the constant pressure of large numbers of people upon our consciousness, making unbearable demands upon our nervous systems, produces psychoneurosis itself, this being one of the causes of nervous breakdown. The writer himself cites from Rivers' observations among the Melanesians a type of insanity which we might call herd-fear,[72] and this is not unknown among civilized peoples.

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[65] *Ibid.*, 252. [66] *Ibid.* [67] *Ibid.*, 256.
[68] *Ibid.*, 256 f. [69] *Ibid.*, 257 f. [70] *Ibid.*, 255. [71] *Ibid.*, 254 f.
[72] *Ibid.*, 259.

The fact is that MacCurdy, like all the psychoanalysts and psychiatrists with a psychoanalytic bent, is confused as to the basis of the conflicts. They generally assume that the conflict is between the so-called instinctive or subjective tendencies and impulses, while, as a matter of fact, it is only secondarily so. This inner conflict is between two sets of subjective neuropsychic processes which can be brought equally well under the control of consciousness or thought and resolved into some sort of psychic unity through compromise or dominance of neural sets. There is no need for the generation of phantasies or a substitute-method of resolving the conflict, because the conflict is "thought" out of existence. In the process of thinking the conflict disappears. There is no distinction between reality and phantasm as far as the subjective neuropsychic processes are concerned. This distinction refers to the environmentally determined facts of the character and availability and immanence of the stimuli and the responses which are connected through the central processes of the stimulus-response arc, where the thinking occurs. Consequently there can be no psychoneuroses arising out of inner conflict of mental processes. Any conflict here must soon be resolved into a unitary attitude or concept. We see things as wholes, even if we are compelled to select our stimuli and control our responses in order to do so.

The real and primary, the stubborn conflicts are between subjective mental or neuropsychic processes and the outside world or environment. The mind cannot control the outside world and order it at will as it can the mental processes. It cannot whip environmental factors into an objective unity in conformity to inner psychic organization. Consequently, when the conflict between external reality and inner mental processes or complexes is sufficiently marked, the mind, in its struggle for inner unity, resorts to the device of selecting external stimuli and the perversion of perception and ideas. This is one of the most common phenomena of psychic life, but it ordinarily takes place on a limited scale, because the conflict between the inner psychic organization and the external reality is not usually especially marked. All of us see things from our own subjective angles. But when we grossly misinterpret external phenomena to fit our subjective needs or desires we regard it as abnormal. It is then that the fact of the personal equation in interpretation reaches the dignity of phantasy, and is classed as neurosis or psychosis. If we act out our phantasy, directly or symbolically, we suffer from a neurosis. Such abnormal mental processes occur because, on the one hand, the environment will not yield to the powers of mental control which the subject possesses and, on the other hand, the mental organization itself cannot immediately adapt itself to the pressures of the external environment.

Freud almost hit upon this principle in his theory of the conflict between the principle of pleasure and the principle of fact,[73] which is referred to by other writers as the conflict of the pleasure-motive and the reality-motive.[74] He was prevented from making the application because of his lack of appreciation of the functional significance of the environment in the conflict situation. Also, possibly, the regnant metaphysical notions of reality, which tend to make it subjective instead of objective, neglecting environmental analysis, unconsciously influenced his thought. Anyway, he missed the scientific explanation, based on objective analysis, and contented himself with a metaphysical and merely verbal theory instead. In this the other psychoanalysts, following the metaphysical and mystical tradition, have persevered. Thus it happens that MacCurdy, like the others, confuses conflict between the internal psychic processes and the objective environment with conflict between the sex and ego, inner or psychic tendencies on the one hand and the inner, psychic herd-regarding tendencies on the other. They do not distinguish the herd-complex (miscalled herd-instinct) from the herd itself. The former is the acquired product of the impingement of the latter upon the consciousness of the individual. The sex, ego and herd-complexes all belong to the same order of phenomena, and may severally be in conflict with the herd or social environment. This is the significant type of conflict, while the conflict of the first two complexes with the last is relatively unimportant and easily resolved. This confusion of the psychoanalysts is all the more remarkable when it is remembered that their data constantly call attention to the persistence of the environmental pressures. Being wedded to their uncritical use of the instinct-theory or phantasy, they are unable properly to evaluate objective reality in the form of the environment.

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[73] *General Introduction to Psychoanalysis*, 309.

[74] White, *Mechanisms of Character Formation*, 56, 71; Lay, *Man's Unconscious Conflict*, 85, 93; *Man's Unconscious Passion*, 18; etc.

MacCurdy recognizes three immediate factors in the production of the psychoneuroses. The normal balance of the mind is maintained through sublimations. But "when the energy supply is low (owing to such organic factors as illness or strain) sublimations are weakened and instincts of the sex or ego group express themselves more directly. This is the principle of regression. Another situation is that of environmental stress. When circumstances make the maintenance of sublimations difficult or impossible the same mechanism comes into play . . . Any environmental event, then, which weakens such sublimations can lead to regression."[75] His explanation of how the psychoneurosis so generally takes on a sex-character, although it may not be sexual in its ultimate origin, is interesting. "Such events may be business or political disappointments which may have little to do with sex. When these conscious activities fail to give satisfaction the process of regression leads to an attempt at unconscious satisfaction. Since the unconscious is constituted largely of egoistic sex cravings the immediate determinants of the ensuing symptoms may be sexual, without the general cause of the disease having anything to do with sex.[76] This conclusion of course assumes the truth of the second term in the syllogism, namely that the subconscious content is repressed sexuality, which is not necessarily true. May not a better explanation be found in the fact that the sex-processes, being largely instinctively based, have deep neuropsychic channels, which fact facilitates the association or overflow of all sorts of impulses into these processes resulting in their mixture or substitution? He continues: "A third pathogenic factor (in the production of psychoneuroses) is the direct stimulation from without of an unconscious tendency. For instance, if the rival of the unconscious Oedipus drama dies the way is left open for autistic indulgence of this primary ambition. This may lead to an inflation of unconscious sex cravings."[77]

Jelliffe[78] apparently adopts a somewhat different basis for conflict. Although, like Freud, he recognizes two great instincts (reproduction and self-preservation) and two only,[79] all others being merely "combinations, amplifications or partial elaborations of these,"[80] he recognizes a great many separate "libidos,"[81] which are individualistic or self-seeking in early childhood[82] and are united by a process of discipline and fusion under the direction of the social environment[83] after the age of five.[84] Curiously enough, he does not suppose these several "libidos" to be founded on corresponding instincts, bearing the same names, although he clearly defines his "libidos" as energy striving for expression towards certain ends.[85] In this he is, of course, correct; for each "libido" represents for him a functioning group of organic processes, such as respiratory, nutritive, pelvic, sensory, etc.,[86] which are grouped together not so much because they are structural unities in action as because they are recognized by the human mind as contributing in their several ways to the same sorts of environmental adjustment. That is, their unity is more external or objective than internal and subjective. However, the two so-called instincts which he does recognize[87] are defined in this same loose abstract and environmental manner.

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[75] *Op. cit.*, 260 f. [76] *Ibid.*, 261. [77] *Ibid.*

[78] S. E. Jelliffe, *The Technique of Psychoanalysis.*

[79] *Ibid.*, 66. [80] *Ibid.*, 67. [81] *Ibid.*, 40–48, 62 f., 132.

[82] *Ibid.*, 41. [83] *Ibid.*, 42, 44, 53, 56. [84] *Ibid.*, 67. [85] *Ibid.*, 40, 44, 66.

It is difficult to determine just where the conflict occurs according to Jelliffe. Apparently it has abundant opportunity to arise during the first five years among or between the various individual or partial "libidos," and probably it does start here. But in the course of time the social environment, at first in the form of the family,[88] and later in that of society at large, represses these separate ego or "libido" tendencies and welds them into a sort of unity for the sake of self-preservation of the whole organism.[89] Elsewhere, however, his statement of the opposing tendencies in life takes on a much more mystical and metaphysical form. He speaks of "concrete immortality" as "the goal of the libido striving, which is reached either directly and foremostly through the birth of children or secondarily through building up of social structures and all that goes to make life worth living."[90] Evidently he identifies the general "libido," as distinguished from the partial "libidos" mentioned above, with the successful prosecution of the two general instinctive tendencies, reproduction and self-preservation. That being the case, serious conflict cannot arise between these two instincts, at least after the age of five. We are not left without a statement of the conflict-process, however. In equally metaphysical language he informs us that "the direct opposite of personal immortality is death," and that fear is its symbol.[91] Are we to conclude, therefore, that conflict arises between these two concepts or between the emotions of hope and fear which attach to them? Other passages which have been cited from the text would seem to imply that the conflict is between the two general instinctive tendencies, which are basic to the general "libido" and the various partial "libidos" on the one hand, and the restraining and moulding environment on the other. In this way the conflict has really a double origin. It arises partly from the internecine warfare of partial "libido" trends before they are integrated by the environment into a unified personality, and partly from the struggle of this resulting personality with the environment. In the latter case the conflict is between the "libido" and the environment, and in the former it may be either between the separate "libidos" or between any one or all of the partial "libidos" and the environment. If there is wanting a concrete and concise statement of this view by the author it is probably due to the fact, previously adverted to, that the psychoanalysts, absorbed in the popular instinct-interpretation, have not yet had the environmental factor definitely and adequately called to their attention.

The rest of Jelliffe's theory of conflict-neurosis is quite according to type. When the "libido" fails to secure adequate expression in action it turns to thought instead, and when this takes on the form of phantasy of a marked character with attendant unconventional or uneconomic and unreal activities, as viewed from the standpoint of an efficient life adjustment, we have the psychoneurosis.[92] Since the social environment is apparently the great censor, antisocial tendencies are especially liable to find substitute expression in phantasy and psychoneurosis.[93]

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[86] *Ibid.*, 40. [87] *Ibid.*, 66.

[88] *Ibid.*, 53, 56. [89] *Ibid.*, 47 f. [90] *Ibid.*, 67.

[91] *Ibid.*, 67 ff. *Cf.* White, *Mechanisms of Character Formation*, 271 ff.

[92] *Ibid.*, 41, 45 f., 54, 64 f., 119, 137, 139, 142. [93] *Ibid.*, 45.

Brill[94] takes the conventional point of view regarding the origin of the conflict-neurosis which we have found in the discussions earlier set forth, and to these he adds some interesting applications or extensions of the theory. He says that the individual suffers from strangulated emotions; "certain feelings and ideas which we would like to give vent to, but cannot. . . . They are finally pushed into the unconscious."[95] Or, on the other hand, "an individual experiences a profound emotional shock and cannot give it emotional expression; it remains in a repressed condition."[96] These repressed or strangulated emotions, associated with painful experiences in many cases, form a complex which is brought to the surface only by some association. This conflict, from which the neurosis arises, may have its origin in any situation which represses our instinctive tendencies or emotions, but it centers primarily around the inhibition of the sex-instincts and emotions.[97] Brill even goes as far as to sanction "Freud's dictum that no neurosis is possible in a normal sex life,"[98] and he maintains that this statement is true also of the more radical functional disturbance known as psychosis.[99]

The neurosis, according to Brill, is to be recognized by the neurotic symptom.[100] This is some substitute-expression which is symbolic, in the mind of the subject, of the thing denied expression by the psychic censor.[101] These substitute-symptoms may be drawn from any source, but they are taken particularly from the vast treasure-house of forgotten childhood experiences.[102] These, although repressed and forgotten, are imperishable and may be brought out at any time to do service in this connection through the principle of association.[103] The fact that the childhood experience is so often used as the basis of the symbolic symptom at first led the psychoanalysts to believe that the neurosis was the product of some infantile or early shock. But the tendency among the psychoanalysts at the present time is to attribute the conflict primarily to sex-repression. Civilization has removed the hunger conflict, the other potential source of the neurosis, so that neurosis does not, at least with any frequency, any longer arise from the maloperation of the instinct of nutrition.[104] But, on the other hand, civilization has only served to increase the improper functioning of the sex-instinct by imposing still further restrictions upon its expressions.[105] Hence the frequency of neurosis arising from this source. Even where neurosis arises from some non-sexual cause, such as the loss of a child or failure in some undertaking, apparently it could have been prevented if sex-love had not been lacking as a salve or substitute for the other loss.[106] Thus, perhaps, we may infer that sex-love is almost, if not quite, a universal panacea for ills of the spirit. This view is in harmony with the statement earlier cited to the effect that there can be no neurosis or psychosis with a normal (or satisfied) sex life.[107]

Many of the conflicts giving rise to neurosis appear in connection with unfortunate love-affairs. Others arise from the lack of opportunity of the sex-powers to function. This is especially true of women, who are more neurotic than men. "In our Anglo-Saxon communities, they (our sex impulses) have not even the aesthetic and social outlet, because of the too great separation of the sexes. The result is that, owing to the matrimonial difficulties and the two-children system, the women especially who have not been able to express themselves adequately for centuries and whose lot is growing harder and harder in our civilization, suffer from a marked need of love. If you find a woman depressed and out of sorts with herself and weeping, who cannot tell you what is ailing her, you may safely conclude that she is craving for love; give her the necessary love outlet through happy marriage and children, and she will have no more crying spells."[108] Much of this conflict and its consequent repression may be symbolized as a conflict between the past and the present, on the one hand, and between the infantile nature and the adult nature, on the other hand. In the first instance our natures, instincts, were selected to fit a natural or pre-civilized world and these impulses persist into the civilized world and come into conflict with our conventions and institutions.[109] "Centuries of civilization have left their mark upon us, and we must now live accordingly . . . There is therefore a constant struggle; no one likes to submit to the repression that the inhibitory aspects of civilization demand; no one finds it easy to repress the primitive impulses."[110] Thus the individual learns the meaning of "no" from infancy. "His life . . . is one long struggle, one bitter revolt."[111] In much the same way the primitive desires of childhood, which may be tolerated before the age of accountability, will be repressed by the social censor when the individual reaches or approaches adulthood.[112] It is significant, Brill holds, that the child's dreams become more difficult to analyze after the age of five. There is no difficulty before that age.[113] It is then that the censorship begins to operate by virtue of the child's growing perception of the conventional. Also "it is noteworthy that it [the child] develops also at this time a sense of humor."[114]

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[94] A. A. Brill, *Fundamental Conceptions of Psychoanalysis*, 1921.

[95] *Ibid.*, 13. [96] *Ibid.*, 14. [97] *Ibid.*, 22. [98] *Ibid.*, 29. [99] *Ibid.*

[100] *Ibid.*, ch. 2. [101] *Ibid.*, 38–40, 44, 63. [102] *Ibid.*, 25.

[103] *Ibid.*, 20, 181. [104] *Ibid.*, 32. [105] *Ibid.*, 30, 32, 291.

[106] *Ibid.*, 31. [107] *Ibid.*, 185 ff.

The censorship over repressed impulses and ideas is exercised by the conscious.[115] "The mind is always protecting us from pain by pushing whatever is disagreeable and unpleasant into the unconscious."[116] This fact affects the conventional or popular theory of memory: "thus what we generally look upon as forgetting is not that at all; certain things are merely pushed into the unconsciousness, because of something unpleasant associated with them; we are not aware of them consciously and we naturally presume we have forgotten them."[117] That which sets the standards of the censorship is one's understanding of what is conventional and of what is forbidden.[118] "The psychic censor, as the term is used in our work, is nothing but the inhibitions imposed upon the individual by society, and once established, operates all the time, whether we are awake or asleep."[119] The censor can be avoided only by subterfuge, and of this nature is the dream.[120] But it is not alone in dreams that we find this attempt at avoidance by subterfuge. Our opinions, our every day actions, attitudes, alliances and allegiances indicate it. Thus day-dreaming and the artificial dream show the same tendency.[121] Any form of make-believe or wish betrays the same type of stratagem. Lying and hypocrisy represent veiled wishes and strivings against the censor.[122] Fairy tales, [123] which are a form of lying, and art,[124] which may be either subterfuge or sublimation or both, also belong to the same general category. Modern art is decidedly auto-erotic[125] and the productions of the futurists and other radical artists resemble closely those of insane people.[126] The expression of such art—painting, poetry, or otherwise—escapes the social censor by taking refuge in a theory of individualism or self-gratification (art for art's sake) which denies the validity of the social censor altogether. It is nihilism.[127]

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[108] *Ibid.*, 29 f. [109] *Ibid.*, 161. [110] *Ibid.*, 161 f. [111] *Ibid.*, 162.
[112] *Ibid.*, 161 ff., 207 ff. [113] *Ibid.*, 164.
[114] *Ibid.* [115] *Ibid.*, 16, 51, 75, 171. [116] *Ibid.*, 51. [117] *Ibid.*, 75.
[118] *Ibid.*, 171. [119] *Ibid.*, 225, note. [120] *Ibid.*, 23, chs. 6–9.
[121] *Ibid.*, 195. [122] *Ibid.*, 195, 200–208. [123] *Ibid.*, 296, 300, 313.
[124] *Ibid.*, 309 ff. [125] *Ibid.*, 312. [126] *Ibid.*
[127] *Ibid.*, 303–12.

The neurosis itself arises from an inability to meet reality.[128] Not all such failure results in neurosis, for many people successfully adjust themselves to substitute-expression without serious functional emotional disturbances.[129] They forget their former loves or desires and become wedded to new ones.[130] In youth this is relatively easy, although one may pay the penalty later in a period of deprivation by having the old experiences, or some symbol of them, become neurotic symptoms. In later life, when the neural organization becomes more set and one's ideals have less resiliency, it is more difficult to find and accept substitutes. Where the strain is too great, the neurosis appears. That is, the repressed complexes refuse to remain under, in the unconscious. They burst through in the form of a substitute-idea or activity which has become associated with the thing desired. The symptom is an outlet, a safety-valve,[131] but it may be more harmful than the act or idea for which it is substituted. The symptom may be a pain, an ecstasy, a dual personality, a fixed idea, or an unregulated enthusiasm or aversion. In the more severe forms of illusion the neurosis is called a psychosis.[132] Some substitutions are sublimations.[133] Others are self-deceptive revolts against repression or censorship. Some individuals cannot solve their adjustment-problems by either method; "they are men and women who could not learn to accept the facts, who could not 'forget.' Instead, they tore themselves away from reality altogether, and realized their wish in their own way, thus solving the problem."[134] They are the insane. Dementia praecox is an extreme example of the inability to face reality.[135]

The theory of the conflict-neurosis set forth by Adler[136] is essentially unique. He starts with a metaphysical concept of a native "impulsion toward a maximation of the masculine ego-consciousness" or male dominance,[137] with a complementary tendency in women which is very similar to the more general but equally metaphysical infantile "will to omnipotence" and "search for lost omnipotence" of other psychoanalysts and psychiatrists.[138] As long as all the organs, physical and mental, are functioning properly, action runs smoothly and there is no psychic difficulty. But if some organ proves to be inferior, success in the primary end of the striving is rendered difficult or impossible and a feeling of inferiority, the inferiority complex, arises.[139] Two ways are open in the struggle to overcome this feeling of inferiority. One method is to build up the efficiency of the inferior organ, which we sometimes do until it is more capable than some of those organs which possessed a better start. Or compensatory development of other organs or of economy of organization may be undertaken with favorable results. But if both of these methods fail the subject may substitute phantasy for achievement.[140] In its severer forms this phantasy becomes an acute psychoneurosis which the subject exploits as a means to gain the ends which his organ-inferiority renders it difficult for him to attain.[141]

Doubtless this theory does explain many cases of psychoneurosis, especially those which arise from maladjustments due to the partial in-capacity of the subject. But there are many other cases which are due to the uncompensated hardness of the environment, which bears more heavily upon some than upon others. Environmental pressures are by no means uniform, and some persons with strong organic equipment must fight a losing struggle because they are "unfortunate," while often weaklings have things made easy for them because of a fortunate environment. This is a phase of the subject to which Adler has not given adequate attention. It is interesting to note, however, that he conceives the struggle or conflict to be between the organism, with emphasis upon the weak organ, and the environment, and not between two instinctive tendencies. It is in this sense that his theory of the conflict-neurosis may be called an environmental theory.[142] On the other hand, it is decidedly subjective, for it derives the psychoneurosis from an attempt to compensate a state of consciousness, the feeling of inferiority, instead of deriving it from an unreal and self-deceptive (and other-deceptive) attempt at establishing an effective adjustment to the external environment. Its subjectivity consists in its remoteness and indirectness. It errs in opposing that there must be a recognition of inferiority in order that a false technique of adjustment, that is, a psychoneurosis, may develop.

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[128] *Ibid.*, 37, 40, 160 f., 296.

[129] *Ibid.*, 40 ff. [130] *Ibid.*, 16, 47, 75. [131] *Ibid.*, 35.

[132] *Ibid.*, 26 ff. [133] *Ibid.*, 314, 320 ff. [134] *Ibid.*, 40.

[135] *Ibid.*, 28 f., 191, 253–261, 286.

[136] A. Adler, *The Neurotic Constitution.*

[137] *Ibid.*, 76.

[138] See White, *Mechanisms of Character Formation*, 177 ff.; *Outlines of Psychiatry*, 8 ff.; Ferenczi, *Contributions to Psychoanalysis*, 186–196; Lay, *Man's Unconscious Passion*, 184–8.

[139] *Ibid.*, ch. 1. [140] *Ibid.*, 56. [141] *Ibid.*, 96, 115–117.

Tansley's view of the basis of conflict is more explicit and dependable than those previously stated. "Mental conflict . . . is the struggle which takes place between two complexes whose conations would lead to incompatible actions."[143]. He does not fully agree with Trotter that the impulses must necessarily have instinct behind them,[144] but holds that "the major conflicts are always between complexes which have the force of the great primitive instincts at their back."[145] Of minor conflicts he says: "Minor conflicts may occur between any two complexes of sufficiently strong affect whose conations are incompatible, but they are of little importance in the general mental life."[146] Speaking of the kinds of social correlations of conflict, he asserts that "the classical examples of major conflict are those between sex and religion, between sex and morality, between patriotism and the family, religion and the family, and so on."[147] These types of conflict are to be met with constantly in the novel and drama as well as in real life. "All these may be generalized as conflict between the strongest individual instinct, that of sex, and that other great instinct which rivals sex in strength and significance, the instinct attaching to membership in the herd."[148] Here evidently he has unintentionally substituted instinct for complex, which earlier he stated to be the basis of conflict. Unlike Freud and some other psychoanalysts, he believes that conflicts may arise within the domain of the same instinct. "In many cases a major conflict may take place between differentiated and segregated complexes derived from one great primitive instinct, for instance the conflict between religion and patriotism, which in some forms at any rate represent different aspects of herd instinct, conflicting loyalties to two different 'herds'."[149]

According to Tansley, conflicts may be disposed of in three ways.[150] In mild cases the weaker complex may be inhibited by the stronger, without serious consequences. In more serious cases, where one impulse cannot be inhibited by another but comes into action in spite of opposition, the resulting action may be harmonized by means of a rationalization. This is called a *defence reaction* and usually has some reference to the social environment. Where these two methods cannot remove the conflict from consciousness a third and more drastic method, that of repression, may be called into service. For a longer or shorter time this last method may prove successful, but in the end the repressed tendency may secure "expression in indirect, symbolic, and often curiously distorted forms[151]. . . It cannot be kept permanently quiet in the unconscious, and the imprisoned libido, sooner or later, breaks out to the surface with violence, generally as a result of the accidental disturbance of the precarious balance which has been initiated and maintained by the repression."[152]Here we find the origin of the psychoneurosis. The results of the repression may be "disastrous to the mental health of the patient. Such repression often gives rise to hysteria, which may take the form of a somnambulism or other hysterical symptom, or may bring about a more complete rupture of the self, as in cases of double or multiple personality, or even a definite type of insanity."[153]

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[142] *Ibid.*, 117 ff.

[143] *The New Psychology and Its Relation to Life*, 101.

[144] Cf. Trotter, *Instincts of the Herd in Peace and War*, 82.

[145] *Op. cit.*, 101. [146] *Ibid.* [147] *Ibid.* [148] *Ibid.* [149] *Ibid.*

[150] *Ibid.*, 102–5.

Several typical psychoanalytic theories of the conflict-origin of neurosis have now been reviewed briefly and the more pertinent criticisms of these views have been made in connection with their exposition. We may turn now, by way of summary, to a statement of some general conclusions to be drawn from this discussion. In the first place, some psychologists and neuropsychiatrists would object to the use of the theory of conflict in explaining the origin of psychoneuroses and would employ some other concept, such, for example, as redintegration.[154] However, whatever the value of other concepts, it seems to be pretty well established that conflict, especially if we take account of its objective social as well as its subjective aspects, does play an important part in the functional neuroses. It is precisely at those points where strain is greatest and most intolerable that normal functioning of the mind breaks down and the psychoneuroses appear.

A second point to be noted is that the assumption regarding the content of the mental processes in conflict is erroneous. Complexes are miscalled instincts.[156] The so-called sex or reproductive, ego or self-preservative, and herd instincts are not inherited unit action-patterns, but are abstract value-concepts or classifications of activities according to similarity of end or aim which have no structural similarity or organic organization and unity. The so-called herd-instinct or herd-complex has the least truly instinctive content, the ego or self-preservative complex somewhat more, and the sex-complex most instinctive content of all. In all of these complexes environmentally produced habit dominates over instinct in the organization of action. Especially the sex or reproductive and the ego-complexes have large bodies of traditional and customary organization behind them, moulded by institutional pressures, which give the organization of their activity-correlates a degree of stability and definiteness which often clothes them with the deceptive appearance of an inherited unity of organization. The child grows so gradually into conformity with these historic action-patterns that their acquisition by him is not obvious. This has done much to foster the error of belief in their instinctive character. The action-patterns for sex-expression are closely standardized around marriage, prostitution and certain other customary, although disapproved, sex-practices. The ego-tendencies likewise are grouped around certain customary and conventional methods of property-holding, self-protection, self-display, eating, and the like, as well as the consciousness of values of relationships and activities for the self, or self-consciousness. The self-consciousness is clearly acquired, growing up as a function of the adjustments of the human organism to its environment, in which process these adjustments are evaluated by the organism's consciousness.

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[151] *Ibid.*, 107. [152] *Ibid.*, 112.

[153] *Ibid.*, 111. Cf. Janet, *The Major Symptoms of Hysteria.*

[154] Cf. H. L. Hollingworth, *The Psychology of Functional Neuroses.*

[156] Cf. Bernard, Instincts and the Psychoanalysts, above referred to.

The herd-complex consists of the consciousness and attitudes of the individual with respect to the social environment, and this consciousness grows up as a complement of the self-consciousness. The earlier analogues of the "herd-instinct" were the "religious" and "moral" instincts, which are now gone out of fashion among the psychologists. These last flourished in an age when thinking was theological or metaphysical,[157] that is, when social relationships were interpreted in terms of traditional dogmas and when morals were largely determined by ritualistie or customary conformity and conscience could be conceived of as an inherited entity. They were concepts of a lower stage of the development of the social consciousness. Their disappearance as instinct-categories is contemporaneous with the rise of the "herd-instinct" concept, which is merely a more enlightened substitute for them. The "herd-instinct" concept is more flexible; it corresponds to the scientific interpretation of social organization and control as proceeding from the dynamics of the motives and adjustments of people living together. It places the emphasis upon adjustment and organization of ends and methods of people living in the present, and discounts the traditional and customary convention and the externality of control which are characteristic of the theological and the metaphysical interpretations. But the "herd-instinct" is no more a reality than the "moral" and "religious" instincts which it displaced with a change in the fashion of thinking. It, like them and also like the so-called reproductive and self-preservative instincts, but even more than they, is an acquired complex, an abstract evaluation of actions or concepts related to the same perceived ends.

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[157] Bernard, Religion and Theology, *Monist*, 1922, 61–88.

Thirdly, in enumerating the sources of conflict, the herd-complex and the immediate pressures of the environment are often confused. The herd-complex is the individual's more or less standardized conception of social values. It is what he thinks of society and of social rights and obligations.[158] It is true that this complex may come into conflict with another complex or, perhaps we might better say, that activities or ideas related to this and some other complex may come into conflict. But the strongest conflicts can scarcely be between two psychic complexes or processes pertaining to them. The chances for conscious integration and adjustment are too great where both complexes are liable to the same conscious supervision and control. The most serious conflicts arise between one's habits or complexes and an unyielding environment which cannot be controlled from within. It is an external thing, in a measure unexpected and unadjusted, the organization of which must be either transformed by the attack of consciousness upon it or it will transform consciousness and rebuild the complexes. Or, if neither of these comparatively normal results can be brought about, the consciousness will develop illusions and hallucinations or some other abnormal and unreal adjustments to the environment, which constitute psychoneuroses. Almost without exception the psychoanalysts confuse the herd-complex or "instinct" with the environment, and thus vitiate the accuracy of their interpretation.

In the fourth place, the complexity and strain of modern life, built around a vast industrial and scientific technique as it is, has intensified the conscious analysis of objects, both of the self and the environment, by the individual intellect. As a consequence both the ego and the social alter are now much better defined than they have ever been in the past, although there have long existed social and self concepts in abundance. Our attention has already been called to the institutionalized forms of self-expression and to the social "instincts" of religion and morality as examples of these. This modern accentuation of self and social consciousness, with a consequent closer definition of the content of these two poles of consciousness, has intensified the conflict which may arise between the corresponding complexes or these complexes and the sex-complex, on the one hand, and these complexes and the environment on the other. The vividness and definiteness of consciousness in these complexes makes possible the drawing of closer distinctions between tendencies to action with a consequent intensification of conflict. The growth in volume of the psychoneuroses bears testimony to this fact.

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[158] Jelliffe approaches this interpretation, but does not formulate it, in his treatment of the welding of the conflicting egoistic partial "libidos" into an integrated personality under the influence of the social environment. *Op. cit.*, 48 ff.

Fifthly, as a corollary of this statement it may be proper to repeat Freud's statement that man only is capable of such intensity of conflict as is here implied and that only man has psychoneuroses.[159] If this is not absolutely true of some of the animals nearest to man in intelligence, it is relatively and essentially correct. The reason for this is, of course, that only man is capable of an effective conceptualization of environment and environmental forces. Therefore only he can develop that bipolar highly defined and intensified consciousness of self and society which makes it possible to transfer the mere physical opposition of individual and environment to the internal and acute psychic conflict.

Sixthly, the need for a rational analysis and organization of the external environment becomes obvious in the light of what has been said. A close analysis of the environment is necessary as a means to its classification and control. Organization of the environment will be possible in the degree to which we are able to define it, although complete organization and control, even with the most extensive applications of science, will not be ultimately feasible. Adaptation to environment is as imperative for the avoidance of extreme conflict as is control of environment. The psychoneurosis is itself in effect an illusion of organization and control of the environment when actual control has not proved possible.

Finally, this organization of the environment should in a large measure properly take the form of the provision of substitute-channels for expression of the energy of metabolism. As a matter of fact, but the smaller portion of our energies is attached by inheritance to instinctive channels of expression.[160] The surest way of avoiding disastrous conflicts which produce psychoneuroses is so to organize the environment as to provide channels of expression which increase the adaptation between the individual and his environment rather than foster the maladaptation or conflict aspects. This can be accomplished only by means of a thorough analysis of both the individual and the environment, especially of the psychosocial environment. This calls not only for the careful development of the science of psychology but of a science of sociology as well. Both are indispensable to the end desired.

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[159] *General Introduction to Psychoanalysis*, 358. Of like significance is Brill's contention that neurotics constitute a high class of persons. *Op. cit.*, 25–7.

[160] Tansley, *op. cit.*, 84.

THE PERCEPTIONS OF LIQUIDITY, SEMI-LIQUIDITY AND SOLIDITY

By ALICE H. SULLIVAN, University of Illinois

The purpose of this study was threefold: (*a*) to compare the perceptions of liquidity and solidity, in order to find what conditions are peculiar to each of the two perceptions; (*b*) to compare the perceptions of liquidity and semi-liquidity, in order to discover the conditions peculiar to the perception of semi-liquidity; and (*c*) to find how many separate and distinct perceptions are included under the term 'liquidity'. Since there was nothing in the literature on touch blends to aid us in answering these questions except Bentley's work on the perception of liquidity[1], it was necessary to have recourse to experiment.[2]

The method of experimentation was as follows: the *O*, blindfolded, sat with his right arm supported by a raised arm-rest, with the middle finger extending down through a hole in the end of the arm-rest. After a 'ready' signal, *E* slowly raised, by means of a pulley, a small beaker containing liquid or other substance until *O*'s finger was immersed to the first joint. *O* then reported (*a*) upon the sensory components of the experience, (*b*) upon the pattern or arrangement of the sensations, and (*c*) upon the meaning of the experience. In order to avoid any clue to the nature of the stimulus, the finger was always wiped after every stimulation whether the stimulus was wet or dry. There were 8 *O*s, some of whom observed in all the experiments and some only in part.[3]

Experiment I.: Perceptions of Liquidity and Solidity Compared

In the first series of experiments, we compared the perceptions of liquidity and solidity. Water, sand, meal, flour, cornstarch, and cotton comprised the stimuli. The dry substances were sometimes cooled or warmed, and sometimes left at room temperature.

Table I. shows numerically the results of analysing the perceptions resulting when dry stimuli were used. It does not include the experiments in which water was used as stimulus because the dry substances, being sometimes warmed or cooled, furnished the conditions for perceptions of liquidity as well as solidity. Consequently, the comparison of the two perceptions was made on the basis of the experiments with dry substances.[4] The second column of the Table indicates the number of experiments; the third column shows the number of times the analysis revealed both pressure and temperature; the fourth column, the number of times the analysis revealed only pressure; the fifth column, the number of perceptions of liquidity or semi-liquidity experienced; and the last column, the number of perceptions of solidity experienced. In most cases columns 3 and 5 and columns 4 and 6 refer to the same experiences.

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[1] M. Bentley, this JOURNAL, 1900, 9, 414.

[2] See references to studies on touch blends at end of this paper.

[3] The observers were: Professor M. Bentley, Dr. P. T. Young, and the writer, all members of the department of psychology, and Misses M. Bales, C. Blackman, O. Nilson, and D. Whitney, and Messrs. W. Lutyens and R. Newcomer, all seniors and major students in psychology.

TABLE I.

Comparison of Perceptions of Liquidity and Solidity

Obs.	No. of Exps.	Pres. Temp.	Pres. alone	Percep. of Liquidity	Percep. of Solidity
Ba	5	1	4	1	4
Bl	1	0	1	0	1
L	6	3	3	1	5
Ne	3	2	1	2	1
S	25	11	14	10	15
W	7	2	5	0	7
Y	22	13	9	9	13
Total	69	32	37	23	46

The Table shows that, generally speaking, perceptions of liquidity are made up of blends of pressure and temperature, while perceptions of solidity are made up of patterns of pressure alone. *E.g.*, Ba reported one perception of liquidity and one case in which both temperature and pressure were present in the analysis, while she reported 4 perceptions of solidity and 4 cases in which the analysis showed a pressure existing alone. Out of a total of 69 experiments, there were 23 perceptions of liquidity and 32 cases in which the analysis indicated both pressure and temperature, while there were 43 perceptions of solidity and 37 cases in which only pressure was reported in the analysis. There were, then, 9 exceptions to the rule that liquidity is composed of a blend of pressure and temperature and that solidity is composed of pressure alone. These 9 exceptions were cases in which a temperature sensation was present, but the meaning was that of solidity because (*a*) the temperature was not blended with the pressure, but surrounded it, or (*b*) the temperature was blended with the pressure and the meaning was that of liquidity until the finger moved and gave a perception of graininess, when the perception changed to one of solidity.

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[4] In the 45 experiments in which water was used as stimulus the reports always indicated a blend of pressure and temperature in the analysis of the perception. This verifies Bentley's analysis of liquidity.

The following reports of the *O*s may serve to make clearer this distinction between the conditions underlying the two perceptions.

Ba (meal) "Clear pressure and no temperature. The pressure is in a pattern—a circle of limited extent. It feels like a piece of stretched cloth and is dry."

Ne (meal) "Slight coolness and intense pressure; wet."

S (sand) "There are uneven bits of intense pressure and a slight warmth. It seems soft and doughy. Now the bits of pressure slide, giving a perception of graininess, and it feels like sand."

W (cornstarch) "Pressure which is not very heavy, and neutral temperature. It feels dry and powdery."

Y (warm cotton) "That is warm mercury. There is weak pressure and a slight warmth; not much temperature and the pressure is vague and ill-defined."

S (sand) "Pressure and cold. It felt at first like a very thick gelatine, but suddenly all temperature disappeared and it then meant sand."

Y (flour) "There is a pattern of pressure which is not uniform, and the temperature is indifferent. It is sand or coarse powder. The pattern of pressure gave me the perception of solidity, for the pressure of solids is spread out in a way quite different from that of liquids."

Common sense, as represented by the dictionary, says that a substance is perceived as a liquid when it "flows freely like water," and is perceived as a solid when it "resists forces tending to deform." According to these definitions, liquidity and solidity depend upon the temporal course of experience. According to our reports, however, the perceptions of liquidity and solidity do not depend upon temporal course; for just as good perceptions were obtained when drops of liquid or solid substance were applied to the back of the hand. In these reports, 'solidity' means merely the absence of liquidity.

The first part of our problem, then, may be answered thus: a perception of liquidity is experienced whenever there is a fusion of pressure and temperature, and the perception may be changed to one of solidity by breaking up the fusion, leaving a pattern of pressure alone.

Experiment II.: Perceptions of Liquidity and Semi-liquidity Compared

The second part of our problem was to study a number of liquids of different densities in order to discover the conditions underlying the perception of semi-liquidity, and to compare these conditions with those underlying the perception of liquidity. The stimuli were liquids of various thicknesses, including water, gelatine, bandoline, cornstarch pudding, cornmeal mush, molasses, and mercury. The procedure was similar to that of Experiment I. There were 6 *O*s in this experiment, most of whom had observed in the first experiment.[5]

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[5] Miss Olga Nilson, a senior and major student in psychology, assisted us in this series of experiments.

Table II. gives the results of Experiment II. It is arranged to show the number of times intense pressure *plus* temperature was found in the analysis, the number of times weak pressure *plus* temperature was found, and the number of perceptions of liquidity and semi-liquidity reported, first when thick liquids were used as stimuli and secondly when thin liquids were used. The second column of the Table indicates the number of experiments; the third column, the number of times the analysis revealed intense pressure and temperature; the fourth column, the number of times weak pressure and temperature were reported; the fifth column, the number of times the meaning was that of a thick liquid; and the last column, the number of times the perception was of a thin liquid. If the analyses of the *O*s agreed completely, the numbers in columns 3 and 5 and the numbers in columns 4 and 6 would be equal.

TABLE II

Comparison of Perceptions of Liquidity and Semi-liquidity

Semi-liquids

Obs.	No. of Exps.	Intense Pres. *plus* Temp.	Weak Pres. *plus* Temp.	Percep. of Semi-liq.	Percep. Liq.
B	1	1	0	1	0
L	19	14	5	17	2
S	20	15	5	15	5
N	3	3	0	3	0
W	23	18	5	22	1
Y	5	2	3	2	3
Total	71	53	18	60	11
			Liquids		
B	2	2	0	2	0
L	2	0	2	0	2
N	2	0	2	0	2
S	6	0	6	0	6
W	4	1	3	2	2
Y	3	0	3	0	3
Total	19	3	16	4	15

The first part of the Table shows 60 cases in which semi-liquids were perceived as thick and 11 cases in which they were perceived as thin, while there were 53 cases in which the perceptions were analysed into a blend of intense pressure and temperature, and 18 cases which showed a blend of weak pressure and temperature. There were, then, 7 cases out of 71 in which the analyses did not agree. Apart from these, semi-liquidity was always analysed into a blend of intense pressure and temperature while liquidity was analysed into a blend of weak pressure and temperature. The second part of the Table, in which liquids were used as stimuli, gives further evidence that weak pressure and temperature when fused mean liquidity. The

4 cases in which a thin liquid meant semi-liquidity instead of liquidity will be explained in Experiment III.[6]

There were 5 experiments which could not be included in the Table because the meaning was different from that of any of the other experiences. Here the *O*s perceived the liquid not as liquid or semi-liquid but as merely damp or vaporous. Ba reported 3 such perceptions, the meaning being at first that of liquidity, and being analysed into cold and pressure, and later changing into a perception of dampness, when the pressure had adapted out. S reported 2 such experiences. Bershansky, in her study of Thünberg's illusion, found perceptions of dampness, which she analysed into a less intense pressure and perhaps a less intense cold than that of wetness.[7] Dampness, then, is the experience which actually contrasts with solidity.

Samples of the reports with different stimuli follow.

B (mercury) "Cold and intense pressure at tip; increase in size and intensity of the pressure and cold; visual imagery giving a panorama of liquid mounting, with fairly definite identification of the liquid as mercury."

L (cornmeal mush) "Warmth, which is intense at first and then decreases in intensity; rather intense pressure, which is greatest at tip of finger. It is thick and spongy."

N (water) "There is a cold coming up the finger in parallel lines and it settles around the finger; there is very little pressure connected with this. It is thin like water."

S (gelatine) "Pressure and temperature, both moderately intense. It means a thick liquid like gelatine."

S (water) "There is cold and a pressure so thin and lacy that it is hardly perceptible. It is merely damp instead of wet. When I have the meaning of dampness I am not sure there is pressure there at all."

W (cornstarch pudding) "Cold although there is no definite cold ring around the top as there usually is. The cold is fused with a close-fitting, clingy pressure which makes it feel thick and wet."

Y (bandoline) "Very cold with a chunk of pressure. It is thicker than water. The meaning of thickness is given by the greater pressure and by the fact that the pressure is more definitely formed than it is in thin liquids."

In order to make sure of our analysis of semi-liquidity, we tried synthetic semi-liquids, using as stimuli substances which were dry and heavy enough to give a rather intense pressure, such as wads of cotton, sometimes warmed or cooled or slightly moistened, and a rubber balloon, partially filled with air, and also warmed or cooled. Out of 12 experiments with cotton, warmed or cooled or moistened, there were 12 reports of semi-liquidity, the meanings being jelly-like, soggy, spongy, muddy, gummy, and doughy. Out of 27 experiments with the balloon, there were 23 perceptions of semi-liquidity, the characterizations being thick syrup, gelatine, mercury, and dough. These were analysed into a blend of moderate pressure and temperature when the meaning was that of a moderately thick liquid, and into a blend of intense pressure and temperature when the meaning was that of a thick liquid or semi-solid. One interesting case where the balloon was used at room temperature, and where the analysis indicated only pressure, resulted in a perception of pumice stone, showing that when the *Os* had been experiencing fusions of pressure and temperature that meant liquidity or semi-liquidity the absence of temperature made them perceive the stimulus as the dryest thing possible. A few of the *Os*' reports for cotton and the rubber balloon follow.

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[6] Cf. p. 536.

[7] Bershansky, this JOURNAL, 1923, 34, 293.

> L (balloon) "Pressure and cold. It is a thick jelly-like mass, like mercury, although it does not cling quite as much as mercury."
>
> S (warmed cotton) "Weak warmth and a moderately intense pressure, which clings to the finger. It feels like warm thick cornstarch pudding."
>
> Y (cooled cotton) "A soft pressure, not lumpy and not nearly so well defined as some of the others. A slight cool. It seems somewhat like mud, slightly cool mud. There is a consistency thing about all these; they have differences in consistency and solidity."

Semi-liquidity, as well as liquidity, is conditioned by a fusion of pressure and temperature. The *fusion* of the sensory components is the condition which gives to all liquids, both thick and thin, the meaning of liquidity. The consistency of the liquid resolves itself, upon analysis, into the intensity of the pressure in the fusion; the more intense the pressure, the denser the liquid perceived. The comparison of perceptions of liquidity and solidity in Experiment I. showed a distinct break between the two experiences; there is no such break between experiences of liquidity and semi-liquidity. The two perceptions merge into each other and both of them carry the meaning of liquidity, for both are conditioned upon a fusion of pressure and temperature.

Common sense says that a substance is perceived as a liquid when it falls away from the finger which enters it, and a substance is perceived as semi-liquid when it offers resistance to the entering finger. To be sure, the *Os* often reported a temporal course in their experiences of liquidity and semi-liquidity, but this was not essential to the two perceptions, and was similar for the two. There is nothing on the side of process to distinguish the two perceptions save varying intensities of pressure.

Experiment III.: Semi-liquidity as Conditioned by Increased Intensity of Pressure with Increased Temperature

It may be recalled that Table II. recorded 4 cases in which water was perceived as semi-liquid.[8] These 4 cases occurred when the water used was colder than usual. This result with cold water brought to mind the results obtained with warm water in our work on oiliness.[9] There we found that increase in the warmth of the water brought about an increase in the snugness of the pressure, and we were inclined to consider this phenomenon to be of central origin, the pressure *feeling* snug simply because it was blended with warmth. Bershansky, in repeating our experiments, found the same phenomenon, and says: "It is of course possible that the pressure in this case has its origin internally, and is referred to the surface. Internal pressure is, however, frequently reported, and we should still have to explain why increase in warmth of stimulus carries with it increase in felt pressure."[10] Since Experiment II. of this study had given us a hint that increase in cold also carries with it increase in felt pressure, we decided now to run through a series of experiments, using water of all degrees of temperature from 0° to 40°C. If increase in cold or warmth resulted in increase in intensity of pressure, water of different degrees of temperature should give perceptions of semi-liquids of varying degrees of thickness. Since there were already a number of cases reported in which increase in warmth brought about increase in intensity of pressure, especial emphasis was laid upon the experiments with cold water. The results of this series of experiments are given in Table III.

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[8] Cf. p. 535.

[9] Cobbey and Sullivan, this JOURNAL, 1922, 33, 121.

TABLE III.

Perception of Semi-liquidity from Cold and Warm Water:
Water 0° to 15°C.

Obs.	No. of Exps.	Percep. of Semi-liquidity	Meanings
B	2	2	Mercury; heavy liquid
Ba	4	2	Denser than water, less than mercury
Bl	3	2	Mercury
L	8	5	Mercury, melting snow
Ne	6	6	Gelatine; thicker than mercury
N	5	3	Gelatine; kerosene
S	9	7	Melting snow; between water and mercury; mercury
W	5	1	Molasses
Y[11]	7	2	Between water and mercury
Total	47	30	
		Water 38° to 40°C.	
L	13	13	Oil; butter
S	5	5	Oil; melted butter
W	4	3	Mercury; oil
Y	3	2	Thicker than water; oil (?)
Total	25	23	

Total for warm and cold water: of 72 experiments, 53 perceptions of semi-liquidity.

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[10] L. Bershansky, this JOURNAL, 1922, 33, 584.

[11] After Y had reported two experiences of semi-liquidity with cold water as stimulus, he was told of the conditions of the experiments, and thereafter he always perceived the stimulus as ice-water. This indicates the importance of keeping *O* in complete ignorance of the conditions of the experiment, especially when *E* is trying synthetic conditions.

The results shown in Table III. bear out the findings of the previous investigations in regard to an increase in pressure with an increase in warmth, and add the fact that there is an even more pronounced increase in intensity of pressure with an increase in cold. Every one of the 9 *O*s perceived very cold water as semi-liquid at times, and every one of the 4 *O*s who observed in the experiments with warm water perceived this as semi-liquidity. Out of 47 experiments with ice-water, there were 30 reports of semi-liquidity, the perceptions varying in density with the intensity of the cold, and running from liquids perceived as slightly thicker than water to liquids perceived as thick as partially melted snow. When warm water was used, there were 23 perceptions of semi-liquidity out of 25 experiments, the meanings here being most often oiliness or greasiness. There were, then, 53 cases out of 72 experiments in which an increase in the temperature of the stimulus resulted in an increase in the intensity of felt pressure, making the perception that of semi-liquidity.[12] Some examples of the *O*s' reports follow.

B (water 10°C.) "Cold of considerable intensity, with fairly strong pressure, which is well defined at end of finger. Variable intensity and extent of both pressure and cold. First there was visual imagery which meant 'There is a liquid stuff coming up the end of my finger.' It seemed to mean something there a little heavier than water. There were also verbal comments in auditory kinaesthesis, which meant 'Ah, there is something cold and wet, possibly mercury; no, it is more like water.' "

L (water 15°C.) "Intense pressure and very cold, which rise in parallel lines. It is slightly thicker than water."

S (water 8°C.) "Intense pressure and subcutaneous pressure plus cold, which is quite intense. The cold is not as clear as the pressure. I could feel the finger cut through the liquid which clung to the finger more than water does. It is very thick like partially melted snow."

Y (water 8°C.) "Intense cold and a ring of pressure going up the finger. It is fairly thick and there is some resistance. It meant a liquid that at first I thought might be water, then mercury. It really was between the two."

These reports show that very cold water is perceived as a semi-liquid. Under the conditions of this experiment only water of a very limited range of temperature is perceived as water or liquid, while water of extreme temperature is perceived as gelatine or mercury or even partially melted snow. The initial contact of the finger with the liquid is the time when thickness is best perceived with cold water. It is at this point that most of the reports of extreme thickness, such as mercury or partially melted snow, were given. The skin very quickly adapts to the cold and then the intense pressure immediately decreases. Then the perception undergoes a change.

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[12] It is interesting to speculate as to the cause of this change in intensity of pressure with change in temperature. We are now engaged in experiments in an attempt to find the explanation. At present we are inclined to think that the vascular change, which is known to occur when the skin is subjected to extremes of temperature, stimulates, in some way, the subcutaneous pressure organs. Another explanation, suggested by Dr. Young, is that a cold or warm stimulus may affect the pressure organs in the skin, as well as the temperature organs.

When warm water was used as stimulus, the perception was that of a liquid slightly thicker than water which meant, in most cases, oiliness. Out of 25 experiments with warm water there were 18 reports of oiliness. Since oily liquids were not used as stimuli, the *Os* could not have perceived oiliness because of suggestion. The following reports of the *Os* are typical.

L (water 40°C.) "Warm, medium pressure, thicker than water. Does not cling to finger. Feels oily."

S (water 38°C.) "It feels like melted butter. The pressure is fairly weak and is blended with an intense warmth. It is a little thicker than water."

Y (water 38°C.) "Warm pressure, wet; I started to say oily; there was just a suggestion of oiliness. The perception was of a warm liquid thicker than water."

It is evident that the perception of oiliness includes the meaning of thickness. This fact may account, in part at least, for Bershansky's disagreement with our earlier study of oiliness, where we said that the compulsory condition for the perception of oiliness was a fusion of warmth and pressure.[13] Bershansky says that oiliness is not a "sheerly cutaneous perception", and this is true. She also says that a fusion of warmth and pressure is not "the single adequate condition" for the perception; but we still fail to find anything else on the side of process. In Experiment III., cold water was perceived as wet, and when the water was heated to a certain temperature it was perceived as oily. It is admitted that wetness is conditioned by a fusion of cold and pressure; and if wetness is changed to oiliness simply by changing the cold to warmth, it is reasonable to say that oiliness is conditioned by a fusion of warmth and pressure. It may be that the internal change, whatever it may be, which causes an increase in the intensity of pressure with an increase in temperature, does not occur except in cases where the stimulation is areal. It would be better, therefore, to lay down the conditions for the perception of oiliness, as well as for all other perceptions of semi-liquidity, for areal stimulation only. Later, with further experimentation, we hope to be able to say definitely what the results of punctiform stimulation are.

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[13] I. Bershansky, this JOURNAL, 1922, 33, 584. See also Cobbey and Sullivan, this JOURNAL, 1922, 33, 121.

By changing the temperature of water, it is possible to get perceptions which change in density so that the whole series of liquid, semi-liquid, solid integrations is represented, thus:

0°C—mushy (partially melted snow)
10°C—muddy (mercury)
15°C—gelatinous (gelatine)
25°C—wet (water)
38°C—oily (oil)
42°C—greasy (melted butter)

It is too soon to say, of course, that the compulsory condition for the perception of mushiness, which may carry the object-meaning of partially melted snow, is water of 0°C., or that one is certain to perceive water at 15°C. as gelatinous, for individual differences are quite pronounced; but it is possible to say that the perceptions most often obtained with water of certain temperatures are the perceptions given in the column above.

This study of liquids and semi-liquids and synthetic semi-liquids makes it possible to enumerate the experiences included in the term liquidity. We found a series of perceptions beginning with dampness or vaporousness, running through all experiences of liquidity and semi-liquidity, and ending with the experience which contrasts with vaporousness, namely, solidity. Thus liquidity is bounded on the one side by vaporousness and on the other side by solidity. Such a series includes the following experiences:

Liquid	Semi-liquid	Semi-solid	Solid
Vaporus, wet, oily,	gelatinous, slimy, greasy,	muddy, mushy, soggy,	doughy soggy, dry

If we let 'p' represent weak pressure, 'ᴘ' represent moderate pressure, and 'P' represent strong pressure, 'c' represent weak cold, 'c,' moderate cold, and 'C,' strong cold: 'w' represent weak warmth, 'w,' moderate warmth, and 'W,' strong warmth; the foregoing perceptions may be pictured in terms of their sensory components thus:

Vaporous,	wet,	oily,	gelatinous,	greasy,	mushy,	doughy,	dry
C(p),	Cp,	Wᴘ,	cᴘ,	wᴘ,	cP,	wP,	P(w)

Conclusions

1. All perceptions of liquidity have in common one condition, namely, a fusion of pressure and temperature. This fusion is lacking in perceptions of solidity, in which there is, in most cases, only a pattern of pressure. The relationship between liquidity and solidity may be shown thus:

$$\text{Liquidity} \propto \text{Solidity}$$
$$pT \propto P(t)$$

2. All perceptions of semi-liquidity, as well as those of liquidity, are conditioned by a fusion of pressure and temperature.

It is impossible to say just where liquidity ends and semi-liquidity begins, the only difference being one of intensity of the pressure. In most cases of semi-liquidity the intensity of pressure and temperature is about equal. The more intense the pressure, the denser is the liquid perceived. The relationship between liquidity and semi-liquidity may be represented thus:

$$\begin{array}{ccc} \text{Liquidity} & \propto & \text{Semi-liquidity} \\ pT & \propto & PT \end{array}$$

3. Liquidity is a broader term than wetness or oiliness or semi-liquidity; it includes all these and a great many more perceptions. It is possible to arrange a series of perceptions beginning with experiences of liquids so thin that they are merely damp or vaporous and running through all the intermediate stages to solidity. Such a series includes the following perceptions:

Vaporous, wet, oily, gelatinous, slimy, greasy, muddy, mushy, soggy, doughy, spongy, dry.

The relationship between the end terms and the middle terms of these experiences may be shown thus:

$$\begin{array}{ccccc} \text{Liquidity} & \propto & \text{Semi-liquidity} & \propto & \text{Solidity} \\ (p)T & \propto & PT & \propto & P(t) \end{array}$$

4. There is a decided increase in the intensity of pressure with an increase in the intensity of the cold or warmth in the fusion which conditions perceptions of semi-liquidity. This enables one to obtain good synthetic semi-liquids by using cold and warm water as stimuli.

5. The large class of experiences included in the term 'liquidity' are true blends or fusions, but the equally large class of experiences included in the term 'solidity', such as dry, soft, hard, etc., are really not touch blends at all, if we mean by touch blends fusions or integrations. They are, rather, cutaneous patterns or mosaics.

References to Studies on Touch Blends

1. M. Bentley, this JOURNAL, 1900, 9, 414.
2. I. Bershansky, this JOURNAL, 1922, 33, 584.
3. I. Bershansky, this JOURNAL, 1923, 34, 291.
4. L. W. Cobbey and A. H. Sullivan, this JOURNAL, 1922, 33, 121.
5. R. S. Malmud, this JOURNAL, 1921, 32, 571.
6. S. Tung, this JOURNAL, 1921, 32, 421.
7. M. J. Zigler, this JOURNAL, 1923, 34, 73.
8. M. Meenes and M. J. Zigler, this JOURNAL, 1923, 34, 542.
9. M. J. Zigler, this JOURNAL, 1923, 34, 550.

AN EXPERIMENTAL STUDY OF THE PERCEPTIONS ROUGHNESS AND SMOOTHNESS

By M. Meenes and M. J. Zigler, Princeton University

In 1903 Thunberg[1] reported that a singular perception of smoothness is touched off by holding the hands, one on each side of a vertical network of metal, in such a way that the volar surfaces of the hands and fingers touch each other through the network openings, and drawing them proximally so that they slide over the wire network always in the same mutual relation. The same perception is realized under identical conditions when the metal network is replaced by other obviously non-smooth objects, such as sail-maker's cord. Thunberg concludes that this perception is a complex experience, which results from a fusion of two different perceptions, each derived from separate portions of the skin that are in immediate proximity during the stimulation. He says that the experience fails to arise when only one hand is moved over the network and when the volar surfaces of the hands are moved back and forth against each other. This perception of smoothness, he says, analyses into a uniform perception of touch and a perception of facile motion of the sensitive surface in relation to the touched object. He emphasizes the fact that only a poor perception of "the degree of smoothness that characterizes an object" is obtained by merely touching the object, even with such a very sensitive part of the skin as the finger tips; hence, movement is indispensable to the perception.

In view of the fact that this perception of smoothness was aroused under unusual and complex conditions, and that Thunberg speaks at times of smoothness as characterizing the stimulus-object, we felt that it might prove fruitful psychologically (1) to repeat certain parts of his work in order to secure, if possible, a fuller description of the psychological experience of smoothness, and to ascertain whether movement is an indispensable condition of the perception, and in that event to determine the precise rôle of this factor in the perception; (2) to simplify the conditions of his experimental work by confining the stimulation to one portion of skin instead of stimulating two juxtaposed surfaces; and (3) to extend his study so as to include also the counterpart experience of roughness, and to ascertain whether these two perceptions represent the extremes of a simple qualitative psychological series between which a number of corresponding intermediate qualities fall.

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[1] T. Thunberg, An egenartad Förnimmelse of Glatthet och des Analys, *Upsala Läkareförening, Forhandlingar N.F.* 8, 1902–3, 660–663.

ANALYSIS OF SMOOTHNESS AND ROUGHNESS

(1) As our first experimental step we attempted to determine whether stimuli which are popularly termed rough and smooth give rise to such perceptions when presented without movement. The stimuli selected for this purpose were a series of 12 papers of varying degrees of roughness and smoothness; a smooth glazed paper represented one extreme and a coarse sandpaper the other, and 10 other papers of varying degrees of roughness and smoothness occupied the intermediate positions. For convenience the stimuli were numbered 1 to 12, from most smooth to most rough. These were presented in haphazard order by means of an apparatus described by Zigler,[2] which was altered, however, by removing the spring and metal disc applicator and substituting in its place a long iron rod over the lower end of which the papers serving as stimuli were clamped. With this apparatus we were able to bring the stimulus down upon the volar surface of the tip of *O*'s forefinger, hold it there steadily for a period of 3 sec., and then remove it directly upward without any movement excepting those in the vertical plane at the moments of application and withdrawal.

There were four *Os*: Mr. Norborne H. Crowell (C), graduate student; Miss Janice H. Osias (Oz), editorial assistant of the Psychological Review; Mr. Frank A. Pattie (P), graduate fellow; and the last named of the joint authors (Z), who worked with knowledge of the problem. The *Os* were blindfolded and seated before a screen with an aperture through which they inserted the right hand, volar side upward. The instructions read: "Two seconds after the signal 'Now,' a stimulus will be applied to the forefinger of your right hand. Observe carefully the field of consciousness during the stimulation and at its termination give a detailed account of the experience."

In this series of experiments only Oz designated the experiences as smooth and rough, and she did not give positive declarations of these qualities but inclined to use such qualifying remarks as; "almost smooth," "gritty or almost rough," "rough but can hardly be called so," "that would hardly be called rough." She reported further that these impressions of smoothness and roughness were perceived mainly at the moments of application and withdrawal of the stimulus, when there was, of course, vertical movement. Thus for stimulus 11 she says; "A little gritty and rough at first, then unevenness of pressure sensations for the main part of the period."

None of the other *Os* named these experiences smooth or rough. They did not use these terms, excepting in an occasional negative connection, when an experience was designated as not rough or not smooth. These *Os* designated the experiences occasioned by this mode of stimulation as evenness and unevenness. Oz also inclined to designate the major part of the experiences by the same labels. For stimulus 12 (grade 3 sandpaper) she reported: "Surface uneven, an unevenness of pressure sensations. There were greater pressure sensations in ridges with slighter pressure sensations between them." And for stimulus 2 she said: "An even surface giving rise to evenness of pressure sensations over the entire area."

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[2] An Experimental Study of the Perception of Stickiness, this JOURNAL, 1923, 34, 75f.

It seems, then, that when differing intensities of pressure sensations are experienced in the absence of movement, our *Os* label the perception unevenness and not roughness; and when the pressure pattern is uniform while lacking movement, the preferred name for the perception is evenness and not smoothness.

The *Os* described evenness as consisting of a field of pressure qualities in which all pressure elements have the same level of intensity and clearness and a closely aggregated distribution, but lack the compactness of aggregation, which is characteristic of smoothness. Unevenness consists of an areal pressure pattern in which certain small aggregations of pressure possess a higher intensity and clearness than others, and in which these aggregations of higher and lower intensities maintain practically the same spatial positions throughout the experience.

(2) Failing to secure reports of genuine roughness and smoothness when both stimulus and stimulated member were passive, we next added activity of the member to the experience. *O* was now instructed: "After the 'ready' signal you will be presented with a stimulus which will be placed on your passively outstretched forefinger. Then, when the 'now' signal is given, you are to drop the thumb lightly on the stimulus directly over the forefinger without the slightest rubbing of the fingers over the stimulus. At the second 'now' signal, carefully lift the thumb from the stimulus, which will then be removed by *E*. Try to name the tactual experience and give a careful introspective account of it." The same series of stimuli was used.

Here occasional judgments of roughness and smoothness appear in the reports of all *Os*. Z, however, reports no clear-cut perceptions of smoothness and roughness, although he finds suggestions of them. All *Os* again prefer to label these experiences evenness and unevenness of pressure. They indicate that the suggestions of smoothness and roughness arise during the initial contact of the thumb with the stimulus when slight movement is involved. The following reports are typical:

> "That experience was evenness of pressure, but during the movement of the thumb in clamping the stimulus I felt slight roughness" (P). "A brief gritty or rough perception was set up during the clamping stage, but this was immediately succeeded by mere unevenness of pressure, which endured until at the release of the stimulus I again got a suggestion of roughness" (Z). "At first roughness, then I just felt unevenness" (C).

The reports of two *O*s indicate that the finger is very obtuse to slight differences of pressure when both stimulus and stimulated member are stationary, while sensitivity is much heightened with the introduction of movement.

"Pressure sensations of different intensities which give rise to unevenness; and it even felt rough and gritty in very definite parts; but when the pressure of the thumb continues to be applied these differences of pressure disappear and blend into one sensation which I call evenness. When the thumb is raised again at the end of the experience I am again aware of the differences of pressure" (Oz). "Unevenness was perceived as soon as I dropped my finger upon that stimulus. There were several tiny points of increased intensity of pressure which gave this impression. This appeared immediately upon the dropping of my thumb, and then until I elevated my thumb I was totally unaware of this unevenness; the experience was evenness during this period. When I raised my thumb, however, I again received the suggestion of slight unevenness" (Z).

(3) In the next experiment the stimulated member was kept passive, all movement being confined to the stimulus.

A special apparatus was devised to carry the moving stimulus. It consisted of a phonograph motor encased in a mahogany box 30 cm. square and 15 cm. high. The vertical shaft, which supports the phonographic disc platform, extended through a small hole at the mid-point of the mahogany box about 15 mm., and a small cogwheel was substituted in place of the platform. This wheel was geared with a larger toothed wheel, which bore a metal radius-block on its upper surface. The radius-block was pierced by 5 holes at regular intervals from center to circumference of the cogwheel, and one end of a straight horizontal shaft 9 cm. in length was fastened to one of these holes by a screw. The radius-block was of sufficient thickness to allow the shaft to clear the center-tap of the cogwheel during rotation. The other end of this shaft was pivoted with the end of a longer straight horizontal shaft 17.5 cm. in length, which ran off at an angle of about 90° from the former. This longer horizontal shaft was centered on a post, which was located at the edge of the mahogany casing; thus, one-half of this shaft extended over the edge of the box. The stimulus carrier, which was controlled by guides at the edge of the box so that it moved along the side of the box without making any lateral movement, was attached to the outer end of this long shaft by means of its center-pin, which played in a slot in the outer end of the shaft. Thus, as the cogwheels rotated under the driving force of the motor, the carrier made a reciprocating movement, carrying the stimulus back and forth.

A delicately balanced horizontal arm was suspended from the center-pin of the carrier. On one end of this arm a cylindrical terminal, 2 cm. in diam., was fitted and the stimulus was clamped over the end of this terminal. A counter-weight, placed on the opposite end of the arm at a like distance from the center-pin, balanced the terminal. The arm was graduated in units of 10 gr. in both directions from this zero point, so that the pressure of the stimulus could be applied to the finger either from below or from above. The extent of the movement varied from a maximal complete excursion of 15 cm. to a minimal of 4.8 cm. with three intermediate distances. Since we stimulated the fingertip only, the shortest distance was most appropriate for our work. Although this apparatus was not constructed to insure absolute regularity of movement, it achieved this end satisfactorily for our work. In all of our work the stimulus carrier moved at the rate of one full excursion in 3.43 sec.

Every *O*'s hand was provided with a plaster cast which allowed only a little more than the first joint of the index finger to be exposed. The hand rested in the cast dorsal side upward. This enabled us to keep *O*'s hand perfectly stationary and also to eliminate the fatigue that is usually occasioned by holding the arm volar side upward for a considerable period of time or by failure in having the weight of the member distributed over a considerable portion of its surface. *O* was again both screened and blindfolded from the stimulated part.

To our original series of stimuli we here added an additional one—a highly polished piece of brass—which better satisfied the demands of extreme smoothness. This became No. 1 of our series of 13 stimuli. At every presentation the stimulus carrier was first set in motion, then the stimulus was applied to the finger tip and allowed to make about 3 full excursions before it was removed and the machine stopped. Thus, the stimulus was in motion throughout the period of stimulation. We began with a pressure of 20 gr. and later took observations with pressures of 30, 50 and 70 gr. The instructions read: "After the 'ready' signal a tactual stimulus will be applied to your finger. Observe the experience carefully and at the end of the stimulation name the perception and give a complete introspective account of the experience in terms of the mental processes."

Under these experimental conditions all *O*s gave positive declarations of smoothness and roughness. Other qualities, such as grittiness, scratchiness, and mixtures of smoothness and roughness also appear very frequently, and evenness and unevenness were named in a few instances, but these two qualities seem to be obscured here by the other more insistent qualities first mentioned.

The experience of smoothness is described as consisting of an areal pressure pattern, essentially cutaneous, in which all the pressure elements are temporally sustained at a uniform level of intensity, have a uniform clearness, and are closely compacted spatially. There are no intervening unstimulated areas or points, nor any areas of lower or higher intensity or clearness, in a genuine smoothness. The experience is at its fullest when the pressure pattern has a low or moderate order of intensity. The degree of compactness of pressure bears a direct ratio to the degree of smoothness. Smoothness is introspectively distinguished from the perception of evenness by the extreme compactness of the adjacent pressure elements and by the higher level of clearness which characterizes the total pattern.

The perception of roughness is constituted of an areal pressure pattern of both cutaneous and subcutaneous elements and is characterized by a lack of uniformity of stimulation of the different elements composing the complex. Certain of the pressure points or subareas have a high order of intensity, others are low or moderate, while distributed among these are intervening unstimulated areas. Moreover, the points of maximal intensity remain so only momentarily, and are succeeded by other points or small areas, which wax and wane in intensity within a brief temporal interval and are succeeded in turn by others presenting the same characteristics. Thus, there are constantly increasing and decreasing fluctuations of intenstiy and corresponding shifts of the point of focal clearness among the various elements constituting the pattern. Z points out that a genuine roughness is not realized unless there are unstimulated points or spots in the pattern. The other *O*s fail to make this distinction, indicating that full-fledged roughness appears when two or more orders of intensity of pressure, which manifest the characteristic changes of intensity and clearness, are represented. Roughness is distinguished from unevenness chiefly by these constant and rapid shifts of intensity and clearness. Like roughness, unevenness is characterized by two or more orders of intensity and clearness, but these remain relatively constant and stable throughout the experience in the latter case.

Movement is reported by Z as being characteristic of the stimulus rather than of the psychological experience. The other *O*s rarely mention movement, and where they do so it is usually in a casual parenthetical remark at the end of the report, to the effect that the stimulus moved to and fro on the finger. The rare reference to movement perhaps points indirectly to the fact that movement is not a psychological datum of the experience; the *O*s were instructed to report in full the psychological characteristics of the experience and should have reported movement at every presentation if it contributed to the psychological experience. Movement is, however, an indispensable condition of the stimulus, since without it neither smoothness nor roughness is realized. In smoothness, movement seems to render more compact the aggregation of pressure elements and to make clearer the uniformly stimulated pressure pattern. In roughness, movement provides the means of stimulating intermittently the different aggregations of pressure which successively undergo sudden changes in intensity and clearness.

Increasing the amount of pressure to 50 and 70 gr. called out no distinctly new qualities, although scratchiness appeared more frequently in these reports. This quality appears to be a type of roughness in which an incipient or actual pain element is added. Smoothness does not appear at its fullest when the pressure of the stimulus is excessive, owing to the presence of a complex of subcutaneous sensations which contribute little if anything to smoothness, and perhaps also to an apparent reduction of cutaneous sensitivity under these conditions. Roughness is not altered so much here, since the "slight pulls" or successive fluctuations of pressure intensities derive in part from subcutaneous organs; at the highest application of pressure, however, there is a certain loss of the configurative detail of the roughness pattern, owing either to a blending of the cutaneous and subcutaneous elements or to a reduction in sensitivity of the cutaneous organs. Grittiness is described as a special kind of roughness, characterized by punctiform points of changing intensity which are separated by very small intervening unstimulated areas.

The *O*s imply through their reports of intermediates that smoothness and roughness represent a single qualitative series. Thus, Oz reports: "Roughness with an element of smoothness"; and to the same stimulus at another time: "smoothness with an element of roughness" (Stimulus 8). For Stimulus 5 she says: "a slightly rough smoothness." Z tended to report certain experiences as half way between roughness and smoothness: "if we regard a straight line as representing the scale of smoothness-roughness series, this experience would stand 40 points from the rough extreme" (Stimulus 8). P reports for Stimulus 6: "that was a smoothness with a slight suggestion of roughness." All *O*s inclined more or less to characterize certain experiences as rough smoothness or smooth roughness.

Since smoothness was designated as essentially cutaneous and roughness as largely cutaneous, we next anaesthetized the tip of the forefinger with ether and presented some of the same stimuli with movement. We found it very difficult to rule out cutaneous sensibility completely, and with one *O* in particular we were never quite able to secure this end. In the more successful cases the presentation of smooth stimuli gave rise to a mere dull diffuse pressure, which apparently derived from subcutaneous organs. There were no reports of smoothness at all. Roughness was not completely ruled out by complete anaesthesia, however, although most of the configurative detail of genuine roughness is lost. We are thus led to the conclusion, already indicated by the *O*s, that smoothness is almost entirely cutaneous, while roughness is both cutaneous and subcutaneous.

SYNTHESIS OF SMOOTHNESS AND ROUGHNESS

Having secured analytical descriptions of these perceptions, we next endeavored to find stimuli that ordinarily are not designated as smooth and rough which might possibly fulfill the requirements for touching off these perceptions. A small portion of the tip of *O*'s finger was exposed through a small hole in a piece of cardboard which covered his finger, and a jet of air from the laboratory air-pressure system was applied to the exposed part of the finger. *O* was blindfolded, his ears stopped with cotton, and a noisy electric motor was set going to prevent his detection by visual or auditory cues that the air system was operating. All *O*s reported smoothness here. Sometimes the smoothness was fused with other qualities, such as softness and coolness, but all described typical experiences of smoothness.

For roughness we used very, very weak faradisation by terminals which, when moving over the skin, gave rise to distinct smoothness. The terminals were either moved over *O*'s finger or *O* was instructed to move his finger over the terminal. Here all *O*s except P reported roughness, Oz and Z at nearly every stimulation. They indicated, however, that the experience was blurred by the sensations of tingle, prickle and tickle. Although P failed to mention roughness, he described pressure sensations having different and changing degrees of intensity.

Professor H. C. McComas also observed in this part of our work and reported, along with other tactual experiences, several instances of slight roughness.

CONCLUSION

Smoothness is a simple tactual perception consisting of a field of cutaneous pressure qualities, all of which possess a low or moderate and uniform intensity, and a relatively uniform clearness, and are so closely aggregated and blended as to give rise to an impression of extreme compactness.

Roughness is a tactual perception constituted of an areal pattern including both cutaneous and subcutaneous pressure elements. This perception is characterized by a lack of uniformity of stimulation. In extreme roughness the pattern lacks spatial compactness, in that intervening unstimulated areas are scattered among the stimulated pressure points or sub-areas. The scattered stimulated parts of the pattern are characterized by several levels of intensity, and are constantly undergoing intensive change, while the points of focal clearness are also constantly shifting among the stimulated parts.

Neither smoothness nor roughness is realized when movement is lacking. In the absence of movement the experiences are labelled evenness and unevenness. Evenness differs from smoothness psychologically by the lack of a high degree of clearness and by the lack of extreme compactness. Unevenness differs from roughness in that the parts of higher intensity of pressure retain their relative prominence throughout the experience, hence it is characterized by stability of stimulation.

Movement is indicated as being a characteristic of the stimulus, and not a psychological datum of the experience. In smoothness, it serves to clarify the pressure elements and to fuse them into an extreme compactness, while in roughness it secures intermittent stimulation of the parts which successively undergo changes of intensity and clearness in brief temporal compass.

AN EXPERIMENTAL STUDY OF THE PERCEPTION OF CLAMMINESS

By M. J. ZIGLER, Princeton University

In setting about the task of analysing and determining the compulsory conditions of the perception of clamminess, we first endeavored to find a stimulus which would give rise to a clammy experience, if possible, as often as it was presented to an *O*. So far as our knowledge goes, the only references to clamminess which appear in the experimental literature are those of Titchener, who in one instance[1] says this perception resolves into cold and soft, where the cold sensations and the pressure elements in the softness are so distributed as to occasion the perception of moisture, and on another occasion[2] testifies that a "horribly clammy feel" is touched off when the finger is set upon a bit of cold boiled potato in the dark. On this authority, we selected as the stimulus for our initial experimental work that mentioned in the latter reference.

(1) The fresh cut surface of a bit of boiled potato was applied to the tip of *O*'s index finger by an apparatus (slightly modified) previously described in connection with another experimental study.[3] The boiled potato was cooled to a temperature considerably below physiological zero, roughly between 15 and 25° C., and cut into small cylindrical pieces of a uniform diameter of 12 mm. One of these cylindrical pieces of potato was pressed into a small inverted metal cup, also of 12 mm. diameter, which was substituted for the metal disc applicator of the old apparatus. This apparatus enabled us to bring the stimulus vertically downward upon *O*'s finger tip, where it was held steadily for 5 sec. and then raised again. *O*'s finger was passive throughout, and there was no movement of the stimulus excepting in the vertical direction at the moments of application and of removal. *O* was blindfolded on entering the room and was also screened from the hand which was stimulated.

The *O*s were Mr. N. H. Crowell (C), graduate student; Mr. M. Meenes (M), assistant in psychology; Miss Janice H. Ozias (Oz), a Smith College graduate, now editorial assistant for the Psychological Review; and Mr. Frank A. Pattie (P), graduate fellow in psychology. Mr. Paul C. Squires (S), graduate student, also observed in Experiments 5 and 6. All *O*s were untrained in tactual observation; but after a brief preliminary training period they all began to use the accepted terminology, and rendered fairly full and accurate descriptions. We then took a total of 50 observations from every *O* by the method of procedure described above; 10 observations constituted a series and only one series was presented at a sitting. The *Os* were instructed: "After the 'ready' signal a stimulus which will set up a tactual perception will be applied to the volar surface of the tip of your forefinger. Observe this perception carefully, and at the end of the experience, name it, if possible, and describe in full all of its sensory characteristics."

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[1] E. B. Titchener, *Textbook of Psychology*, 172.

[2] E. B. Titchener, *A Beginner's Psychology*, 47.

[3] M. J. Zigler, An Experimental Study of the Perception of Stickiness, this JOURNAL, 1923, 34, 76.

All *Os* stated that no single tactual term was comprehensive enough to cover all of the sensory qualities occasioned during the stimulation. We obtained abundant and consistent reports of cold, soft, moist or damp, and quite a number of reports of wetness, stickiness and adhesiveness. But we received very few references to clamminess, a total of 10, all of which were rendered by a single *O*, M. Six of these appeared in the second series, and the other 4 in the third; Series 1, 4 and 5 received not a single mention of clamminess.

(2) Failing by this method of attack, we next introduced movement into the perception in the hope that this condition, so indispensable to some of the touch blends, might supply the necessary basis for arousing clamminess. The same stimulus, a cylindrical piece of cold boiled potato of 12 mm. diam., was carried in the cup of the moving arm of a piece of apparatus described elsewhere.[4] The stimulus made a reciprocating movement, a total excursion of 4.8 cm., and pressed against *O*'s finger with a total pressure of 20 gr. The same instructions were used as in Experiment 1, and 5 similar series of 10 observations per series were taken from every *O*.

The reports of this bit of experimentation bear a number of new tactual qualities, most of them obviously attributable to the introduction of movement; *e.g.*, smoothness, grittiness, greasiness, oiliness, slipperiness, granulation of surface; but the references to clamminess were even rarer than in the initial experiment. Only one *O*, again M, used the term, this time only twice in a total of 50 presentations, and both of these allusions appeared in the same series.

The dozen references by this one *O* in the work so far were not distinctly outspoken and positive declarations of clamminess. In nearly every instance he made remarks such as: "There was a suggestion of clamminess," or "It was perhaps a little clammy;" and several times the allusion was negatively stated, as: "It was not much clammy," or "It was hardly clammy." He did not give a positive statement or implication that any perception was a distinctive and unqualified clamminess. Moreover, the use of the term usually appeared at the end of the report after most of the other characteristics of the experience had been reported.

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[4] M. Meenes and M. J. Zigler, An Experimental Study of the Perceptions Roughness and Smoothness, this JOURNAL, 1923, 34, 542.

(3) For this one *O*, at least, movement of stimulus seemed to make the realization of clamminess less likely than presentation without motion; consequently, movement was eliminated in the next experiment. We now varied the nature of the stimulus, hoping to find a more effective one. We tried in order plasteline, raw clams, oysters, bread-dough, putty, clay, moist absorbent cotton, raw potato and fresh ground meat. In every case the stimulus was cooled to a temperature of between 15 and 25° C., and was presented for two full series of 10 presentations each. In those cases where the stimulus left any residue or odor, which might apprise *O* of its nature, *E* bathed *O*'s finger with absorbent cotton saturated with gasoline at the end of the observation period. The instructions remained unchanged.

This entire period of experimenting failed to yield 'clammy' reports of any consequence. The only *O* to use the term was M, who employed it only two times, both at a single sitting with plasteline as the stimulus.

(4) There were several hints in the reports of this one *O* that an 'object-attitude' was assumed in certain instances where the perception of clamminess was aroused. In one instance, after he had reported all the sensory characteristics, such as cold, soft, moisture or dampness, he remarked: "That was not unlike the finger of a cold clammy hand. It was a cold, clammy, unpleasant experience." This gave us the suggestion that clamminess might be partially conditioned upon a central or imaginal contribution of *O* to certain kinds of stimulus-objects. Acting on this suggestion, we arranged as stimuli for our next experiment (1) a rubber glove filled with cold water, (2) a kid glove filled with moist sand, (3) a kid glove filled with moistened bread crumbs, and (4) a kid glove filled with moistened oatmeal. These were presented in irregular order, a total of 50 presentations for every *O*, by being dropped across the palm and fingers of *O*'s open hand. The instructions remained the same, excepting a slight change to the effect that the stimulus would be applied to the palm as well as to the fingers.

The results of this experiment proved more encouraging. Here all 4 *O*s reported clamminess; Oz 16 times, M 14 times, P 25 times, and C 6 times. Stimuli 3 and 4 were more effective in touching off the perception than 1 and 2, which elicited 5 and 2 'clammy' reports respectively. Clamminess was analyzed into two constituent parts, the one essentially sensory, the other imaginal. The sensory or peripheral component of clamminess breaks up into cold, a yielding or "givy" softness, and the perception of moistness or dampness.[5] Cold and moistness or dampness were indicated as the more obtrusive aspects of the complex, although in the absence of softness, as was practically true in the case of Stimulus 2, we received scarcely any 'clammy' reports. The rubber investment of Stimulus 1 proved inadequate, not so much because it failed to arouse the perception of moisture, as because the *O*s detected that it was rubber; hence the stimulus did not suggest to *O* a distinctly unpleasant stimulus-object, a characteristic seemingly essential to clamminess, as we shall indicate presently. The other 3 stimuli were actually objectively moist on the outside, owing to the wet inner contents of the gloves seeping through.

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[5] Cf. Titchener, *op. cit.* Dampness and moistness are themselves touch blends, not simple touch qualities. See M. Bentley, The Synthetic Experiment, this JOURNAL, 9, 1900, 414ff.

As a general rule the *O*s reported all of the sensory characteristics of the experience first, without mentioning clamminess; the references to clamminess appeared at the end of the report, when *O* at times obviously shifted from the attitude of describing the sensory data to an attitude of characterizing the stimulus-object or the imaginal and emotional data suggested by it. The *O*s stated that the most markedly clammy experiences were those in which the stimulus suggested a distinctly and unexpectedly unpleasant stimulus-object. Moreover, it was only in those instances where the object-attitude was quite evident that an immediate use of the term appeared in the reports. All *O*s asserted definitely or implied that clamminess is always unpleasant and is imaginally associated with a disagreeable stimulus-object. The following excerpts from the reports indicate these several facts.

> "It was a cold clammy thing. I was much surprised and slightly shocked. It felt like the fingers of a hand. I had strong visual associations of dead man's hand, a game played at Hallowe'en parties" (Oz). "That felt like a 5-fingered hand thrown across my own hand. It was a distinctly clammy hand" (M). "Clamminess as of a putty hand" (M). "It felt like a glove. I felt the cold clammy fingers. It was distinctly disagreeable" (P). "That was clammy. It called up a visual image of a crawling snake" (C). "That was clammy like filthy rags" (C).

The unpleasant imaginal component seems positively essential to the experience of clamminess. This is indicated by the fact that the same stimulus, which at one time produced a good clamminess and was designated as disagreeable, is described as merely cold, soft and moist when the unpleasant element drops out. This central component, which renders the experience clammy, was elicited much more frequently in the earlier sittings; towards the end of this experiment both clamminess and unpleasantness disappeared from the reports almost entirely. The 16 'clammy' reports of Oz and the 14 of M all appeared in the first 3 series; the 6 of C in the first series; while the 25 of P were distributed throughout the 5 series. P repeatedly asserted, however, that the clammy experiences in the later series were by no means as distinctly clammy as those of the earlier sittings, and stated that this difference was primarily due to the absence of marked unpleasantness in the later sittings. The other *O*s, all of whom ceased to use the term after a few sittings, indicated that the experiences were no longer unpleasant, implying that some sort of adaptation to these particular stimuli had set in, thereby attesting indirectly and in a negative connection that unpleasantness may be intrinsically bound up with clamminess. The following reports confirm this conclusion, the first one indicating that clamminess drops out during the course of an observation, if unpleasantness is at first realized, and then lost.

"A very disagreeable experience. First a shock of coolness and moistness resulting in a distinct clamminess, but soon there was a reduction of its disagreeable nature. With this reduction of disagreeableness, clamminess was lost, although the coolness, moistness and slight 'giviness' remained throughout" (M). "That was not a good clammy; in other words, it was not at all so unpleasant" (P). "I would hardly call that clammy because it was not much unpleasant" (P). "To be a good clammy there must be an unexpected element of unpleasantness, which usually gives me a sort of creepy feeling" (Oz).

The increased familiarity with the nature of the stimuli probably accounts for the paucity of 'clammy' reports in the later series of observations. One *O* states that when she finds the stimulus to accord with her anticipation of it, or when she takes an attitude which prepares her for the coming stimulation, the clamminess is altered and reduced, apparently through some inhibition of the central imaginal component.

She says, for example: "That one did not seem as clammy as the previous one, but I think it was not due to a difference in sensation, but to a difference in attitude. I was somewhat prepared for this one and it was as I expected it to be" (Oz). "The sensation this time was much like the previous one, yet I did not seem to experience clamminess this time. I think clamminess depends much on the attitude. This experience was familiar, so there was no surprise or shock or novelty" (Oz).

The following report indicates that clamminess does not belong exclusively to determinate kinds of stimulus-objects; that is, it may be realized when an unpleasant suggestion accrues to the sensory data of cold, soft and moist, the stimulus-object to which these characteristics belong being quite unknown.

"Clamminess is based on cold, moistness, and yielding softness, the perception of form and the meaning of not knowing just what it is. If I had my eyes open and could see, I feel that I might not call it clammy" (Oz).

It is not justifiable to assume that this central factor constitutes the whole or all-important factor of the perception, for in that event the reports should distribute themselves uniformly among all 4 of our stimuli. The realization of the perception depends upon the presence both of the sensory data pointed out above and of the central unpleasantness.

Softness. Softness, in so far as our analysis went, was indicated as consisting exclusively of a pattern of pressure qualities, usually of fairly low or moderate intensity, whose extent increases by a gradual spreading simultaneously with a gradual increase of intensity. At a low level of intensity the pressure elements in the softness lack compactness, but at higher intensities of softness the pressure pattern is described as possessing greater compactness. The yielding or "givy" characteristic is ascribed to the gradual temporal integration of changes of extent and of intensity. Softness appears, consequently, to be an integration of 3 attributes of pressure, viz., extent, intensity and duration. Softness also lacks a definite peripheral contour, and sometimes is described as being more compact at the center than at the margin. Softness was synthetized by the application of a jet of air under steady pressure to the tip of *O*'s finger. This was reported by all *O*s as giving rise to typical softness. Yielding softness is aroused by running water from a faucet if the finger is held across the mouth of the faucet.

(5) By this time we were fairly convinced that this central imaginal component is one of the compulsory conditions underlying the perception of clamminess. However, we altered our type of stimuli and instructions somewhat in another series of experiments in order to make certain, now that all *O*s had become acquainted with the clammy experience, whether or not this perception could be evoked under instructions which set up a strict attitude of observing the purely sensory qualities of the experience. We presented moistened bread crumbs or oatmeal in a receptacle to the tips of *O*'s fingers, which extended over the edge of a table. We thus hoped to eliminate the perception of a hand form, which resulted from the stimuli of the last experiment, while leaving the nature of the substance practically unchanged. The instructions read: "After a 'ready' signal a stimulus which will set up a tactual perception will be applied to your finger tips. Observe carefully all touch qualities that are aroused on the fingers and at the end of the stimulation report them in the order of their appearance."

Here, amid a great abundance of reports of cold, moisture or dampness, and a "givy" or yielding pressure or softness—the sensory components of clamminess—we obtained a total of only 5 'clammy' reports, all of which were given by one *O*, P, at a single sitting. In every instance the reference to clamminess was delayed; *i.e.*, it appeared only after all of the strictly sensory qualities had been enumerated. Usually it was a casual remark at the end of the report, as: "I guess I would call that clammy," or "It was a bit clammy." It appears, then, that when the *O*s take the strict attitude of observing the sensory qualities of touch very few 'clammy' references enter the reports, even though all of the sensory components of the clammy experience are represented and reported.

(6) In this experiment we undertook to extend our study of the central component of the clammy experience. Our *O*s had stated that, unless unpleasantness accrues to the requisite

sensory data, the perception of clamminess is not elicited. We wished to determine whether the perception is evoked by the unpleasantness that is associated with specific stimulus-objects or is aroused in connection with any unpleasantness whatsoever that may be added to the adequate sensory data of clamminess. We prepared a series of 10 brief legends, every one of which was suggestive of a more or less unpleasant experience. These legends referred to a number of different kinds of stimulus-objects. *O* was instructed: "After the 'ready' signal *E* will read a brief legend, at the completion of which a stimulus which will arouse a tactual perception will be applied to the volar surface of the palm and fingers. Observe the experience carefully, and at its termination name it and report in detail upon it." As stimuli we used a man's kid glove, a woman's kid glove, and a closely woven muslin bag about 5 cm. by 11 cm., all of which were filled with moistened oatmeal and kept cool in an ice-box. The legends are given below in the order in which they were read to every *O*.

(1) Mr. Jones hastily opened the tool box of his Ford, and was just in the act of picking up some tools when he observed a snake coiled up in the box.

(2) Mr. Smith returned to his home for dinner, walked up to the chair on which his wife was seated, and was just about to kiss her, when he noticed that she was dead.

(3) The cook was carrying a pail of raw oysters when she tripped and fell and immersed her entire right hand in the contents of the pail.

(4) Bill was always careless at the table and was living true to form when he accidentally dropped his spoonful of mayonnaise dressing on the wrist of the friend beside him.

(5) James is one of those mortals who always want to do things that rather disgust their comrades. Recently he has developed the habit of amusing his friends by allowing snails to crawl about on his open hand.

(6) It was a cold icy morning and John had just carried the cuspidor out of his office to empty it. He lifted off the lid and just as he was about to empty the vessel he slipped and his whole hand was thrust into the cuspidor contents.

(7) Mr. S. stopped at a little village to spend the night and inquired at the only inn in town for accommodations. The innkeeper told him every room was taken, but Mr. S. insisted that he must have a place to sleep. Finally the innkeeper stated that one room had only one occupant and that he might sleep with this man provided he promised not to make a light in the room. Mr. S. agreed and retired in the dark. The next morning when he awoke he was horrified to discover that he had slept all night with a corpse.

(8) John came home at noon, found his wife lying on the bed, took hold of her hand, and found that she was dead.

(9) Two strangers occupied the same bed one night because of limited quarters. The next morning one man awoke, and picked up a small round object from the bed. The other interrupted hastily: "Here, give me my glass eye."

(10) Two roommates were always playing pranks on one another. Once upon a time the one decided to leave college late at night before the other had come in without notifying him that he was leaving. But before leaving he brought in a small statue and placed it in the double bed which

the two boys occupied in common. Eventually the other boy came in and retired and was much disconcerted when he discovered that his sleeping mate was lifeless.

The muslin bag was used in every case after the reading of Legends 1, 3, 4, 8 and 9; the two kid gloves were interchanged irregularly after the reading of the other 5 legends.

The following Table indicates the number of times and in connection with what legends the *O*s reported clamminess.

TABLE I

O	Legend	1	2	3	4	5	6	7	8	9	10
C		*	*	*	*		*	*	*		*
M			*		*	*	*	*	*		*
Oz			*					*	*		
P		*	*	*			*	*	*		*
S		*	*					*	*		

It appears that rather specific kinds of associative connections predispose all *O*s to realize clamminess. Thus, in the allusions to corpses in Legends 2, 7, and 8 every *O* reported clamminess; and in these particular reports every *O* to a greater or less extent expressed an immediate judgment that the experience was distinct clamminess. All *O*s stated, moreover, that they associate clamminess with corpses. Two *O*s, who had been service men and had had unpleasant experiences with corpses, expressed very pronounced associations in connection with these distinctly clammy experiences. But one of them, S, although the same stimuli were used throughout, reported clamminess only one other time than in these three corpse-connections,—that of the snake legend. Legends 1, 6 and 10 were capable of suggesting clamminess moderately consistently, but these references were neither so immediate nor so positive as those pertaining to corpses. In the case of Legend 9, where no 'clammy' reports were given by any *O*, and of 3, 4, and 5, where only occasional 'clammy' reports occur, there was a marked tendency for *O* to assume a 'critical attitude' to the effect that the nature of the stimulus was too discordant with the suggestions of the legend to allow those suggestions to carry over into the perception; hence, if clamminess was mentioned, it was usually in a rather negative connection, as: "That was not much clammy," or "That could hardly be called clammy." In several instances, the stimulus and the legend-suggestion were rather inharmonious, but in spite of this fact *O* failed to shift to this 'critical attitude,' and therefore allowed the suggestions of the legend to carry over into the perception. In such cases the *O*s reported, instead of clamminess, such perceptions as oiliness for Legend 4 and sliminess for Legends 1, 3 and 6.

It should be noted that the perception of definiteness and appropriateness of form is not absolutely essential to the realization of clamminess, since the muslin bag was as effective in securing 'clammy' reports in connection with Legend 8 as the kid glove was in connection with Legends 2 and 7. However, the muslin bag was here perceived as the hand of a corpse. This is perhaps partially due to the fact that the muslin bag aroused the essential sensory qualities of clamminess in connection with a legend possessing strong associative suggestions, and partially to the obtuseness of form-perception by touch when the member remains passive.

(7) In our final work we put a large number of men through the legend-experiment in order to ascertain whether or not the statements of our several *O*s regarding clamminess hold generally. With the exceptions of Professor H. C. McComas and Dr. R. A. Brotemarkle, both members of the teaching staff and both ignorant that the problem was in progress, and Mr. S. C. Dodd, a non-resident graduate student, also ignorant of the problem, these consisted of undergraduates, most of whom had had one or two courses in psychology. The latter were untrained *O*s, only 18 having had as much as the elementary laboratory course. Altogether exactly 100 *O*s served in this experiment. These were divided into two groups of 50 each. Every one of the first 50 gave two observation periods, which as a rule were separated by 2 or 3 days. At the first sitting, we took a series of 10 observations from every *O*, using the same instructions that we employed previously in Experiment 1. At the second period, we used the same instructions and presented the legends in the same order as given above in Experiment 6. The same stimuli—those of Experiment 6—were used at both sittings. At the end of the sittings *E* asked *O* whether the stimuli were 'pleasant, unpleasant or indifferent,' unless *O* had previously volunteered the answer to this question in his report.

In the first series, only 11 of the 50 *O*s reported clamminess. They gave a total of 23 'clammy' references. Five of these men used the term only once, most of them at the first presentation, when a shock of surprise and disagreeableness was aroused. All of these *O*s stated that the stimulus was unpleasant, and particularly so in those instances where clamminess was realized. The 41 *O*s who failed to get clamminess almost invariably reported the stimuli as pleasant or indifferent.

In the second series, with identical stimuli, 35 of the 50 men reported clamminess a total of 148 times, 15 men failing to use the term at all. The influence of the legends is patent, both in the greater total of 'clammy' reports and in the greater number of men reporting the experience. Moreover, in Series 1 the 11 men reporting it averaged only 2 experiences each, while here the

35 *Os* average a little above 4 experiences per series. As appears in the Table below, certain legends seemed distinctly more effective in calling out clamminess than others, and the results confirm our findings in Experiment 6 in a rather clean-cut fashion. The percentages in the Table are calculated from the 35 men reporting the experience, not from the total of 50.

TABLE 2

Legends	1	2	3	4	5	6	7	8	9	10
No. 'clammy' references	15	27	9	6	6	5	27	33	2	18
Percent.	42.8	77.1	25.7	17.1	17.1	14.2	77.1	94.2	5.7	51.4

The 15 men who failed to report clamminess at all were again asked at the end of the second period whether the experiences were 'pleasant, unpleasant or indifferent.' Nine stated that the experiences were indifferent or not at all unpleasant, and 2 that they were rather pleasant. When asked what influence the legends had, all except two of these men stated in substance that they were paying attention to the touch qualities and regarded the legends as irrelevant or perhaps as attempts of *E* to test their ease of distraction. The other two took a positive set against the legends. One said: "I knew you wanted me to let the legends suggest to see how much I would imagine," and the other: "I forced myself against the influence of the legends and did not pay much attention to them." The other 4 men found the experiences unpleasant, but were unable to find the appropriate word; when *E* finally suggested clamminess to them, 3 asserted that it was the word they wanted, but being a rare word in their vocabulary, they were unable to get it; the fourth man did not know the word at all.

Excepting 3 *Os* who use clamminess as synonymous with dampness and moisture, all *Os* who employed the term stated that it is distinctly unpleasant and has fairly definite associative connotations.

The second group of 50 gave only one period to this experiment, the conditions of which were identical with the second sitting of the former group of 50. We thought it possible that clamminess, being a term of rather infrequent usage, might have been reported more frequently at the second sitting because *O* had, perhaps, reflected during the interim upon the previous experiences and searched for a more fitting term than he was able to find at the first sitting, and therefore came back with the word 'clamminess' in mind. But this supposition is invalidated by the fact that in this experiment 38 of the 50 *Os* use the term a total of 156 times, a larger number of reports than the other 50 *Os* gave at their second sitting. The greater number of 'clammy' references and the greater number of *Os* using the term here may be purely a matter of chance, or it may be due to the fact that a few *Os* in the former work might have used the term at the second sitting excepting for the fact that they recognized the stimuli at the second sitting to be quite similar to those of the previous one, and so took the attitude of reporting the same qualities that they used in the earlier sitting. One or two *Os* gave evidence of trying to recall the qualities they had used the previous hour.

The following Table, which embodies the results of this latter work, indicates again that certain of the legends touch off considerably more clammy experiences than others. The percentages again are based on the 38 men reporting the experience and not upon the total 50.

TABLE 3

Legend	1	2	3	4	5	6	7	8	9	10
No. 'clammy' references	12	29	7	5	10	6	30	34	5	18
Percent.	31.5	76.3	18.4	13.1	26.3	15.7	78.9	89.4	13.1	47.3

Here again Legend 8 receives the highest number of 'clammy' reports. This is perhaps due to the fact that this legend refers explicitly to taking hold of the hand of the corpse, while in Legends 2 and 7 the idea of touching the corpse is merely implicit.

Of the 12 *Os* who did not report clamminess here, 8 took an "objectively critical" attitude and found that the stimulus failed to fulfill the suggestions of the legend. These *Os* found the stimulus too cold, too soft, too wet, not hard or stiff enough, etc. The other 4 blocked off the suggestions of the legends and reported the purely sensory or peripheral qualities due to the assumption of a set against the influence of the legends.

Sliminess. Although we have made neither any attempt to touch off slimy perceptions nor any direct attack upon the analysis of sliminess, there are indications in the reports that this perception too may depend in part upon a central emotional factor. Thus the 50 *Os* in the non-legend observations reported sliminess only 10 times, while in the legend series they report it a total of 55 times. The other 50 *Os* gave a total of 54 'slimy' references. Moreover, there is a fairly definite distribution of the larger number of these reports with the legends that most naturally suggest sliminess. It should be pointed out that the conditions of our work were not particularly favorable to the realization of this perception, since there was no movement of any consequence either of member or of stimulus. But certainly, unless some of the sensory data were represented to which this central factor was added, there was no perception of sliminess. The following Table shows the distribution of the 109 reports of sliminess among the different legends.

TABLE 4

Legend	1	2	3	4	5	6	7	8	9	10
Group 1	10	3	7	3	12	10	3	1	4	2
Group 2	8	2	8	1	14	12	4	2	2	1

The perception of *oiliness* was mentioned 5 times in connection with the mayonnaise legend, but did not appear at all in connection with any of the others. *Slipperiness* and *greasiness* were reported a number of times but show no definite connection with the suggestions of any particular legend.

CONCLUSION

Clamminess is a complex tactual perception, which breaks up under introspective analysis into (1) a sensory or peripheral part and (2) a central or imaginal constituent. The sensory or peripheral part analyses further into sensations of cold and a yielding softness, which must be so combined as to give rise to the simple sensory perception of moisture. Cold is a simple sense quality, but softness appears to be a simple perceptual integration of the intensive, extensive and durative attributes of the pressure quality. The central or imaginal component consists of an unpleasantness, which attaches to associative connotations of rather definite kinds of stimulus-objects. A genuine clammy experience is always designated as distinctly unpleasant.

The perception of clamminess fails to mature, either in the absence of one of the constituent sensory data and when these in combination fall short of setting up the perception of moisture, or in the absence of the central or imaginal factor when all the sensory data fulfill the requirements of that part of the perception. The assumption of an 'objectively critical' or of a 'strictly sensory' attitude renders unlikely the arousal of the perception, since such an attitude blocks out the central constituent; but the assumption of an 'object'-attitude favors the touching off of the experience, owing to the fact that such an attitude facilitates the accrual of the central or imaginal component to the requisite sensory data.

REACTION TIMES AND THE CONSCIOUSNESS OF DECEPTION[1]

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HISTORICAL SETTING

Both Jung (4, 5, 6) and Wertheimer (10, 11) claim the discovery of the association-reaction method of studying mental processes. The method consists essentially of giving to the subject a list of words with the instruction that, as the list of words is presented to him, one by one, he is to answer as quickly as possible by the first word that occurs to his mind in connection with the seen or heard stimulus-word, and of measuring the reaction time to each word. If any of these reaction times is unusually long, such a lengthened reaction is taken to indicate that some emotional factor is at work in addition to the association. The primary interest of the early workers in this field lay in the diagnosis of the emotional complexes underlying cases of hysteria and neurasthenia. The method was, however, also applied by Jung and Wertheimer in attempts to detect crime. In this use of the association-reaction method, the list of words used as stimuli contains words relating to the crime. The reaction time to these critical words was found to be lengthened in the case of those criminals who attempted deception by avoiding responses which implicated them in the crime.

In 1909 Yerkes and Berry (12) and Henke and Eddy (3) published experimental studies which were designed to test the applicability of the association-reaction method to the detection of crime. In these experiments, *S* was allowed privately to open one of two boxes, and *E* endeavored by means of a series of association-reactions to determine which box had been opened. These investigators found that reaction times tended to be longer whenever deception was practised. Leach and Washburn (7) published a careful qualitative and quantitative study of the method in 1910. They used a similar procedure and tried to test the method on as many *Ss* as possible. Their quantitative results are in agreement with those of the previous investigations, namely, that during a deceptive situation reaction times are increased. We are not concerned here with the qualitative aspects of the results obtained, that is, with the character of the words with which *S* responds, although the words are equally important with the reaction times in the detection of crime.

Marston (9) published in 1920 an account of a study in which he attempted to test the thesis that reaction times are increased during deception. His method purposed the elimination of the qualitative aspect of the association-reaction experiment, the substitution for this aspect of arithmetical exercises, and the isolation of the quantitative measurements. It aimed at the exclusion of all mental elements extraneous to the interaction of deceptive consciousness and reaction times. The results of his study disclosed two distinct types of reaction time behavior during deception. First, the "positive," clearly evident in 4 out of 10 *Ss*, and previously believed to be practically universal, was characterized by increased length of reaction times during deception. Secondly, a "negative" type manifested itself with equal distinctness in 3 *Ss*, in whose cases the reaction times appeared to be consistently shortened during deception. In addition there was a mixed group of 4 *Ss* who completely shifted from positive to negative on different days, but behaved consistently within the positive or negative type in each single day's work. The introspective reports of the positive-type subjects show fear, excitement, and confusion of motor impulses during deception, all of which were lacking in the experiences of the subject of the negative type. From these experiments, Marston concluded that increased reaction times, as well as other symptoms of emotional excitement, can be taken as indications of deception only in those persons who have been shown to be of the positive type, while decreased times and lack of symptoms of emotional excitement can be taken as indications of deception in persons of the negative type.

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[1] From the Psychological Laboratory of the University of Pittsburgh (communicated by Jesse H. White and Gilbert J. Rich). The experimental work was performed in the spring of 1921.

Experiment I

Problem.—Marston's results are at variance with the findings of all workers in the psychology of deception previous to him. His conclusion would lead to the belief that the thesis, reaction times are increased during deception, is fallacious. A consideration of his work brings to the front the following questions: Do the conditions of Marston's experiment actually disclose two distinct types of reaction-time behavior, a "positive" type characterized by increased reaction time whenever there is an attempt to deceive, and a "negative" type characterized by decreased reaction times during deception? Was Marston's method adequate for the production of a *deceptive* situation in which the quantitative factors were isolated by elimination of the qualitative aspect? Experiment I. aims to answer these questions.

Observers.—Ten *Os* took part in the experiment. One (S) was an Instructor in the Department of Psychology, a man with his doctor's degree and highly practised in the performance of arithmetical exercises. One (L) was an undergraduate student of psychology without laboratory experience. The remaining eight, six women and two men, were students in the second semester of the laboratory course in psychology.

Apparatus and Procedure.—This experiment was necessarily a repetition of Marston's work. So far as his published account enabled us to do so, every detail of materials, conditions and procedure of Marston's study was duplicated.[1]

320 pasteboard index cards, 8 by 5 in., were prepared, every one of which carried two parallel columns of 10 two-digit numbers each. Every number column or list was repeated 4 times, every time with a different digit to subtract or add, in order to exclude as far as possible any chance influence of combinations of figures peculiarly difficult to any individual *O*. These cards were divided into 40 sets of 8 cards each. The cards were presented to the *O* in groups of 8 each, blank side up. The following instructions were given.

"You will be given a group of eight cards, upon each of which are written two parallel columns of ten two-digit numbers each. These cards will be placed blank side up before you. At the signal 'ready' you are to turn over the top card and at the signal 'go' you are to add to or subtract a given number from each number in one of the lists, as directed, giving the answer as fast as you can. Before the giving of the 'ready' signal you will be directed to choose either the right or the left column and to proceed through that column either up or down, adding or subtracting from each number in the list 2, 3, 4, or 5. In any four out of each group of eight cards you are to reverse the instructions by taking the opposite list, by proceeding in the opposite direction, and by adding when instructed to subtract and *vice versa*. You are to deceive me as to the identity of the cards with which you reversed the instructions by proceeding to make your calculations with the list on these cards as rapidly as on the cards with which you obeyed the instructions.

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[1] In a conversation following the completion of this portion of the work, Marston indicated that his experiments were performed in the presence of a 'jury.' Since he gave no indication of the presence of a 'jury' in his printed account, this condition was not duplicated by us.

"At the completion of each card you are to enter in the table provided by the symbols C (correct) and O (opposite) whether you obeyed the instructions or disobeyed them.

"You will then characterize with as much detail as possible your attitude when obeying directions and your attitude when disobeying directions."

Reaction times were taken with a stop-watch showing fifths of a sec. Time was allowed between the instructions and the starting signal for the *O* to perform the central (mental) process of reversing the instructions, and the watch was started simultaneously with pronouncement by the *O* of his first result, so that the reaction times recorded actually included the time used in 9 instead of 10 additions or subtractions.

The instructions for every *O* and with every set of cards continued as follows.

"Take the column to the right; proceed up; add 2.
Take the column to the left; proceed down; subtract 3.
Take the column to the right; proceed up; add 4.
Take the column to the left; proceed down; subtract 5.
Take the column to the left; proceed up; subtract 2.
Take the column to the right; proceed down; add 3.
Take the column to the left; proceed up; subtract 4.
Take the column to the right; proceed down; add 5."

At the completion of every group of 8 cards, *E* attempted to judge by means of the reaction times alone which 4 lists the *O*s had performed according to instructions (C) and which 4 they had performed in the way opposite to the directions (O).[3]

The series of 40 sets of 8 cards each were used with every *O* except L. In L's case, 47 sets were used. The amount of work done in a single experimental period consisted of from 3 to 16 sets. An experimental period varied from one to two hours in length.

Results.—The average reaction times for each *O* revealed two types of response under the experimental situation. One of these was analogous to Marston's positive type, and was characterized by longer reaction times for the cards with which the instructions were disobeyed; the second type was analogous to Marston's negative type, and was characterized by shorter reaction times for the cards with which the instructions were disobeyed. Three *O*s were of the positive type, 7 of the negative type. The significance of these types is to be understood only by analysis of the numerical and introspective data for the separate *O*s.

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[3] The percentages of correct judgments by *E* varied so little from 50% that their significance is negligible and they are omitted here. They are reactions of *E*, not of the *O*s, and are, therefore, irrelevant to our problem. Their percentages may be found in a bound manuscript with the title "Reaction Times During Deception" which is in the University of Pittsburgh Library and may be borrowed under the usual courtesies of inter-library exchange.

Table I. shows the average reaction times of the three positively inclined *O*s to the cards with which they obeyed instructions ("C" reaction times); their average reaction times to the cards with which they disobeyed instructions ("O" reaction times); the difference between these two averages; the probable error of this difference; and the probable correctness of the difference, computed according to the formula suggested by Boring (1).

TABLE I

Observer	Average "C" Reaction Times	Average "O" Reaction Times	Difference between "O" and "C" Averages	Probable Error of Difference	Probable Correctness of Difference
E	6.4095	6.4412	.0317	.2671	.5550
H	8.6550	9.0312	.3762	.1572	.9125
Mc	8.3175	8.3812	1.0637	.1409	.6150

An analysis of the introspections[4] of these three *O*s shows that they were actually conscious of deceiving whenever they disobeyed instructions. The consciousness of deception appeared in various guises.

(1) It made the *O*s feel guilty when disobeying directions.

E, 1. "When I disobeyed I seemed to feel that it wasn't right."

H, 4. "I realize all the time I am disobeying that I am disobeying. There is always a strain or tension of my whole body while disobeying."

Mc, 4. "At first when I decided to disobey instructions I felt like a liar."

(2) It made them self-conscious when disobeying directions.

E, 1. "I felt more self-conscious when I disobeyed than when I obeyed."

E, 2. "This time, when I disobeyed instructions, I seemed to hesitate. This hesitation was caused by self-consciousness, because when obeying I felt that I wasn't deceiving the experimenter."

H, 2. "This time I seemed doubly conscious that I was disobeying."

(3) It was harder to disobey.

E, 1. "It was harder to disobey instructions."

H, 2. "As usual it is much harder for me to disobey."

H, 4. "It is much easier to obey."

(4) It made them hesitate when reversing instructions.

E, 1. "I hesitated longer when I disobeyed than when I obeyed the instructions. When I disobeyed I seemed to feel that it wasn't right and this led to a hesitancy."

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[4] Each quotation from an introspective report is prefaced by the initial of the *O* and by the day upon which the report is made, as 1 for the first day of experimentation, 2 for second day of experimentation, etc.

E, 2. "This time when I obeyed instructions I seemed to hesitate. This hesitation was caused by self-consciousness, because when obeying I felt that I wasn't deceiving the experimenter."

H, 1. "This time I hesitated and stopped at intervals when disobeying and was more conscious of the fact that I was disobeying, and was more excited. I didn't go so smoothly as before."

(5) It caused strain and tension when disobeying instructions.

H, 4. "There is always a strain or tension of my whole body while disobeying, for when the command is given it means to reverse the method on a second's notice."

Mc, 2. "I have a greater feeling of strain when I disobey."

(6) It caused a feeling of relaxation and relief when obeying.

H, 4. "It is much easier to obey. I feel relaxed and would rather disobey the first four times and then be able to relax while obeying."

Mc, 1. "I felt almost relaxed when I was obeying."

Mc, 4. "After I have decided to obey or disobey the instructions, I have a feeling of relaxation and pleasantness."

(7) It caused excitement when disobeying the instructions.

H, 1. "When I know that I am going to obey I feel relaxed and relieved and I go with much more speed."

H, 2. "I get excited and must concentrate whenever I disobey."

M, 1. "I felt more excited while disobeying than while obeying."

M, 4. "At first when I decided to disobey instructions I felt like a liar. I had an excited, unpleasant feeling."

The consciousness of deceiving was evident, even when the *Os* attempted to devise a plan whereby to make equal the reaction times for the C and O cards.

H, 2. "By trying to mix my methods so as to change, I do get excited and must concentrate. However, I have a fixed time when I shall disobey, but as usual it is much harder for me to disobey. It is much easier to add than to subtract; so in disobeying, I tried to reverse the method and to add. But in so doing, I hesitated."

H, 4. "My plan when directions are given is to trick the experimenter· I reverse and change in order that my method may be different with each set of cards, so that the experimenter may not know when I am obeying or disobeying. I found myself hesitating while disobeying; so, in order that the experimenter might not learn my method, I hesitated at times when obeying. Even then I realize that I am disobeying. It is much easier to obey."

The consciousness of deception bears a definite relation to the probable correctness of the difference between the "O" and the "C" reaction times. The more acutely conscious the *O* was of deceiving, as shown by his introspective reports, the more probably correct was the positive difference between the times of the two forms of response. Thus, on the basis of the degree to which they were conscious of deceiving, the three *Os* would be ranked in the order: H, Mc, E. They stand in the same order with respect to the probable correctness of the difference in reaction times.

The quantitative data for the 7 negatively inclined *O*s are shown in Table II. The introspections of these *O*s reveal that they were not conscious of deceiving.

A, 3. "There is no difference in my consciousness now, so far as I can judge, when I obey or disobey instructions."

B, 2. "I did not feel any difference between obeying and disobeying."

D, 3. "After I have started, the nature of my consciousness is not affected by my decision to obey or disobey."

D, 6. "My mental attitude when I obey is no different from what it is when I disobey."

L, 1. "I have no special feeling attitude either way."

L, 4. "Whether or not I obey does not affect my consciousness, so far as I know."

M, 1. "I do not feel affected by the decision to obey."

R, 5. "As a rule, the decision one way or the other did not affect the nature of consciousness."

S, 2. "There is no difference in my mental attitude between the times when I obey and when I disobey. I simply decide upon what I shall do and then do it."

It will be remembered that the actual reaction times recorded included only 9 additions or subtractions, and did not include the time for the central (mental) process of reversing the instructions. The central process took place between the 'ready' and the 'go' signals. Nor was the time for the first calculation by the *O* included in the reaction time recorded. It was during this time, if at all, that the decision to obey or disobey had any affect upon the *O*s. The decisions once made, however, the experiment became simply a process of performing some arithmetical exercises.

TABLE II

Observer	Average "C" Reaction Times	Average "O" Reaction Times	Difference between "C" and "O" Averages	Probable Error of Difference	Probable Correctness of Difference
A	11.9750	11.0775	.0975	.1800	.6350
B	13.7675	12.4862	1.2813	.2420	1.0000
D	15.3862	14.9662	.4200	.3331	.8000
L	11.4361	10.5989	.8372	.2376	.9875
M	13.5556	12.6557	.8781	.1890	1.0000
R	9.9925	9.9175	.0750	.1983	.5575
S	5.9437	5.8750	.0687	.0739	.7300

B, 4. "Once I had decided whether I should obey or disobey, I tried to get through as quickly as possible. Which way I had decided did not affect me."

B, 5. "I disobey and obey according to some arrangement of how the C's and O's will appear in a list, varying the arrangement from time to time. I decide to obey or disobey before I turn over the card. In some instances, however, I changed my mind after turning the card over. This change was due to the order in which the numbers appeared. Sometimes after seeing the numbers it seems as though it would be easier to disobey than to obey and then I did that. My consciousness is not affected after I have decided what to do."

D, 3. "I decide to obey or disobey sometimes in a regular order before I begin the set of cards. I always decide before I hear the instructions. If I decide to disobey instructions, I am under somewhat of a strain until I am sure that I have reversed the directions correctly. After I have started the nature of my consciousness is not affected by my decision to obey or disobey."

L, 4. "I decide to disobey instructions always just after hearing the process named. Whether or not I obey does not affect my consciousness."

R, 2. "In deciding, there is some confusion of consciousness; the two sides seem to be contending for themselves. I feel more certain and satisfied when I have reached a decision."

R, 3. "When making a decision there was much confusion in consciousness, and it was usually necessary to exert some effort in order to reach a decision and to remember it so as not to get mixed up. When a decision was once reached, I seldom really got it mixed up, but had to concentrate on it. As a rule, the decision one way or the other did not affect the nature of consciousness."

All the *O*s reported a plan, made for the purpose of equalizing the reaction times on all cards. The plan, in general, consisted in setting a medium pace at which to call off the calculations for both the C and the O cards; in going slower on the C cards; and in arranging the order C's and O's in such a way as to be able to disobey with the easier process.

A, 1. "I hurried on the opposite cards and went slower on the correct ones."

B, 3. "I tried always to add when disobeying so as to go faster. I tried to increase the speed when disobeying."

B, 4. "When disobeying, I nearly always added a 2, because I found that it was easier to do so and that adding 2 went faster. I tried to disobey whenever the instructions were to add or subtract 5."

D, 3. "I prefer to begin obeying, because the fuss of getting started prevents my disobeying on the first set. I like to get the four times I must disobey off my mind as soon as possible."

L, 1. "I consciously selected all requests to subtract for the cards on which I should disobey."

L, 2. "I try to pick the easiest alternative and keep within the rules."

L, 3. "I prefer to add anything over 3."

L, 4. "It seems easier to take four C's and then four O's."

R, 1. "By making an extra effort at speed when disobeying and hurrying less when I obeyed, I hoped to make the times for all cards as nearly equal as possible."

S, 4. "I usually choose to disobey the more difficult instructions and to obey the easier instructions."

The *O*s felt no strain and tension when disobeying, and did not experience a feeling of relief and relaxation when obeying. Nor did they feel any excitement when disobeying.

B, 3. "I felt more confidence when disobeying and tried to gain speed from that fact."

D, 4. "When I put out of my consciousness the idea of obeying or disobeying and of deceiving the experimenter, and simply performed the process as I had decided in my own mind, I found it easier for me and less revealing to the experimenter. That is, I get rid of the ethical feeling and work on a mechanical basis."

D, 6. "The process is entirely mechanical, involving not the least feeling of moral obligation."

Thus, in the case of the 7 *Os*, whose quantitative measurements showed a decrease in reaction times for cards on which they reversed instructions, there was no consciousness of deception. The decision to obey or disobey was made either before or after the directions how to proceed were given; and if this decision did affect the consciousness of the individual *O*, it did so for only one out of the ten operations; and the time for this operation was not included in the reaction times recorded. Therefore, there was no means to determine whether the desicion to obey or disobey caused the reaction time for the first calculation to be decreased or increased. But, usually, once the decision was made, all idea of obeying and disobeying was dropped, and the performance of the arithmetical exercises became merely a matter of adding and subtracting. They felt no strain or tension and excitement when disobeying; nor did they feel any relief and relaxation when obeying. It made no difference to them whether they obeyed or disobeyed.

Further analysis of the quantitative results discloses the fact that every *O*, whether positive or negative, had at least one reversal day, that is, one day or more on which he completely shifted from his ordinary attitude. Thus, every positive *O* had at least one day on which he was negative, and every negative *O* had at least one day on which he was positive. When these reversals were checked up with the introspections of the *O* on reversal days, it was found that on negative reversal days of positive *Os* the introspections were similar to those of the negative type, and that on positive reversal days of negative *Os* the introspections were similar to those of the positive type.

TABLE III

Observer	Number of Sets a Day	Average "C" Reaction Times	Average "O" Reaction Times	Difference between "O" and "C" Averages	Probable Error of Difference	Probable Correctness of Difference
E	6	8.0250	7.2000	− .8250	.1944	1.0000
	6	7.2333	6.7166	− .5167	.1903	.9700
	16	6.2593	6.3625	+ .1032	.1014	.7500
	12	5.6750	6.2625	+ .3566	.1181	.9800
H	4	11.4750	11.3500	− .1250	.6480	.5700
	12	9.7958	10.4000	+ .6042	.1596	.9900
	12	7.6666	7.8250	+ .1584	.1937	.7085
	8	7.6375	7.8000	+ .1625	.2414	.6875
	4	7.4750	8.6375	+1.1625	.4181	.9700
Mc	8	10.2812	10.8750	+ .5938	.2810	.8500
	8	8.4375	8.3000	− .1375	.2246	.6500
	8	8.2000	7.7687	− .4313	.2454	.8800
	16	7.3500	7.4812	+ .1312	.1398	.7400

In Table III.[5] are shown the data for the three positive *O*s fractionated by periods of experimentation (which occurred on separate days). It will be noted from this Table that the first and second periods of experimentation were reversal days for E, that is, days upon which the reaction times were decreased for the cards with which she disobeyed instructions; and that the second and third days were reversal days for Mc. Thus, the two *O*s, for whom the probability that the positive difference was not due to chance was slight, were actually positive in only half of the experimental periods. Moreover, the probability that the positiveness or negativeness of each of these *O*s on any given day was not due to chance was in most cases relatively high. On the other hand, H, who was markedly positive, had only one reversal day (the first period of experimentation), and in this case the probable correctness of the negative difference was so low as to show that chance factors were operative,[6] and indicates that tendencies in both directions were operative during that period. This conclusion is borne out by the fact that of the 4 sets of cards completed during the experimental hour, 2 were negative and 2 were positive. The introspective reports show a difference in attitude between the two types of reaction. The introspection for the positive set was as follows.

H, 1. "When I know that I am going to obey, I feel relieved and relaxed and I go with much more speed. In disobeying, it is harder to get adjusted and to concentrate and I am much slower in my responses. I hesitated and stopped at intervals when disobeying and was more conscious of the fact and was more excited. It didn't go smoothly. There was more of a strain this time."

Whereas the introspection for the negative set was as follows:

H, 1. "This time, although I hesitated in subtracting, I have no difficulty in disobeying. In fact, it seemed just as easy to disobey as to obey, compared with the previous time." (The previous time, the set was positive.)

A few excerpts from the introspections of the two other *O*s on the reversal days will show that, in general, their consciousness while disobeying was similar to that of the 7 *O*s of the negative group.

E, 1. "During this experiment, I obeyed or disobeyed according to the instruction. If told to add, I added; but when told to subtract I disobeyed so that I could add. A few times I broke from this general rule just in order to try what the result would be. Heretofore, I believe that I have accelerated when disobeying in order to cover up the fact, but now I try to keep about the same time for all, sometimes trying to go faster when I obey than when I disobey. I perform this work without consciousness of deception. The most interesting and most prominent motive is one of speed. The idea of really trying to deceive does not enter my mind."

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[5] The differences between the "C" and the "O" averages are called positive whenever the "O" times are greater than the "C" times, and negative whenever the "C" times are the greater.

[6] It must be remembered that a difference between averages which is due to chance factors alone would have a probable correctness of .50.

E, 2. "I find that the factor that caused me to obey or disobey instructions most of the time was based upon the fact that I like to add better than to subtract. Therefore, I added every time I could. By disobeying when told to subtract, I added nearly all the time. However, I wouldn't always carry this out; at such times I would decide before the instructions were given. Several times I deliberately decided before the experiment began. I know that I have to obey four times and disobey four times; so I make up my mind as to what I am going to do and I no longer have any feeling of guilt when disobeying. I do not believe that the decision to obey or disobey has any effect upon the time in which one calls off the figures. I think it all depends upon the easiness or difficulty of the column of figures. It also depends upon whether one is interested or not interested in performing the process."

Mc, 3. "I feel relaxed when it is all over. When I finish the series either of obeying or disobeying, I feel about the same degree of pleasantness and relaxation."

Table IV. shows the data for the 7 negative *O*s, fractionated by experimental periods. A few selections from the introspections of this group on their reversal days suffice to show that on these days they were conscious of deception very much as were the positive *O*s, as described above.

A, 2. "The answers came much faster when adding or subtracting 2. Several times I think I made mistakes and knew them, but I didn't correct them. I find always more difficulty in doing subtractions than in doing additions. I tried this time to go very fast, giving as short a space of time before the first number in all eight cases as possible. On the fourth one, I mixed the cards and after the signal was given I had to straighten them. It took longer to react. I rushed through them all wildly and tried to give accurate judgments. I found it easier to make quick reactions because of the method of grouping the C's and O's. I tried going slower on all judgments. I went very fast. I wondered all the time whether I was fooling the experimenter this time, since the last time I had gone clear wrong."

B, 1. "When trying to gain speed when disobeying, I became somewhat confused. I sometimes became confused over the number to be subtracted or added. There was more confusion when disobeying than when obeying. There was less confusion as I became accustomed to adding or subtracting. I found that I could add faster than I could subtract. Trying to increase the speed did not cause so much confusion this time. I became more accustomed to the numbers and the confusion still lessened. When disobeying, I always added as that seemed to go faster. I gained speed because the confusion lessened."

D, 1. "There is increasing difficulty in getting started when I disobey instructions. It is easier for me to add than to subtract. It is easier to add or subtract very small numbers than those a little larger. There is considerable strain when starting in to reverse instructions."

L, 6. "I was altogether worried when making the decision as to obedience. I felt somewhat guilty when disobeying."

M, 2. "I am under a greater strain when disobeying than when obeying, and feel that I am unable to act normally and proceed with the same regularity as when obeying. When the card is completed, there is a slight relaxation, but as new instructions follow quickly, attention and the plan of procedure cause a renewed effort to be made immediately."

TABLE IV

Observer	Number of sets on Day	Average "C" Reaction Times	Average "O" Reaction Times	Difference between "O" and "C" Averages	Probable Error of Difference	Probable Correctness of Difference
A	4	18.1375	16.4125	−1.7250	.7688	.9475
	10	12.0600	12.1150	+ .0550	.3050	.5600
	14	11.6678	11.2607	− .4071	.2063	.9125
	12	10.2125	10.8875	+ .6750	.1749	.9950
B	12	13.6541	14.0166	+ .3625	.4830	.6950
	12	14.2625	12.5833	−1.6742	.3648	1.0000
	16	13.4812	11.2637	−2.2175	.3628	1.0000
D	3	16.2000	18.4833	+2.2833	1.1874	.7500
	9	14.7916	14.7333	− .0583	.4913	.5350
	4	15.9745	13.9875	−1.9870	.7952	.9600
	4	15.1125	14.8500	− .2625	.8416	.5850
	8	16.4000	14.5187	−1.8813	.4396	1.0000
	12	14.8458	14.7166	− .1292	.3693	.5497
L	7	14.7285	12.3357	−2.3928	.6600	.9925
	9	12.6111	12.0308	− .5803	.4073	.8450
	8	12.9062	10.3500	−2.5562	.7270	.9900
	7	9.5428	8.7000	− .8428	.3311	.9575
	8	9.7812	10.2750	+ .4938	.3536	.8300
	8	9.0750	9.7062	+ .6312	.3573	.8900
M	12	14.5000	13.4082	−1.0918	.3584	.9800
	8	13.0281	13.0875	+ .0594	.3498	.6250
	4	14.8750	12.9375	−1.9375	.4397	1.0000
	4	15.9750	13.7125	−2.2625	.6130	.9925
	12	11.7166	11.2416	− .4750	.2684	.9375
R	8	12.8000	11.9187	− .8813	.5965	.8400
	8	8.7625	10.7625	+2.0000	.3510	1.0000
	8	9.2250	8.7312	− .4938	.3507	.8250
	12	9.4208	9.1375	− .2833	.3262	.7200
	4	10.8750	8.9375	−1.9375	.4752	1.0000
S	8	6.6062	6.8375	+ .2313	.1939	.7900
	8	5.8125	5.7312	− .0813	.1486	.6380
	12	6.0000	5.5375	− .4625	.1171	.9900
	12	5.5333	5.6660	+ .1327	.1300	.5600

R, 2. "During the cards with which I obeyed I had but few conflicts. When I changed to disobedience I found somewhat of a predisposition toward obedience. It seemed more of an effort to disobey and I was anxious for the times when I could obey. Obeying came as a relief. I believe I made better time on obedience than on disobedience."

S, 1. "There was a feeling of confusion about remembering whether I am obeying or disobeying instructions. There was a feeling of confusion about getting the right instructions and remembering whether I had determined to obey or disobey."[7]

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[7] In the case of S, the difference in attitude on the reversal day was marked. It must be remembered that he had had considerable experience with the performance of arithmetical exercises and the experiment became for him a mechanical procedure. He was unaware of deception, yet showed a shift of attitude on his reversal days.

Thus a detailed analysis of the introspections reveals the fact that, on the days on which the reaction times for the O-cards are decreased, the positive-type *O*s are not aware of any difference in their attitude when disobeying as compared with their attitude when obeying; whereas on the days on which the reaction times for the O-cards are increased the negative-type *O*s are conscious of deception,—this consciousness of deception appearing in various guises, as a feeling of guilt or as a feeling of strain and tension and excitement when disobeying, and as a feeling of relief and relaxation when obeying.[8]

Conclusions.—Under the conditions of this experiment, two tendencies in reaction time behavior appeared. The one tendency consisted in the reaction times being longer for the cards for which the instructions were reversed. This type of reaction, whether it appeared in the majority of an *O*'s responses or in the responses on a single day, represented a true deception type, for the *O*s reported, when they were reacting in this fashion, consciousness of deception, strain, conflict, and emotional disturbances when disobeying instructions. Following Marston's nomenclature, we have called this the positive type of response.

The other tendency was characterized by shorter reaction times for the cards with which the instructions were reversed. We have called this the negative type. It was not a true deception type. The introspective reports of *O*s responding in this manner showed that they were not conscious of deception, strain, conflict, or emotional disturbances when disobeying instructions. How, then, did this form of response appear during the experiment? The experimental procedure, devised by Marston with the intent to isolate the quantitative phase of deception, simplified the situation to which the *O* reacted to such an extent that all meaning of deception was, in the majority of cases, eliminated from the symbols dealt with in the *O*s' mental processes. The experiment, as a result, became at these times nothing else than practice in arithmetic, into which the factor of deception did not enter. That 7 of the 10 *O*s (and the remaining *O*s on certain days) did not show longer reaction times when disobeying instructions may not be taken to indicate increased reaction times during deception, for a condition of deception was not induced in these cases by the experiment. The so-called negative type is not a type of response to deception, but a type of non-deceptive response to this particular experimental situation.[9]

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[8] An analysis of the relative frequency of positive, negative, and indifferent sets for every *O* for every day's work may be found in the manuscript copy mentioned in Footnote 3.

[9] Marston assumed that his experimental setting induced deception because the instructions were disobeyed. We have cited introspective reports to prove the incorrectness of this interpretation.

If the factor of deception did operate in any way for the *Os* of the negative type, it did so only during the process of deciding to disobey the instructions, of actually making the reversal in the instructions, and (perhaps) of making the first calculation on the card. Thereafter, the mental set induced at the beginning of the card gave the cue to the individual computation. But the time required for making that decision, for beginning its operation, and for making the first calculation was not included in the reaction times measured, and there was, thus, no quantitative measure of the effect of disobedience upon these initial phases of the reaction to the card.

Moreover, the wording of the instructions "to make your calculations with the list of these ["O"] cards as rapidly as on the cards with which you obeyed instructions" implied that the reaction times for the "C" cards would naturally be smaller than the reaction times for the "O" cards; and on the basis of this implication the *Os* formed their plans to make the times equal for all cards. Thus, they invariably slowed down on the "C" cards and hurried on the "O" cards.

To summarize: a repetition of Marston's experiment has failed to give evidence of two types of reaction-time response to deception; but it has shown that the experiment induces deception in some *Os* and at some times, and fails to induce deception in other *Os* and at other times, the failures to produce deception being nore numerous than the successes.

EXPERIMENT II

Problem.—Experiment I. failed to produce, save in a few instances, deception on the part of the *Os*, and therefore affords inadequate information concerning the effect of deception upon reaction times. In the second part of the work, we have attempted so to alter the procedure as to set a new experimental situation which would be adequate to induce deception in the *O* and would at the same time be so simple as to isolate the quantitative phase of the deceptive response from complicating factors.

The most significant change in the situation consisted in extending the effects of the decision to obey or disobey to all items on the stimulus-card. In Experiment I., the same arithmetical operation was performed on every item on the card; while in Experiment II. the operations of addition and subtraction were distributed throughout the card and the *O* had either to obey by performing the *various* indicated operations or to disobey by performing the opposite of the *various* indicated operations. A decision to disobey by adding to (or subtracting from, as the case might be) all items on the card could not be made by the *O* at the beginning of the card and then forgotten, allowing the mental set induced to give the cue for every computation. As the decision to use one or the other column and to proceed either up or down likewise affected only the beginning of the response, only one list of exercises and one direction of procedure were used.

A further change was the substitution of instructions "to make your calculations on these cards ["O"] at the same rate as on the cards where you performed the operations indicated" in place of the former instructions "to make your calculations on the cards ["O"] as rapidly as on the cards with which you obeyed the instructions." It was hoped to avoid by this change the suggestion to the *O* that he would go faster on the "C" cards then on the "O" cards, as some *O*s had regulated their pace in accordance with this suggestion. The suggestion from Experiment I. may, of course, have held over for some *O*s.

Observers, Apparatus, and Procedure.—The *O*s in Experiment II. were the same as those in Experiment I.

320 pasteboard index cards, 8 by 5 in., were prepared, every one of which carried a single list of 10 arithmetical exercises. Every exercise consisted in subtracting or adding a single digit number to a two-digit number. There were 5 additions and 5 subtractions on every card. The number to be subtracted or added was constant for every card, and was either 2, 3, 4, or 5. The cards were divided into 32 sets of 10 cards each.

The instructions were as follows.

"You will be given a group of ten cards, upon each of which are written ten arithmetical problems. These problems involve addition and subtraction. These cards will be placed blank side up before you. At the signal 'ready,' you are to turn over the top card, and at the signal 'go,' you are to proceed through the problems on the card, giving the answer to each problem as fast as you can. In any five cards out of each group of ten cards you are to perform the operations indicated on each card and in the other five out of that group you are to reverse the operations indicated; that is, you will subtract instead of adding and add instead of subtracting. You are to deceive me as to the identity of the cards where you reversed the operations indicated by proceeding to make your calculations on these cards at the same rate as on the cards where you performed the operations indicated.

"At the completion of each card you are to enter in the table provided by the symbols C (correct) and O (opposite) whether you performed the operations as indicated or reversed them.

"At the conclusion of each set of ten cards you will be shown the experimenter's judgment as to the cards on which you reversed the operations indicated.

"At the end of the hour you will give an introspective description of your consciousness when performing the operations as indicated and when reversing them, noting especially any and all points of difference between the two."

In order actually to provide a motive for deceiving, which motive was lacking in Experiment I., every *O*, with the exception of S, was informed that his semester's grade in the course which he was taking in Psychology would be raised in accordance with the degree to which he succeeded in lowering the percentage of correct judgments by *E* as to when he was disobeying instructions.[10] The *O* was warned that he must obey the directions for the experiment.

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[10] No *O* was able to bring this percentage for him below 50% and thereby obtain an increase in grade.

Reaction times were taken with a stop-watch reaching to fifths of a sec. Time was allowed between the giving of the 'ready' and 'go' signals for the *O* to perform the central (mental) process of deciding to obey or disobey, and the watch was started simultaneously with the pronouncement by the *O* of his first result, so that the reaction times as recorded include the time for 9 of the 10 arithmetical exercises.[11]

At the completion of every set of 10 cards, *E* attempted to judge, by means of the reaction times alone, on which 5 cards the *O* had performed the operations as indicated (C) and on which 5 cards he had performed the opposite operations (O).[12]

All of the *O*s completed the series of 32 sets of 10 cards each. The amount of work done in a single experimental period was from 4 to 10 sets. An experimental period varied from one to two hours in length.

Results.—Analysis of the quantitative measurements of Experiment II., as shown in Table V., discloses but one type of reaction-time behavior under these conditions, namely, a type which is characterized by increased reaction times during deception. This type will be called the positive type, following the nomenclature we have been using. That it is truly a deception type is substantiated when the quantitative results and introspections are checked against each other. In general, the introspections are similar to those of the positive-type *O*s in Experiment I. That is, the positive-type *O*s in Experiment II., as in Experiment I., were conscious of deceiving and actually tried to deceive. This consciousness of deception caused strain, tension, and excitement, confusion, and at times unpleasant feeling and lack of ease when reversing the operations indicated; and it caused a feeling of relief and relaxation when obeying instructions. A few selections have been chosen from the introspections which will illustrate the above mentioned points.

TABLE V

Observer	Average "C" Reaction Times	Average "O" Reaction Times	Difference between "O" and "C" Averages	Probable Error of Difference	Probable Correctness of Difference
A	15.4587	17.0937	1.6350	.3018	1.0000
B	17.9965	18.4900	.4935	.3267	.8450
D	19.3675	19.4237	.0562	.3648	.5450
E	10.0425	11.0400	.9975	.1747	1.0000
H	10.1337	11.2437	1.1100	.1808	1.0000
L	12.8737	13.3737	.5000	.2637	.9000
Mc	12.9112	13.5487	.6375	.2718	.9400
M	16.6312	17.1462	.5150	.3042	.8750
R	16.9187	17.7500	.8312	.3251	.9600
S	8.5025	8.6137	.1112	.1310	.7175

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[11] We were not, in this experiment, interested in the time required to make the choice between obeying and disobeying, but in the effect of this choice upon the subsequent operations.

[12] See Footnote 3.

A, 1. "I tried some real deceit."

E, 1. "When I reversed the operations there immediately came to consciousness a feeling as if I must put on the brakes and be more careful. I would decide to reverse the operations just when the examiner said 'ready'; then when ordered to 'go' there was a new decision to make other than I had expected. Confusion resulted."

E, 2. "The hesitation caused when reversing the operations seems to be due to the fact that a decision must be made for each problem. I have been trying in both operations to regulate the amount of time so as to increase deception, but there is a tendency to go fast on certain cards because of performing the operations as told."

H, 1. "I did not feel at ease, but became excited."

H, 2. "I still continued to be excited and only occasionally was I able to relax. I was conscious all the time that I was disobeying. I felt practically no relaxation in the experiment. It was more strain. When I was disobeying my voice quivered because I was not so sure of myself."

Mc, 1. "I had a greater feeling of strain when disobeying. I had an unpleasant feeling when disobeying but not when obeying. I had a feeling of relaxation when both the O and the C cards of each set were completed, but especially when the O cards were completed."

Mc, 2. "The unpleasantness and strain were greater when doing the O cards than when doing the C cards."

M, 1. "I felt a relief at the end of a card, more so when disobeying than when obeying. I had to force myself to work fast when disobeying, for there always seemed to be a little resistance coming forth which I had to overcome."

M, 3. "Disobeying caused confusion, I having forgotten just what order I am following.

R, 3. "In changing from obedience to disobedience, I find it hard to get started. I tried hard to deceive."

In the cases of D and S, it should be noted that the probable correctnesses of the differences between the "O" and the "C" reaction times were, respectively, .5450 and .7175, so low that a classification of them with the other *O*s as of the truly positive type is not warranted. When the intropections are checked up with the quantitative measurements, further proof is given for the fact that in these *O*s deception was not truly induced by the experiment. A few excerpts are taken from their qualitative data to validate the above statement.

D, 1. "There is no emotional difference in my consciousness when I obey or disobey instructions. My mind was as calm as it was possible to make it. I do not have any noticeable difficulty in reversing instructions. I did not try to hurry either when obeying or when disobeying, but allowed my mind to take as long a time as needed to give the answers. I made no attempt to slow down the process when I was going rather fast, whether I obeyed or disobeyed, and gave the answers just as they came. I tried to make myself as unconscious and indifferent to the time that was passing as possible, and simply to give the answers as they came without thought of trying to deceive the experimenter."

D, 2. "I decided to obey or disobey usually upon looking at the card. If the numbers were small, I reversed the signs; if large, I performed the operations as they stood."

D, 4. "I am quite indifferent to whether I obey or reverse and I do not seem to find one more difficult than the other."

TABLE VI

Observer	Number of sets on Day	Average "C" Reaction Times	Average "O" Reaction Times	Difference between "O" and "C" Averages	Probable Error of Difference	Probable Correctness of Difference
A	8	19.7600	21.5100	+1.7500	.4881	.9925
	8	15.5300	17.5100	+1.9800	.4699	.9975
	6	14.1600	13.9000	− .2600	.4649	.6500
	10	12.7400	15.1440	+2.4040	.3478	1.0000
B	8	19.2550	21.6450	+2.3900	.6425	.9925
	8	19.1600	19.4000	+ .2400	.8409	.5750
	8	16.6450	16.7250	+ .0800	.4565	.5475
	8	16.9250	16.1900	− .7350	.5682	.7750
D	5	26.6640	27.7360	+1.0720	.9491	.7750
	7	26.4785	26.1285	− .3500	.0946	.9925
	8	17.0500	17.7350	+ .6850	.5519	.8000
	6	16.2400	14.9333	−1.3067	.4034	.9850
	6	17.3866	17.5133	+ .1267	.4069	.9825
E	8	11.4550	13.1600	+1.7050	.2973	1.0000
	8	10.3800	11.8950	+1.5150	.3183	1.0000
	8	9.7500	10.4850	+ .7350	.2738	.9650
	8	8.5850	8.6200	+ .0350	.2044	.5475
H	8	11.5200	12.0450	+ .5250	.4140	.8000
	8	11.0450	12.8500	+1.8050	.4022	1.0000
	8	9.2050	10.6550	+1.4500	.3540	.9975
	8	8.7650	9.4750	+ .7100	.2393	.9750
L	4	16.7400	18.5300	+1.7900	1.0197	.8800
	7	16.9642	17.2428	+ .2786	.3216	.7200
	5	11.5360	11.4160	− .1200	.5895	.5625
	8	12.7550	13.6650	+ .9100	.4812	.9000
	8	11.2850	11.3600	+ .0750	.3278	.5625
Mc	6	18.7866	18.0200	− .7666	.4791	.8625
	6	12.4800	13.4866	+1.0066	.3459	.9750
	8	12.1600	13.6950	+1.5350	.3043	1.0000
	8	11.0250	11.5500	+ .5250	.3142	.8675
	4	10.0200	10.6400	+ .6200	.4447	.8250
M	4	22.1000	22.4500	+ .3500	.7125	.6375
	8	19.6050	19.5600	− .0450	.5140	.5300
	4	16.3900	16.7100	+ .3200	.3585	.7375
	8	14.5250	15.2600	+ .7350	.2637	.9700
	8	13.1500	14.1800	+1.0300	.3409	.9800
R	4	23.9500	24.9300	+ .9700	.9245	.7625
	7	24.5642	25.7500	+1.1858	.1309	1.0000
	5	15.2800	16.7280	+1.4480	.4644	.9825
	8	14.7400	14.9050	+ .1650	.3152	.6375
	8	14.2150	15.1300	+ .9150	.2643	.9900
S	6	10.6800	11.0266	+ .3466	.6811	.6875
	10	8.4880	8.4400	− .0460	.2517	.5625
	8	7.8950	7.9150	+ .0200	.1700	.5300
	8	7.4950	7.7200	+ .2250	.2164	.8500

The following introspections of S were obtained by further experimentation. It will be remembered that S has had considerable practice with the kind of work which constituted the material of both Experiment I. and Experiment II.

S. "The attitude is apparently the same toward each set. I am completely indifferent to the various sets. I am as willing to tell the truth as to tell a falsehood, especially as I gain nothing by lying. The difficulty of the two forms is about the same. The work has become mechanical. I am not aware of any difference between my attitude when obeying and my attitude when disobeying."

Examination of Table VI. shows that 7 of the 10 *Os* of Experiment II. had reversal days, that is, days on which the average of the O reaction times was less than the average of the C reaction times. E, H, and R had no reversal days. D and S had reversal days, but, as has already been noted, the nature of both their introspections and their quantitative measurements prohibits the conclusion that they actually practised a true deception during the experiment. In the case of the other 5 *Os*, A, B, Mc, M, and L, analysis of their introspections for the reversal days reveals that the attitude of these *Os* on these days was different from their attitude on normal days and that it was not the typical attitude of deception.

A, 3. "I notice quite a good deal more ease in computing the O cards than in computing the C cards."

B, 4. "It seemed to make no difference to me whether I disobeyed or obeyed. I usually obeyed with the smaller numbers and disobeyed with the larger ones. I tried to go as fast as I could in both cases."

L, 3. "There was really no difference in my consciousness in obeying or otherwise."

Mc, 1. "There was no feeling of strain or unpleasantness. The longer I did it, the easier it became to do both O and C, and the more pleasantness accompanied the experiment."

M, 2. "The usual hesitancy when disobeying was not so common as formerly."[13]

Conclusions.—In Experiment II., we attempted so to alter the procedure of the first experiment as actually to induce our *Os* to practise deception. The introspective reports bear testimony to the success of this attempt in the cases of 8 out of the 10 *Os*. The quantitative results for these 8 *Os* show that their reaction times were increased during deception, with relatively large probabilities that the increases were not due to chance. In the cases of the two remaining *Os* the conditions which were designed to induce deception (disobedience of the indicated operations) did, indeed, increase their reaction times, but the differences were so slight that they may have been due to chance factors. And these two *Os* reported, introspectively, no deception. Thus, reaction times were increased when disobeying in those cases in which the *Os* were conscious of deception.

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[13] See Footnote 8.

This conclusion is supported by the fractionation of the data by days. The "O" reaction times were less than the "C" reaction times on those days on which the *O*s reported that they had little or no consciousness of deception.

We find, therefore, complete agreement between the two parts of the work. The situation in Experiment I. was only in small part successful in inducing deception. Whenever it did induce deception, the "O" reaction times were longer than the "C" reaction times; whenever it failed to do so, the opposite was true. The so-called positive and negative types of response were, therefore, not two types of response to deception, but a deceptive and a non-deceptive type of response to the particular experimental situation. When the experimental setting was changed in Experiment II. so as to render more effective the factors which tended to produce a consciousness of deception, all of the *O*s became positive in type, that is they tended to give the desired deceptive response. Two *O*s, in whom this tendency was not marked, failed to report deception. Five other *O*s were negative on certain (reversal) days, that is, they gave non-deceptive responses on these days, as evidenced by their reports. In both experiments consciousness of deception was accompanied by lengthened reaction times, as compared with the reaction times when the *O*s were not conscious of deception. The reaction times were in no case an indication of obedience or disobedience to instructions, but were, on the other hand, an indication of conscious deception.

Other investigations have shown that various so-called symptoms of deception, including reaction times, are not indications of objective lack of truth, but only of conscious deception. Leach and Washburn (7, p. 64) found several cases in which the reaction times to words concerning which the *S* was to practise deception were not longer than the reaction times to other words. But they also found that in the majority of these cases the factor of deception was inoperative. Marston (8, p. 162) came himself to the conclusion that "the behavior of the blood pressure does not act as the least indicator of the objective validity of the story told by any witness, but it constitutes a practically infallible test of the *consciousness of an attitude of deception*" (italics Marston's). Burtt (2) similarly recognized that the inspiration-expiration ratio is a symptom of the deceptive consciousness, rather than of objective truth or falsity. So, we have shown that the time required to perform simple computations is increased by a consciousness of deception, but is not increased by objective, but unconscious, falsity of the responses. If the results are applicable to the more heterogeneous conditions of association-reactions to words, increased association-reaction times may be taken to indicate conscious deception, but not objective falsity, as all investigators prior to Marston have found.[14]

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[14] Marston (9) found that the mean variations in reaction times tended to show the same differences (positive or negative) as the reaction times themselves. Our results show no constant tendency of the mean variations in reaction time during deception. The data are, therefore, omitted here, but may be found in the manuscript copy referred to in Footnote 3.

SUMMARY

1. The time required to perform simple exercises in addition and subtraction was increased by the consciousness of deception.

2. The consciousness of deception appeared as feeling of strain, self-consciousness, hesitation, conflict of impulses, and emotional disturbances.

3. A repetition of Marston's experiment showed that the experimental setting failed in large measure to produce deception, and that his so-called "negative" deception-type was, in reality, a type of non-deceptive response to the particular situation.

4. When the situation was changed so as to make the consciousness of deception occur more frequently, all observers became "positive" in type. In other words, they gave deceptive responses.

5. If the results obtained in simple computations may be generalized to apply to other situations, increased reaction time is a symptom of conscious deception, but not of objective falsity.

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A CASE OF SYNAESTHESIA

By Leon Ginsberg, Columbia University

A search of the literature on Synaesthesia (surprisingly large) fails to reveal any case in which secondary sensations of color appear upon the stimulation of each one of the sense organs. Colored hearing, colored odor, colored taste, or a combination of the three, is comparatively common. But no report has been published of any individual in whom every primary stimulus capable of producing a sensation is certain to cause the simultaneous appearance of a secondary color sensation.

The following report concerns itself with the synaesthesia of the author. He was first cognizant of the phenomenon during the year 1918. Whether it had been present before that year he is unable to say. During that year he was physically run-down and nervously instable. Whether he became subject to these secondary sensations because of his physical condition he is unable to assert.

Various Forms of Synaesthesia and Allied Phenomena

1. Colored hearing:
    - (*a*) Colored vowels;
    - (*b*) Colored consonants; the consonant taking the color of the vowel used in naming it;
    - (*c*) Colored music, colored tones, colored auditory imagery of tones;
    - (*d*) Colored noises.
2. Colored tastes.
3. Colored odors.
4. Colored cutaneous sensations:
    - (*a*) Colored temperature;
    - (*b*) Colored touch;
    - (*c*) Colored pain.
5. Colored kinaesthetic sensations.
6. Colored organic sensations.
7. Number forms, forms for the days of the week, months of the year, centuries.

The following color-labels are taken from the standard colored Chart for Elementary Color Instruction based on the Bradley Color Standards. The chart was used whenever and wherever possible, but the secondary color sensations are so varied, they change so slightly but definitely in shade and tint, that it was found impossible to find enough color variations on any chart available. For this reason, even where the colors of the chart are used, it must be understood that they are only approximate.

The Letters of the Alphabet

A(day): light, bright, beautiful apple green; yellow green, tint 1.
Ah(rah): dark brown, almost black.
Aw(raw): dark brown.
E(met): tan, color of a new pine-board.
E(me): white.
I(sight): strange blue; nearest equivalent is the flash of lightning
I(hit): dull silver or aluminum.
O(so): dark blue.
O(hot): almost black.
U(sounding like few): milky blue with some white in it; almost grey.
Ae(aesthetic): green.
Oo(good): dull grey.
Oi(void): blue green.
Ow(now): dark blue.

Taste

Sweet: orange red standard.
Salt: intensest blue; blue, shade 1.
Sour: intensest greenish blue; standard green.
Bitter: black.

Odor

Oil of Cloves: standard orange.
Rhubarb and Soda: yellow green, shade 2.
Camphor: standard red.
Peppermint: yellow, shade 1.
Iodine: yellow-green standard.

Temperature

Cold: white.
Normal: neutral dark grey.
Extreme cold or warm: reddish (color of pain).

Pain

Headache: black.
Intense headache: red.
Toothache: blue.
Intense toothache: red.

Tones (on Piano); Fourth Octave above Middle C

D: green, shade 1.
A: blue, tint 2.
D♯: blue-green, tint 2.

Third Octave above Middle C

D♯: green, shade 2.
F♯: blue-green standard.
C♯: green-blue standard.

First Octave above Middle C

A: green, shade 1.
B: yellow-green, shade 2.
G♯: green-blue, shade 2.

Middle Treble

C: red standard.
E: red, tint 1.
F: red-orange standard.

First Octave below Middle C

E: red, shade 2.
A: red, shade 1.
G: orange, shade 1.

Second Octave below Middle C

F: deep brown.
A: deep brown, a little lighter than the color for F.

Third Octave below Middle C

B: deep brown with a blue tinge, almost maroon.
E: black brown.

The highest notes run through a series of color, blue or grey to a silvery grey, and finally the highest notes of all become a dull silver. It will be seen, then, that the range of colors runs from a brown of a very dark shade, almost a black, for the lower notes, those of the third octave below middle C, through a deep brown for the higher notes, of the second octave below middle C, through orange-red and red for the notes of the first octave below middle C, through red for the notes of the middle octave, through green-blue and yellow-green, blue-green and green and green-blue for the notes of the third octave above middle C, and through green-blue for the notes of the fourth octave above middle C.

The colors therefore run as follows:

Black Brown
Brown
Dark Red
Orange Red
Lighter Red
Blue Green
Green Blue
Cold Blue
Dull Grey
Silvery Grey
Dull Silver

The above list shows not a very close, if any, resemblance to the spectrum. As the tones run through a series of dark, heavy, massive bass notes to notes gradually lighter, less massive and more tinkling, so too the colors run through a series from dark, heavy colors, to lighter and lighter colors, until grey is reached.

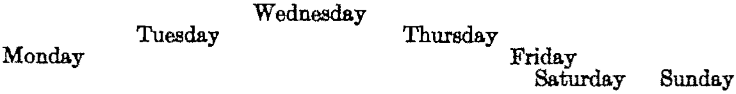
Photisms for Digits

The digits take on the same color as the vowel used in saying the digit. Thus:

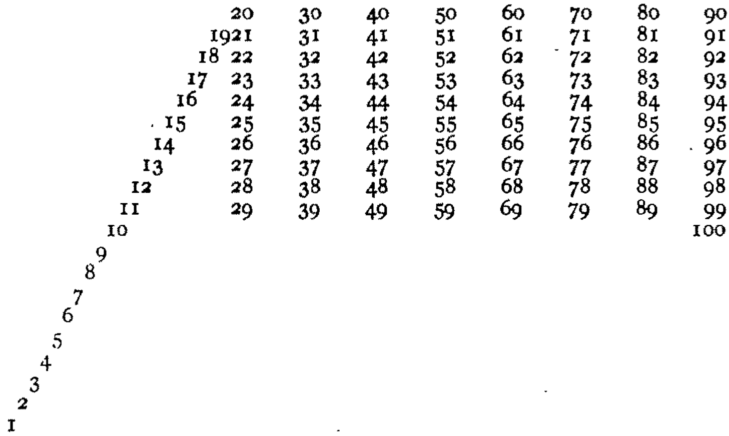
Digit	Vowel Component	Vowel Color	Digit
1	*u* (hut)	dark, dull blue	dark, dull blue
2	*u* (oo)	blue grey	blue grey
3	*ee*	white	white
4	*aw*	dark brown	dark brown
5	*i* (fight)	metallic blue	metallic blue
6	*i* (fit)	dull silver	dull silver
7	*e* (met)	tan	tan
8	*a* (ate)	green	green
9	*i* (fight)	metallic blue	metallic blue
10	*e* (met)	tan	tan

The days of the week take on the color of the vowels used in saying each day. Thus Monday has two concomitant colors, dark bluish grey and green.

Forms: Days of the Week



Forms: Numbers



The following experiment was performed to determine the permanence of the secondary sensations.

July 1921	Nov. 1921
Middle Treble	
C: red standard.	O. R. standard.
E: red, tint 1.	Red, tint 1.
F: red-orange standard.	Red-orange standard.
First Octave above Middle C	
A: green-blue, shade 1.	Violet-blue, shade 1.
B: yellow-green, shade 2.	Yellow-green, shade 2.
G#: green-blue, shade 2.	Green-blue, shade 2.
Third Octave above Middle C	
D#: green, shade 2.	Green, shade 2.
F#: blue-green standard.	Blue-green standard.
C#: green-blue standard.	Blue, tint 1.
Fourth Octave above Middle C	
D: green, shade 1.	G. standard.
A: blue, tint 2.	Blue, tint 2.
D#: blue-green, tint 2.	Blue-green, tint 2.
First Octave below Middle C	
D: red, shade 2.	Red, shade 1.
A: red, shade 1.	Red, shade 1.
C: orange, shade 1.	Orange, shade 1.

Second Octave below Middle C	
F: deep brown.	Deep brown.
A: deep brown.	Dark brown.
C: deep brown, almost black.	Blackish brown.
Third Octave below Middle C	
B: deep brown, with blue tinge; almost maroon.	Dark purple.
G: black, with brown tinge.	Black.
E: black-brown.	Dark brown.

After a five-month period the secondary sensations present practically no change. What slight variation appears, is one of shade and tint rather than color.

Many authors have reported cases of fusion of the secondary sensations in accordance with the laws of color mixture. Other authors have failed to find any such fusion. The following experiment was performed. The same piano-notes as above were sounded in pairs as chords, five months after the colors for the individual notes had been determined. There was no fusion of the secondary sensations. As the chord was sounded, the colors for the notes composing the chord were seen to run like two colored ribbons side by side. Repeated sounding of the same chord made the color of each note appear more clear, but at no time was there fusion. An effort was then made to see whether the color of each note of the chord was the same as the color determined for it five months previously, when it was sounded alone.

Color of Note Sounded Alone	*Color of Note Sounded in Chord*
Middle Treble	
C: red standard.	Orange-red standard.
E: red, tint 1.	Green-blue, shade 1.
First Octave above Middle C	
E: yellow-green, shade 2.	Blue standard.
G#: green-blue, shade 2.	Orange-red standard.
Third Octave above Middle C	
D#: green, shade 2.	Red, tint 1.
F#: blue-green standard.	Violet-red, tint 1.
Fourth Octave above Middle C	
D: green, shade 1.	Green-blue, shade 2.
A: blue, tint 2.	Blue, tint 2.
First Octave below Middle C	
D: red, shade 2.	Red, shade 2.
A: red, shade 1.	Violet-red, shade 2.

The color for every tone, when sounded in conjunction with another tone, is found to be different from the color of the same tone when sounded alone. There seems to be a reciprocal reaction of one tone upon the other, with a concommitant change in the secondary sensations.

An attempt was made to see if any fusion of colors, comparable with laws of color mixture, occurred when taste was the primary sensation. In a glass of water at room temperature

(68°) three teaspoons of sugar were dissolved. The sugar was allowed to dissolve thoroughly, and was distributed as evenly as possible through the water by stirring. The water was sipped, and the color of the secondary sensation noted. A glass with an equal volume of water at the same temperature, in which was dissolved a teaspoon of table salt, was sipped (on the next day), and the color of the secondary sensation was noted.

Into a bottle was now poured a glass of water containing dissolved in it three teaspoons of sugar, and a glass of water of equal volume into which had been put a teaspoon of salt. The water was thoroughly stirred. The temperature was as before. The mixture was sipped, and the color of the secondary sensation was noted. The results were as follows.

Sweet: R. O. standard.
Salt: blue standard.
Mixture: violet-red, tint 1.

There is a fusion, and the color obtained is an intermediate between the colors for salt and sweet, red-orange and blue.

Fusions of sensations from different senses were next tried. The tongue was projected from the mouth, the mouth was shut, and the nose was plugged with cotton. Oil of cloves was put upon the tongue. The color of the secondary sensation was recorded. The mouth was then tightly closed and the secondary sensation of the odor of oil of cloves was recorded. Then the tongue was once more projected, oil of cloves was put on it, and at the same time the bottle of oil of cloves was put under the nose. The secondary sensation was noted. The same experiment was tried with rhubarb and soda.

Oil of Cloves

Taste: blue, shade 2, almost black.
Odor: standard orange.
Taste and Odor: red, shade 2.

Rhubarb and Soda

Taste: orange-red standard.
Odor: yellow-green, shade 2.
Taste and Odor: dark green (yellow green, shade 2).

Here too, there was fusion, and once more an intermediate color was obtained.

Localisation

Colors are projected in the direction of the sound, if the sound comes from without. In inner speech, as when the letter *a* is inwardly pronounced, the color is located somewhere within the head; and so rapidly does the color follow upon the sound that it is located, I am almost sure, just against that part of the palate and mouth used in pronouncing a vowel. Thus, the white photism for the vowel *e* (me) is localized against the roof of the mouth, and the photism for *e* (met) is localized down in the throat.

Taste photisms are localized in the mouth, and odor photisms in the nose. Temperature photisms are localized on that part of the body experiencing the sensation. Pain photisms are localized on the part of the body experiencing the pain. The photisms for all the sensations are localized on the part of the body concerned in the primary sensation.

The term 'synaesthesia' has been used so loosely that it has led to confusion. The two leading theories, the association theory and the physiological theory, have adherents, just because of this blanket-term for two groups of different phenomena. All adherents of the association theory present proof which points clearly to an associational cause. And the adherents of the physiological theory present very pertinent material to uphold a physiological causation. Both theories are correct. But each, more or less adequately, accounts for phenomena entirely different. The phenomena should not be included under the same class-name. Due to association are types of colored hearing in which a color is attached to an object for manifestly associative reasons, number forms, forms for days and months, personifications of various sorts, and association between colors and theories (of history, etc.) and colors and abstract terms (there is present here, I believe, a common affective tone serving as an associative bond). Under pure synaesthesia should be included colored hearing, colored taste and odor, colored temperature, pain, etc., gustatory audition, etc. Number forms, etc., and colored hearing are very often found together, but this conjunction does not mean they have a common casual factor. For just as many individuals have number forms and various personifications, etc., and yet betray no evidence, at the same time, of colored hearing.

To say that synaesthesia is due to associations formed in childhood seems to me not to be in accord with the facts. There is too great a dependence of the secondary sensation upon the primary sensation. The slightest change in the primary sensation causes a proportional change in the secondary sensation. There are as many concomitant color changes of shade and tint as there are possible changes in the primary sensations. The number of colors which are definitely 'seen' as photisms are too varied, too definite and permanent, to have been learned in childhood, or indeed in a life-time. The color of a new odor or taste combination (a mixture of oil of cloves and rhubarb and soda never before tasted) gives as definite and immediate a photism as does a primary sensation many times experienced. Associations so ingrained, so definite and fixed, as are the color photisms would remain so through life (especially if formed in childhood). Yet synaesthesia usually disappears in senescence, and indeed very often after adolescence.

It seems to the author that synaesthesia is not a phenomenon peculiar to about 15% of the population, but is, in some form or other, true of everyone. It would be better to say that in 15% of the population it is so intense as to be more noticeable than in the other 85%. Literature gives a clue to the universality of the secondary sensations. Most, if not all of us, seem to agree with the following descriptive phrases; the silvery tone of the violin, the red blare of the cornet, the golden voice of the tenor, etc.

Titchener says (*Text Book of Psy.*, 194 ff.): "Every sensory stimulus of moderate intensity arouses a wide-spread reaction. It has been shown, for instance, that the sounding of a single note upon the harmonium will not only bring about an adjustment of the organs of hearing, but will also call out visual, verbal and other associations. . . .

"Sometimes they [the phenomena of synaesthesia] seem to depend upon a purely individual feature of nervous constitution, as when a sour taste makes the scalp itch; sometimes they are common to a large number of persons: most of us shudder and grit the teeth when we hear the squeak of chalk against the blackboard, or the grating sound of a saw against metal. The connection may obtain between separate sense-departments, as sound and sight, or between areas of a single sense-department; thus an itching of the nose often comes along with pricking sensations at the back of the neck on the same side . . . In some cases, the concomitance is stable, in others it is highly variable; in some it is limited, as it were incidental, in others systematic, extending to an entire series of qualities."

SOME OBSERVATIONS AND EXPERIMENTS OF THE INTELLIGENCE OF THE CHIMPANZEE AND OURANG

By WM. THOMAS SHEPHERD, Columbia University School

The experiments and observations reported herein were made on two apes, Sokker, a male chimpanzee 7 years old, and Rufus, a male ourang 4 years old, in the National Zoological Gardens, Washington, D. C.

Purposes: I. To test their mentality for (1) ideas, ideation, (2) reasoning powers, and (3) imitation as involving ideation.

II. To test them by experiments similar to experiments made by the present writer on Rhesus monkeys some years ago; to ascertain if their intelligence is equal to, or greater than, that of other monkeys, so far as these tests would indicate.

III. To verify conclusions reached previously in tests on other apes (the trained vaudeville stars Peter and Consul) observed and previously reported on by the present writer.

IV. To test their intelligence as compared with that of dogs and cats in similar experiments made by the writer at the George Washington University.

Tests of Reasoning

In this test, as in those on Rhesus monkeys, dogs, and cats, referred to above, I suspended a piece of apple (for which the apes have a liking) by a string in front of the cage beyond their reach; it had a piece of stick run through it, one end of which was pointed toward the cage in which the animals were kept; that end of the stick was within the reach of the apes if they would pass their hands through the wire of the cage. The problem was to see if they would reach out and seize the end of the stick, and draw in and thereby secure the piece of apple.

Sokker, the chimpanzee, reached through the wire, seized the end of the stick, and drew in the piece of apple: in the first trial, in 5 sec.; in the second trial on that day in 4 sec. Rufus, the ourang, succeeded in securing the piece of apple in the same manner: in his first trial, in 8 sec.; in his second trial on that day in 6 sec.

These experiments were repeated the next day. Both animals did the act, and in shorter time. In this experiment both gave what I consider to be clear indications of a low form of reasoning,—what I have called, in the previously reported experiments on Rhesus monkeys, Adaptive Intelligence.

Tests of Ideation

To test the animals I employed a plan formerly used with Rhesus monkeys. I placed a light board, 30 in. long and 8 in. wide, outside the cage and at right angles to it, upon an improvised platform on a level with the floor of the cage. One end of the board was within reach of the animals through the wire of the cage; the other end was beyond their reach; a piece of banana, for which the animals have a fondness, was placed out of reach on the board. The problem was to see if the apes would take hold of the end within reach, pull the board in, and thus secure the banana.

Both the animals did the act successfully. The figures are: Sokker, first trial, 20 sec.; second trial, 15 sec. Rufus in the first trial took 25 sec.; in the second trial 16 sec. We might remark that Rufus appeared somewhat fearful and cautious. The next day the experiments were repeated. Sokker secured the banana in the first trial in 8 sec., and in the second trial in 6 sec. Rufus took 9 and 6 sec. respectively in his first and second trial on that day.

Tests of Imitation

In these tests I employed a method used by Hobhouse in England on a chimpanzee and Cebus monkey, and by Watson at Johns Hopkins University on monkeys. I had employed the plan in tests on Rhesus monkeys, reported in Monograph Supplement No. 52 of the Psychological Review. In these tests on the monkeys, I had secured only negative and partially positive results.

A T-rake was used; food, a piece of banana, was placed in front of the cage and in the apes' sight but beyond their reach; the T-rake was placed with the end of the handle within their reach; the problem was to see if they would take hold of the handle and draw in the food, after seeing me do it; if they would imitate my actions. I demonstrated the use of the rake three times, and then gave each separately (they were in different cages) a chance to imitate my act, and thereby to secure the piece of banana.

Sokker in the first trial immediately seized the end of the rake handle and pulled it under the lower part of the wire into the cage; he examined it carefully, and played with it; but he made no effort to use the rake to draw in the food as I had shown him how to do. The banana was, of course, within the sweep of the rake, if he had used it. He was given 2 min. to get his ideas together, if he had any, in the matter. In his second trial, he repeated the actions of the first trial; he made no attempt, so far as I could observe, to use the rake to secure the banana. He at times looked, or appeared to look, at the banana. In similar tests next day, two trials under similar conditions and time, he entirely failed.

Rufus, on the first day, first trial, was allowed 2 min. He repeated essentially the actions of Sokker. He seemed, perhaps, to be more fearful and cautious. He failed likewise in the second trial.

Consideration of Results

A comparison of the results of the experiments on reasoning (of the low form) shows that the apes did the acts the Rhesus monkeys did, and in quicker time. The experiments with the apes were fewer in number, but seem to me to be significant. Their mentality, as compared with that of the Rhesus monkeys, seems to be superior, so far as these experiments show. The dogs and cats tested failed entirely or almost entirely in similar experiments. But we should bear in mind that the latter have not the motor equipment, that is, the hands, that the apes and other monkeys possess. In every way, however, they seem to be inferior in intelligence.

In the tests on ideation, in general the apes surpassed the Rhesus monkeys, and by far the dogs and cats tested. That is, they did all, or nearly all the acts the monkeys did, and in shorter time; their understanding of a given situation, like these here reported, was better.

In the test for imitation here reported, two of the Rhesus monkeys did make a success, or at least a partial success, in similar trials. But they were allowed many more trials than the apes. They did not succeed in the number of trials given the apes. For myself, I am not convinced that the apes under similar conditions of more trials would not have done as well as, or better than, the Rhesus monkeys, supposed biologically to be their inferiors. I suggest that they probably would have done so.

Summary

I. The superior motor equipment of the apes accounts in large measure for their apparently superior intelligence.

II. Their semi-erect and biped position accounts in part for their alleged superior intelligence.

III. (1) They give indications of ideas, probably of a crude and unanalysed type. (2) They give evidence of a form of intelligence which may roughly be called a low form of reasoning—as is shown by the suspended-apple experiments here reported.

IV. Observations and experiments, as well as the theory of evolution, appear to give the apes, at least the chimpanzee and ourang, a place above all other sub-humans, in intelligence, as in anatomical structure.

The writer is fully aware that the experiments here reported are few in number. But they suffice, taken together with other observations, to make him reasonably confident of these conclusions.

CONCERNING QUANTITATIVE PLETHYSMOGRAPHIC TECHNIQUE

By SAMUEL W. FERNBERGER, University of Pennsylvania

Several years ago Hartman and McDonough[1] reported a method for making quantitative records of the increase and decrease in the blood volume of the forearm. The method consists in inserting a graduated glass tube for the usual glass tube of a Lehmann plethysmograph, and thus being enabled to read the volumetric changes directly. If the volume of the entire plethysmograph is known, and if the amount of water inserted is known, one can then ascertain the volume of the arm involved in each observation. It is thus possible to express the results in terms of percentage of increase or decrease of the volume of the arm involved. These authors used diastolic blood pressure to determine the maximum expansion or elasticity of the vascular system. In this part of their experiment, a Stanton Blood Pressure Apparatus was applied to the upper arm while the same forearm was inserted in the plethysmograph. The bag was then inflated until the veins were closed while the arteries remained open. Thus the blood continued to flow into the forearm without any possibility of flowing out, and so the vascular system was stretched to the limit of its elasticity. Hartman and McDonough also determined the percentage of increase and decrease for a purely physiological function, such as holding the breath, and for mental work, such as difficult mental multiplications. These investigators report one observation each for 24 subjects. In general they found the following results. A considerable variation was found from *S* to *S* in the increase due to diastolic pressure. The average for all *Ss* was 1.76 *per cent.* of the volume of the arm. Much smaller variations were found for holding the breath and for mental work, and the peripheral changes due to these tasks never approached the limits of elasticity.

It seemed worth while to repeat this experiment, taking a number of observations at intervals from each of a group of *Os*, to ascertain the variability of a given *O* from time to time. The present study reports such observations on 10 *Os*. The number of observations ranges from 6 to 20 per *O*, taken at intervals of a week. Diastolic pressure, holding the breath and mental work were again used, so that the results might be directly comparable with those of Hartman and McDonough.

In a preliminary series of experiments it was ascertained that the column of water in the graduated tube almost invariably came to rest, after an observation, at a point lower than when the observation had been started.

It was believed that the cooling of the water in the plethysmograph, and its accompanying contraction, might be the cause of this lowering. Hence we inserted a thermometer through the metal tube, so that the bulb was entirely immersed in the water over the wrist. The metal sleeve was covered with cotton-wool and had an outer covering of oil-cloth. It was discovered that water inserted at 98° F. cooled less than 2° F. during a 15 min. observation. This cooling is not great enough to account for the disparity of the readings before and after an observation.

It was then noticed that the elbow had a tendency to slip upward in the elbow-rest during a long observation. The usual padded arm-rest had been used. We then discarded this type of arm-rest, and made a plaster cast of the elbow for each *O*. With the plaster-cast elbow-rest the *O* had little trouble in keeping the arm in position, and this source of error was entirely eliminated.

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[1] G. N. Hartman and D. L. McDonough, On Arterial Expansion, this JOURNAL, 32, 1921, 516–518.

Obs.	Number of Observations	% of Change Diastolic	P. E.	% of Change Holding Breath	P. E.	% of Change Mental Work	P. E.
1	11	+3.01	0.222	−0.37	0.046	+0.04	0.046
2	13	+1.51	0.090	−0.02	0.047	+0.15	0.036
3	18	+1.66	0.425	+0.33	0.039	−0.39	0.072
4	19	+1.82	0.115	+0.44	0.038	+0.03	0.032
5	20	+1.98	0.093	+0.48	0.078	−0.22	0.025
6	20	+2.21	0.115	+0.34	0.052	−0.07	0.252
7	6	+1.80	0.181	+0.43	0.044	−0.10	0.049
8	18	+1.29	0.114	+0.24	0.038	−0.01	0.036
9	6	+2.64	0.126	+0.22	0.074	+0.17	0.044
10	18	+1.40	0.069	+0.16	0.024	+0.11	0.024

The results reported in the accompanying Table were all taken with plaster-cast elbow-rests. The *Os* and the number of observations for each are given in the first two columns of the table respectively. In the third column are given the averages for the percentage of change under diastolic pressure, with the probable errors in the fourth column. In the fifth and sixth columns are given the average percentage of change and the probable errors for holding the breath, and in the final two columns the corresponding values for mental work. In all cases the averages were calculated taking account of the sign of each observation.

Our results agree exactly with those of Hartman and McDonough. The increase for diastolic pressure is very much greater than for either of the other conditions for all of our *Os*. The general average is 1.93 *per cent.* The probable errors are larger than one would desire but, considering the inaccuracy of the present best methods of measuring blood pressure, they are probably as small as one could expect.

For holding the breath the averages show an increase for 8 *Os* and a decrease for 2. The percentage of change is not great, however, and the probable errors are so large as relatively to invalidate the results in most cases. This is because no *O* gave all of his determinations either as increases or as decreases of volume. These results are somewhat at variance with those of Hartman and McDonough, who found that 16 of their 24 *Ss* showed a decrease, 5 showed no change, and only 3 showed an increase. We both agree that the reactions of the different *Os*, as well as of the same *O* from time to time, seem to be variable.

There is even greater variation from *O* to *O* in our results for mental work. Five show an increase and five a decrease. Again no *O* had all the observations in the same direction, so that the probable errors are so large as to lend little validity to the results. For mental work our results agree more closely with those of Hartman and McDonough. Of their 24 *Ss*, 13 showed a decrease, 4 no change, and 7 an increase.

We took account of temperature, weather conditions, blood pressure and a subjective estimate of the *Os'* general health and well-being; but none of these factors seems to correlate with the variations in the results.

It would seem that this extreme variability makes questionable the entire method of plethysmographic research in psychology. Apparently there are a number of factors, both physiological and mental, which may affect the plethysmographic results. For example, we are convinced that, during mental work, the trend of increase or decrease of the volume of the forearm is largely determined by the *O's* breathing during the observation. The fact that the *O* may at times hold his breath during mental work we believe has a much greater effect on the plethysmographic curve than the actual work itself. Hence it may well be that we have been studying artifacts or indirect effects in our vascular work in psychology rather than the direct effects of our stimuli. This, then, may be the cause of the great variability of plethysmographic results which Leschke[1] discovered when he attempted to statisticize the results of former investigators. However, this theory needs further investigation and research.

Summary

1. Continued observations on the same *Os* yield results, in quantitative plethysmographic research, that are very similar to those of Hartman and McDonough.
2. For the limits of elasticity, the probable errors are as reasonably small as one might expect with the present inadequate methods of taking blood pressure.
3. For holding the breath and for mental work, the probable errors are so large as probably to invalidate the averages.
4. The cooling of the water in the plethysmograph is so slight that it may probably be disregarded for a 15 min. observation.
5. The old-style padded elbow-rest of the Lehmann plethysmograph was found to be a source of considerable error which may be eliminated by substituting a plaster cast of the elbow for each *O*.

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[1] E. Leschke, Die körperlichen Begleiterscheinungen seelischer Vorgänge, *Arch. f. d. ges. Psychol.*, 21, 1911, 435ff.

GOERENZ' *VESTIGIA*

By E. B. Titchener

In his famous Note on the history of the association of ideas,[1] Hamilton refers to four earlier works: the *Geschichte* of Hissmann, the *Paralipomena* and *Beyträge* of Maass, and the *Vestigia* of Goerenz. The full titles, etc., of these works are as follows.

(1) M. Hissmann, *Geschichte der Lehre von der Association der Ideen, nebst einem Anhange vom Unterschied unter associirten und zusammengesetzten Begriffen, und den Ideenreyhen*, Göttingen, 1777, pp. [24 +] 144.

The date is given correctly by Hamilton, Maass (*Einbildungskraft*, 1797, 402), W. Volkmann von Volkmar (*Lehrbuch*, ii., 1885, 549), W. H. Burnham (this Journal, ii., 1889, 618), M. Dessoir (*Geschichte*, i., 1897, 211), etc.; it is given as 1776 by L. H. Jakob (*Grundriss d. Erfahrungs-Seelenlehre*, 1810, 118) and by Goerenz; and as 1772 by Dessoir (i., 1894, 307) in a Note which contains other errors.

(2) J. G. E. Maass, *Paralipomena ad historiam de associatione idearum*, 1787. *Dissert. inaugur.*

I have not seen this tract, which was written as a supplement to Hissmann's *Geschichte*.

(3) J. G. E. Maass, *Beyträge zur Geschichte der Lehre von der Vergesellschaftung der Vorstellungen:* Part iii. of *Versuch über die Einbildungskraft*, Halle & Leipzig, 1792.

I have not seen this first edition. In the edition of 1797 the *Beyträge* occupy pp. 311–453. It is reasonable to think that Maass has here repeated the substance of his *Paralipomena*,—which he does not mention; though Hamilton's reference to both works may be significant.

(4) J. A. Goerenz, *Vestigia doctrinae de associatione quam vocant idearum libris veterum impressa*, Vitebergae (Halle), 1791.

This dissertation seems to be extraordinarily rare. Maass does not mention it, although he brings his list of authors down to 1796; and Dessoir says nothing of it. I have been unable, by the ordinary means of search, to find it in London, Oxford, Edinburgh, Paris, Leipzig or Halle. I have finally run down a copy in the *Stadtsbibliothek* at Berlin, and have had a transcript made, from which I have prepared the following summary,—a small contribution to the archaeology, if not to the history, of psychology.

Goerenz sets himself a simple task of compilation, that of bringing together all the passages from ancient writers that anticipate the modern doctrine of association of ideas. He sees that there are two main difficulties in the way of a properly historical treatment: the lack of documents for the earlier period, and the danger of reading into his authorities more than they themselves intend. He avoids these difficulties, again very simply, by quoting his sources for what they are worth, and by cautious phrasing of his interpretations. His arrangement is chronological.

The first name on his list is that of Pythagoras (c. B. C. 580–500). If we may trust what we are told of Pythagoras' rules for the cultivation of memory, their author had some inkling, however vague and indefinite, of the two laws of succession (*ordo*) and resemblance. Simonides of Ceos (c. B. C. 556–467) invented a mnemonic art, which evidently implies the three laws of resemblance, simultaneity and succession (*ordo*); Simonides himself lays especial emphasis upon succession. We are still far from an express formulation of the laws of association; but Simonides' practice is surprisingly modern. There is no reason to think that the sophist Hippias (a contemporary of Socrates), or Theodectes (c. B. C. 376–335), or Metrodorus of Scepsis (of the time of Aristarchus) added anything of importance to his teaching.

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[1] Sir W. Hamilton, *The Works of Thomas Reid, D.D., Now Fully Collected, with Selections from his Unpublished Letters*, Note D.**, *Contribution towards a History of the Doctrine of Mental Suggestion or Association* (ii., 1863, 889 ff.).

Goerenz now makes his sole reference to American psychology; I quote in full. "I have this one thing to add," he says, "that the barbarous tribes of America make use of a certain art of memory which is not so very different from that employed in antiquity. For just as the ancients bound things fast (so to say) to certain places, by way of images, so do those barbarians connect things with cords from which are hung an immense number of little shells of various colors, and which they call 'belt' or 'string' or 'fathom of wampum.' By means of such cords, then, to which they take recourse on every occasion that is at all serious, and which they religiously preserve, they both strengthen and prolong their memory of past deeds; for when they look at any particular cord they readily recall what was said and done at the time when they first received it."[2]

From Simonides we take a long step forward to Plato (B.C. 427–347); Socrates does not come into our story. Goerenz discusses four principal passages: the experiment with the slave-boy, in the Meno; the proof of immortality from the doctrine of recollection, in the Phaedo; the image of the waxen tablet (origin of truth and error), in the Theaetetus; and the comparison of the mind to an aviary, also in the Theaetetus.[3] He concludes that Plato knew a good deal about the laws of succession (*ordo*) and resemblance, and had at least a vague and partial knowledge of the law of coexistence. Plato, moreover, is the first and perhaps the only ancient writer to derive and explain from these laws of association the origin and causes of false opinion. And so (the sentence is surely worth its space!) "nihil obstitisse Nostro videtur, quominus eo usque penetraret, quo nostra aetas hac in re evasit, nisi illud, paene dixerim vagum disputandi genus, quod sibi indulsit; quo etiam fit, ut saepius rem dimittat, antequam ab omni parte considerata excussaque fuerit."[4]

After Plato comes Aristotle (B.C. 384–322), whose teaching is summarised under eight heads. "(1) We recollect, when the affections from things perceived by the senses so follow one another in the mind that each one arouses the next. (2) When we recollect, those things which originally affected the mind first are first aroused, and so on until we have arrived at the thing which we wish to recollect. (3) In recollecting, we start out from the like, the contrary and the contiguous. For the affections which present themselves are either the same as formerly, or simultaneous, or include a part in such wise that the remainder can readily be recalled. (4) In recollection the affections come to the mind afresh in the same sequence in which they originally impressed it. Hence we readily recollect things which, when we learned them, were held together by the bond of succession (*ordo*); otherwise we recollect with difficulty. (5) Recollection in general should start out from some fixed first term (*principium*), namely, from the term from which everything else may be found most readily. This is the term that holds the middle place. Suppose, for instance, that the letters *abcefg* stand for certain ideas of things, and suppose that you have found by memory the middle letter *d*, and that this is not itself the letter you are in search of: you may then turn to *e* or *f* or *g*; and if the thing you are in search of is not among these, by retracing your steps you will readily discover it in *c* or *b* or *a*. (6) Sometimes we cannot recollect; and this happens if, in remembering, we wander off from some first term in all directions. (7) Sometimes, in recollecting, we are carried away in the direction of our dominant habit; here as elsewhere habit is a sort of second nature. (8) But most of all we are led astray, in recollecting, in the emotions and passions of the mind, as in fear or anger."[5] Aristotle thus gives us, explicitly, the four laws of succession (*ordo*), resemblance, opposition and simultaneity.[6] He also makes use of the doctrine of association for the explanation of dreams.

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[2] Goerenz' source is G. H. Loskiel, *Geschichte der Mission der evangelischen Brüder unter den Indianern in Nordamerika*, [1788,] 34 f. There is an English edition of 1794.

[3] See B. Jowett, *The Dialogues of Plato*, 1892, ii., 40 ff., 213 ff.; iv., 258 f., 262 ff.

[4] I must add that Goerenz goes on: "At iudicamus forte audacius?" But the question is merely rhetorical.

Goerenz is surprised and disappointed that Aristotle, having said so much, does not in other contexts say more; but we have seen that Goerenz does not shine as critic. He finds little to his point in subsequent Greek philosophy. Carneades (c. B. C. 160) seems to have had a general notion of the association of ideas; the Stoics do not touch it at all.

Among Latin writers, Quintilian (c. A. D. 40–118) clearly recognizes the laws of succession (*ordo*), resemblance and coexistence, in their application to the art of rhetoric.

Maximus of Tyre, a Greek writer of the age of the Antonines, next calls for mention. Maximus does due justice to the great law of succession (*ordo*), but for the rest occupies himself with sub-laws—of time (night follows day, winter summer), of affection of the mind (love follows beauty, anger insult), of place, of (logical) distribution, of meaning (*potentia*)[7]—which may evidently be multiplied at will, and which are in any event nothing more than special cases of the true laws. Maximus derives in part directly from Plato and in part, perhaps, from Xenocrates; hardly from Aristotle.

The Neoplatonists yield us nothing; the few remarks of Porphyry (c. A. D. 230–300) are trivial. We thus come to the Fathers. Tertullian (A. D. 165–220), though he wrote a book on the mind, says nothing of the association of ideas. Everywhere but in Augustine (A. D. 354–430) we draw blank: Augustine appears to have been the first to see "that all our experience is born of the law of resemblance." In general, he follows Plato.

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[5] Cf. W. A. Hammond, *Aristotle's Psychology*, 1902, 203–212. Goerenz' summary is preceded by a polemic against Maass' interpretation of the term *κίνησις*, which Goerenz translates, in psychological context, preferably by *motus* (in the metaphorical sense), though he will accept *affectio*. The summary is followed by a discussion of the questions whether Aristotle placed the *notionum perceptarum simulacra* in the brain, and whether he regarded them as in any way corporeal. Goerenz answers both questions in the negative.

[6] Warren has recently stated that I agree with Hamilton's rendering of the key-passage in the tract On Memory and Recollection. The statement is not quite correct. I should translate: "This, too, is why we hunt through the series in thought, starting from our present topic or from something else,—I mean, from something similar or contrary or from the contiguous;" I do not see that the *καὶ* can otherwise make sense. See H. C. Warren, *A History of the Association Psychology*, 1921, 25 ff.; W. Hamilton, *The Works of Thomas Reid*, Note D.** (ii., 1863, 897 ff.). Cf. W. Wundt, *Kleine Schriften*, i., 1910, 560 f.; ii., 1911, 182 f.; iii., 1921, 502 ff.; *Leibniz*, 1917, 38 f.; and ct. H. Siebeck, *Geschichte der Psychologie*, i., 2, 1884, 77. Siebeck translates *τὸ σύνεγγυς* by *zeitliche Aufeinanderfolge;* if that translation stands there can, of course, be only three Aristotelian 'laws.'

[7] Cf. Siebeck, *op. cit.*, 309.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF NORTHWESTERN UNIVERSITY

Contributed by ROBERT H. GAULT

I. SOME DATA ON THE INTELLIGENCE OF JAPANESE CHILDREN

By TONAN FUKUDA

The following study of the intelligence of the Japanese children in Denver, Colorado, was made in June, 1922. The English version of the Stanford Revised Binet-Simon Intelligence Test was employed. However, in view of the double standard in their use of language, *viz.*, Japanese at home and English outside, it seemed wise to avoid the use of the tests which require familiarity with English, such as the Vocabulary Test or Repeat Syllable Test. Therefore, whenever possible, suitable alternative tests were used instead of these. Although a complete elimination was not possible, the disadvantage was removed to a considerable extent by this change.

The number of children tested is 43, which comprised practically all the Japanese children within our reach. They are classified according to their chronological ages and sex as follows:

Age	Number
Over 3	3
4	9
5	7
6	7
7	6
8	6
9	1
10	2
11	1
12	1

Sex	
Male	21
Female	22

The parents of these children are immigrants: none of them has been naturalized. Their home language is usually Japanese. They may be classified according to their occupations as follows:

Professional, intellectual work; large business......	33
Small business; skilled labor.....................	39
Unskilled labor................................	28

Almost all the children were neatly dressed and in good health, except one who was weak from digestive disturbance,—who, nevertheless, scored the highest point of all, *viz.*, I.Q. 143. None of these children was suffering from physical defect. In general they were in good humor and seemed eager to be tested, except in case of some of the minor children, who were so shy that a considerable amount of effort was required before a proper "rapport" was established.

The general result of the test is as follows:

Sex	Number	I. Q.			
		Median	Average	Highest	Lowest
Male	21	92	94	114	81
Female	22	97	100	143	67
Total	43	95	97	143	67

The following Table shows the distribution of the I.Q. of the total 43 children.

I.Q.	Number	Percentage
60 to 70	1	2
70 to 80	1	2
80 to 90	7	16
90 to 100	20	47
100 to 110	8	19
119 to 120	3	7
120 to 130	2	5
130 to 140	0	0
140 to 150	1	2

Fig. 1 presents the same distribution of the I.Q.

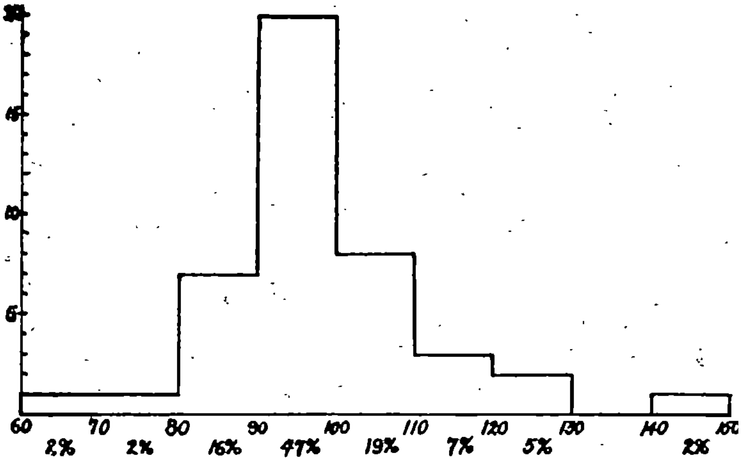

FIG. 1. Distribution of I.Q. of 43 Unselected Japanese Children, 3–12 years of age

Of this group, 24 children were in attendance at public schools, in grades ranging from I.B. to VI.B. The following Table shows the general findings of the intelligence of this group.

Sex	Number	I.Q.			
		Median	Average	Highest	Lowest
Male	13	96	96	114	81
Female	11	98	97	143	67
Total	24	96	97	143	67

The distribution of the I.Q. of these school children, which is presented graphically in Fig. 2, is as follows:

I.Q.	Number	Percentage
60 to 70	1	4
70 to 80	1	4
80 to 90	3	13
90 to 100	10	42
100 to 110	7	29
110 to 120	1	4
120 to 130	0	0
130 to 140	0	0
140 to 150	1	4

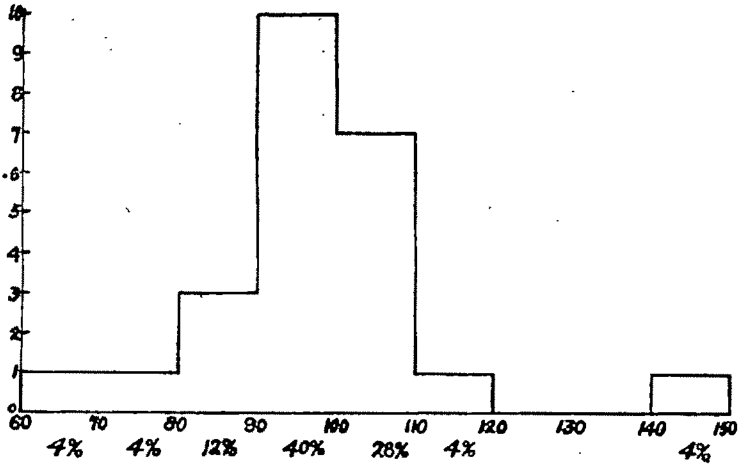

FIG. 2. Distribution of IQ. of 24 Unselected Japanese School Children, 6–12 years of age

It is interesting to note the relation of the mental ages of these children to their grades in the schools, which is shown graphically in Fig. 3. Of the total 24 cases only 8 are placed in their right grades, while 5 are too low, and 11 somewhat too high.

The coefficient of correlation between the I.Q. of these children and their teacher's estimate is 0.61, and that between I.Q. and school success is 0.47.

The average I.Q. in reference to the occupations of the parents of the children is as follows:

Professional, intellectual work; large business......	103
Small business; skilled labor......................	82
Unskilled labor................................	90

The coefficient of correlation between the I.Q. of these children and their social status is only 0.19.

It may be of interest to note some distinguished individual cases. The one who scored the highest marks of all is a girl of 10 years and 2 months by chronological age, but who is found to be 14 years and 7 months old according to her intellectual development. Her I.Q. is 143. At the time of taking the test she was ill, but she did not at all hesitate to go through the test. No illness, according to her teacher, would prevent her from making such an attempt. She is looked upon as a very active girl, the best student in her class and the leader of her companions. Her father is manager of a branch-office of a large Japanese dry-goods store. She was born in Japan and came to this country with her father about 4 years ago. It is therefore natural that her English vocabulary is rather limited; yet she scored 30, passing Year X of the series. In the Superior Adult Test she passed Binet's Paper Cutting Test and Repeating Seven Digits Backwards Test, missing the Ingenuity Test by one. It is not surprising, therefore, to find her superior in passing the Ball and Field Test and the Healy-Fernald Form Board Puzzle in less than 10 sec. Although she was said to have skipped grades, considering her intelligence, we may say safely that she should be in the eighth grade, if not in a high school, instead of in the fifth grade as at present.

The one who scored the lowest points is a girl of 9 years and 9 months by chronological age, but who is only 6 years and 6 months old according to her mentality. Her father is an unskilled laborer, working as a farm-hand in another state and leaving his daughter under the care of another family, living in Denver at the time of this test. Her slowness was recognized by her teachers. She was still classed in the second grade. But the cause of her slowness was attributed to her undesirable environment. The present test, therefore, is very illuminating on this point. Although she scored about half the tests in Year VII and one in Year VIII, she failed miserably in those which require higher comprehension, such as the Comprehension Tests, Description of Pictures Test, and Giving Differences and Similarities Tests. There is no doubt that her slowness is due to the retarded development of her mentality.

Grade / Age	I		II		III		IV		V		VI		VII		VIII	
	B	A	B	A	B	A	B	A	B	A	B	A	B	A	B	A
Under 5		●														
5-0 to 5-5		●														
5-6 to 5-11		●														
6-0 to 6-5		●		●												
6-6 to 6-11	●	●●		●●												
7-0 to 7-5			●	●												
7-6 to 7-11				●●												
8-0 to 8-5				●	●	●										
8-6 to 8-11						●										
9-0 to 9-5				●		●										
9-6 to 9-11																
10-0 to 10-5										●						
10-6 to 10-11										●	●					
11-0 to 11-5																
11-6 to 11-11																
12-0 to 12-5																
12-6 to 12-11																
13-0 to 13-5																
13-6 to 13-11																
14-0 to 14-5																
14-6 to 14-11										●						

FIG. 3. Age-grade Distribution of 24 Unselected Japanese School Children

II. TRADE-NAME INFRINGEMENT[1]

By EDWARD L. CLARK

With the rapid growth of large-scale production and advertising the significance of the trade-name has greatly increased. During the years just previous to 1905 our annual national registration of trade names and trade-marks was about 5,000; in 1919 the number of registrations reached 12,218. This great increase in registrations has been marked by a still greater increase in the amount of litigation concerning trade-names and trade-marks. Whether a trade-name is enough like another to cause confusion is, in part, a psychological question. Does the purchaser remember the name or mark of the article which he is buying well enough to recognize it the second time and avoid confusing it with a like article of similar label, wrapper, or trade-name? This problem, then, being one largely of recognition, is primarily psychological.

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[1] Abstract of a thesis submitted in partial fulfillment of the requirements for the degree of Master of Arts in Northwestern University.

Experimental work relative to this problem was undertaken at Northwestern University during the winter months of 1921–22. Two methods of determining confusion were compared with each other and with the decisions of courts in a number of cases of trade-name litigation. Sophomores and upperclassmen were used as subjects. An unselected group of 18 pairs of names was taken from a large number of court records. Nine of these cases had been declared infringing and had resulted in injunctions. For the experimental work the original names were separated from the imitation names in the case of both the infringing and the non-infringing names. These 4 sets were arranged in chance orders. In the first method of experimenting, the visual method, mimeographed copies of the original names of the infringement group were presented. Following these the imitations were presented, and with them directions instructing the *S*s to check those names which they considered as duplicates, and to indicate by a question-mark those about which they were doubtful. The same type of procedure was followed in handling the non-infringing names. In the other form of procedure, the auditory method, the *S*s were given the same amount of time for hearing the names as the others were for reading them. Following this, instructions were given about checking names as in the visual method. While the imitation names were being read, the *S*s recorded their reactions on sheets of paper which were handed in immediately at the end of the experiment. About one-half of the *S*'s were given the non-infringing names first, while the other half were given the infringing names first. *S*s giving reactions to names for the first time were designated as inexperienced, while those who had before reacted to one or the other of the sets of names were designated as experienced. The following is a Table of the average number of times the different types of names were checked and questioned, arranged according to the types of *S*s and methods. The values are expressed in percentages.

Set of Names Used	Set of Subjects	Type of Subjects	(?)	(—)	Experimental Method Used
Infringing	81	Inexper.	4.53	10.14	Visual
Infringing	93	Exper.	1.77	2.87	Visual
Non-Infrin.	86	Inexper.	3.23	7.88	Visual
Non-Infrin.	91	Exper.	3.18	5.25	Visual
Infringing	150	Inexper.	3.63	18.44	Auditory
Infringing	120	Exper.	2.22	3.98	Auditory
Non-Infrin.	120	Inexper.	2.22	10.18	Auditory
Non-Infrin.	150	Exper.	4.75	7.26	Auditory

In the case of each group, as can be seen by inspection of the Table, the percentage of names checked by experienced *S*s was the smaller. This difference, due to the effect of experience, is much more marked in the auditory method than in the visual. The average percentage of checks of the two sets, the infringing and the non-infringing, by the visual method for the inexperienced *S*s was 9.01, while for those who had given reactions once it was 4.06. But for the auditory method this drop was from 14.31% to 5.62%. The ratio of the change in the visual was about 11:8, while in the auditory it was about 20:8. This result apparently indicates that a knowledge of the purpose of the experiment was more capable of being used in the auditory than in the visual method. If the questioned names are considered to represent one-half as much confusion as the checked ones, the percentages then become for the visual 11.20 and 7.28 and for the auditory 15.61 and 7.36. If we express these values in the ratio used above, the two sets of visual data are in the ratio of 12:8 and the auditory 17:8. (Since the students were told to question those names about which they were doubtful, it is fair to assume that the question-mark had one-half the value of the check. The question-mark was used to represent degrees of confusion from the just bare confusion to almost absolute confusion, so that it meant on the average just one-half as much confusion as the check.)

The results also show that there was less questioning in the auditory method even though there was more confusion. The *Ss* seemed to be more sure of their judgments when they were checking for duplication of names by the auditory method. This fact and the fact of more confusion noted above suggest that names which confuse by similarity of appearance are less likely to infringe than those which depend upon similarity of sound.

To both the infringing and the non-infringing groups were added a name and its exact duplicate. This was done to serve as a control. The control pair in the infringing group of names was somewhat shorter than the pair used with the non-infringing names, but on the average they were recognized as duplicates almost an equal number of times. The former pair was recognized 81.87% of the time and the other 85.90%. An examination of the data of those who failed to recognize the exact duplicates indicates that they checked on the average as many pairs as the other *Ss*.

In making the final grouping of data only the results from the inexperienced *Ss* were used. It was felt that the experienced *Ss* knew too much of the nature and purpose of the procedure to give results comparable with those of the inexperienced. Psychologically, the inexperienced *Ss* corresponded more closely to the buyer in the market. The order of the degree of confusion of both sets of names as determined by the visual method of procedure, when compared with the order of names as determined by the auditory method, resulted in a correlation of .86. Although the absolute amount of confusion was a little higher in the auditory method, the relative amounts were very nearly the same. The following is a Table of the confusion as determined by the combined results from the auditory and visual methods by inexperienced *Ss*. The trade-names are numbered to indicate pairs which were used in the experimenting. "N" denotes non-infringing in the opinion of the courts, while "I" signifies infringing pairs.

Order	Percentage of Confusion	Trade-Names	Order	Percentage of Confusion	Trade-Names
1 & 2	.62	N—7	10	.862	I—9
	.62	N—10	11	11.15	I—8
3	3.04	N—5	12	12.36	N—4
4	3.16	I—4	13	16.39	N—9
5	4.57	N—8	14	18.21	N—1
6	5.82	N—6	15	21.00	I—2
7	6.27	I—7	16	31.91	N—3
8	6.98	I—1	17	40.36	I—5
9	7.34	I—33	18	42.12	I—6

Assuming that the courts based their decisions entirely on the possibility of confusing the appearance, sound, or meaning of the names irrespective of their surroundings (and this was the case with several pairs of the names used), we could then by examining the data on infringing and non-infringing names determine the court's standard of the amount of confusion necessary to constitute infringement. If such an assumption were made, confusion would become dangerous under these experimental conditions somewhere between 10.39%, the average percentage of confusion of the non-infringing names, and 16.33%, the average percentage of confusion of the infringing names. If we take either of these as a standard, it is evident that in these cases the courts either based their decisions on things other than the appearance, sound, and meaning of the names or else made several mistakes in judging the possibility of confusing the names, because neither of the percentages suggested divides the infringing from the non-infringing names. In fact, either percentage puts an equal number of infringing names on either side of the dividing line.

In speaking of percentages of confusion it must always be remembered that these percentages for these names are true only under the experimental conditions used in this work. However, the relative position of these names would not change much under other experimental conditions. Attention is called to the fact that the names do not divide themselves into confusing and non-confusing but form a continuum; many degrees of confusion are represented from the least confusing to the most confusing.

Reference

Paynter, R. H., *Psychological Study of Trade Mark Infringements*, New York, pp. 73, 1920.

BOOK REVIEWS

Tobacco and Mental Efficiency. By M. V. O'Shea. New York, The Macmillan Co., 1923. pp. xx, 258.

This is the first study to be issued by the Committee to Study the Tobacco Problem, which was organized in 1918 "with the object of collecting and publishing scientific data regarding tobacco and its effects, particularly physiological and economic." The statement is made that the Committee "contains both smokers and non-smokers; that the views of its individual members probably differ considerably with reference to different phases of the question, but that they are all agreed in the desire to ascertain the facts and in the willingness to accept them, whether the ultimate results may confirm or qualify the views previously held." We should expect, therefore, a study which is, in so far as is humanly possible, unprejudiced. The reader will be impressed throughout the volume with O'Shea's earnest endeavor to fulfill this obligation.

The work is divided into three parts according to the source from which the evidence is obtained. Part I contains Data derived from Observation, Introspection and Biography; Part II, Data derived from School and College Records; Part III, Data derived from the Psychological Laboratory. As the Committee has stated that its object is to collect and publish scientific data, the evidence presented challenges our attention particularly from this point of view.

A study of the "Habits of Prominent Men of the Past" (Chapter II) throws no new light upon the problem, either for or against tobacco. It does yield, however, some interesting anecdotes, such, for example, as the saying attributed to Stevenson "that if his doctor told him that smoking would kill him, he should continue to smoke since he would have to die sometime, and he was certain that nothing could bring death more pleasantly than tobacco."

The results of a questionary sent to thirty-five distinguished men in ten different fields of activity are presented in Chapter III. Striking differences of opinion are shown, and it would seem quite impossible to draw any conclusions from these observations and testimony. A scientist, for example, replies that "I do not use it and never have . . . Young men—boys—from fourteen to twenty-five, who use cigarettes, show lack of energy, hazy memory, general unreliability. More forceful, however, is the pernicious effect of tobacco in any form upon the aesthetic and moral nature . . . The habit is a vile one, its influence bad in *every* way." Another scientist states that "I was a persistent user of tobacco in all forms of smoking for more than forty years. In early life I had some prominence in athletics and have been interested in out-of-doors work since. I have not used tobacco in any form for more than two years, simply because the desire to do so has disappeared. I have not noticed at any time any change or any influence." Such statements cannot, of course, be reconciled: they are declarations influenced to a greater or less degree by the personal prejudices of the writer.

In Part II are given the data derived from a study of high school and college records. Here the attempt is made from reports of previous investigators, the replies of principals and faculties of high schools, and school and college records, to ascertain whether any relationship exists between scholarship and smoking. It is shown that smokers are inferior to non-smokers in school and college work, and that in certain fields smokers are not inferior in general intelligence to non-smokers. Considering two thousand cases, a correlation of —.51 was found between smoking and scholarship, which is interpreted to mean that "smoking exerted a detrimental influence upon the scholarship of a large proportion but not all of the pupils whose records were examined." "The depressing factor is undoubtedly smoking, for the reason that the standings of smokers in the early years, before tobacco became a factor in their intellectual operations, were as high as the standings of non-smokers." The reviewer would raise a question as to the value of this evidence. A correlation of —.51, if correlation by the method of association be valid here, does nor necessarily mean that "smoking exerted a detrimental influence upon scholarship," but rather that there is a negative relationship, which should be interpreted in this case to mean that smoking is associated with or accompanied by a decrease in scholarship. There is no justification for saying that smoking in itself directly influences scholarship. It would appear more reasonable to state that some factor, or factors, possibly other than smoking, are the cause of the decrease in scholarship; and that smoking might be identified with or associated with this unknown factor upon the basis of much more substantial proof than we have at present. This seems to the reviewer the only possible conclusion which may be drawn from the evidence presented. If a pupil, for example, loses interest in school work and tends to develop certain habits, among which may be smoking, that are detrimental to success in school, then one is not privileged to draw the conclusion that smoking is the cause of the failure. Rather it is the attitude of the students towards school work—a problem which is exceedingly complex and intricate.

In the laboratory investigation, reported in Part III, apparatus was so arranged that the observer would have "all of the sensations of smoking but without the use of tobacco." "The testimonies showed that the control fulfilled its purpose completely and that the factors of interest, prejudice and suggestion were prevented from playing a disturbing rôle in measuring the effect of tobacco as a drug upon intellectual functions." "On certain days the subject would smoke actual tobacco, on other days the control pipe; but he would believe that he was always smoking tobacco." The original plan for the study called for experimental work upon high school students, and adult men and women. It was decided, however, that "it would be inadvisable to ask any woman or immature boy who had never used tobacco to undergo experiments which would require smoking."

A separate monograph is promised giving a full description of the methods and apparatus used in the laboratory investigation. We can notice, therefore, only certain general points concerning the method and results of these experiments. In the first place, the subjects are grouped as smokers and non-smokers with no reference to the actual extent of the smoking of the subjects used in these experiments or the length of time during which they have smoked. If tobacco be a drug, then it is a physiological fact that the effect of tobacco as a drug upon subjects would be greater or less according to the extent to which they had previously been subjected to it. In other words, the effect of tobacco upon a person who had never smoked would probably be quite different from its effect upon a person who had smoked for a long time. Similarly, it is possible that the effect would be different upon a person who had smoked for a year or two from that upon the person who had smoked for ten years.

In the second place, in the Summary of the Laboratory Data (Table N), where it is shown that there is a gain in some mental functions and a loss in others under the influence of tobacco, an average is taken for every subject on a large number of different tests. The units in the different tests are of unequal value and mean different things; and if a final average for every subject is to be taken, then certain tests must be given more value than others, as they represent more important mental functions. Subject 1, for instance, shows a loss of 28.40 in "Muscular Fatigue" and a gain of 22.60

in "Accuracy of Addition." The conclusion, therefore, that "it can be said that, taking a large number of individuals, tobacco will slow down and disturb the intellectual processes of the majority as shown in Table N" would rest upon the assumption that "Muscular Fatigue" and "Accuracy of Addition" are equally important functions in the mental life of the individual, contributing alike to his mental efficiency. As this assumption cannot be accepted, the results of the laboratory investigation simply show that there is a loss in some functions and a gain in others, and that it is impossible to draw any generalizations from these results.

The problem of the effect of tobacco upon mental efficiency still remains, it seems to the reviewer, in that limbo of personal opinion where one person's opinion is as good as another's. It is to be regretted that the Committee, whose definite purpose is the presentation of scientific data, has inaugurated its programme with the present volume.

SETH WAKEMAN

Cornell University

Social Psychology. By ROBERT H. GAULT. New York, Henry Holt & Co., 1923, pp. x, 336.

Under the category "social psychology," one finds a great divergence of material. The Durkheim school was principally interested in developing a defence of the group-mind concept; McDougall in presenting a rigid scheme of instincts and emotions as the core of social behavior; Wallas and Thomas in giving us a picture of personality modified largely by cultural influences; Ellwood in positing a systematic social psychology after the analogy of other systematists in psychology and sociology. Gault's book is not so ambitious as most of the previous works but is more satisfactory, in many ways, for elementary use.

Taking a sensible view of the prevalent controversy over instincts in psychology, he holds that while the concept "instinct" may be valid, much more important are the habits and "complexes" built up by conditioning, and the imagery and idea of other persons which we develop in experience. He also finds the concept "group mind" or "group consciousness" as unnecessary for his purposes as he does the system of instincts *à la* McDougall. After this preliminary survey of the field and method, he goes on to discuss the sense of social unity, social motives, intellectual and racial differences in reference to social problems, suggestion and crowd phenomena, convention, custom and morale, ending with three chapters which deal with questions of social adjustment and social progress.

The author draws upon the recent studies in mental testing, and connects them with matters of social behavior; and though sympathetic with the movement for intelligence-measurement, he is critical of the wide assumptions made about the meaning of the results therefrom. He realizes, both from the side of experimental psychology and from the side of sociology, the incompleteness of the methods and findings of mental testing. His treatment of negro intelligence, for instance, is far more fair-minded than much of the usual writing of psychologists who know the negro only through the superficial contact of the classroom and the statistics of averages and correlations.

In his chapter on social progress, Professor Gault might well have discussed the rise of the *idea* of progress itself and its psychological effect in terms of habits, sentiments and values. Instead, there is a rather too naive acceptance of progress, *i.e.*, betterment, as inevitable. There is strong hope for social advancement, social control, and the power of the environmental factors to alter present maladjustments. One or two minor criticisms may be offered: first, too lengthy quotations are made where summarizations would suffice; secondly, the practice of omitting page-references, especially to long quotations, is undesirable.

The book is well-written, and contains well-selected concrete materials. While one wishes other phases of social psychology, such as the place of the press and propaganda in making public opinion and a discussion of the growth of new folkways and *mores*, in a book of this type, on the whole the work is welcome, and ought to appeal both to the college student and to the general reader.

Clark University KIMBALL YOUNG

Public Opinion. By WALTER LIPPMANN. New York, Harcourt, Brace & Co., 1922, pp. 427.

This is a presentation of the author's views on the factors behind the formation of public opinion. After spending three-fourths of his book in showing how public ideas and attitudes are the outcome of cleverly planned schemes of special interests, he announces a very simple plan to remedy this state of affairs by certain conscious social controls, such as bureaus of government research, which would give the people the truth and nothing but the truth. From this truth the populace would react sensibly, and all would be well.

The book is primarily of interest as an example of the use of one psychology, based on the analysis of instinctive, unconscious trends making for an irrational type of social behavior, as witnessed in the creation of public opinion, and then, at the close of the volume, the use of another psychology, bearing the tinge of eighteenth-century rationalism, *plus* that of recent social idealism, in projecting an utopia of a perfectly adjusted human nature and social order.

The concrete material is excellent, and the book is well-written. Mr. Lippmann has had some years of newspaper experience, and knows whereof he speaks on the practical side of journalism.

Clark University KIMBALL YOUNG

Studies in Mental Deviation. By S. D. PORTEUS. Vineland, N. J., The Training School, Department of Research Publication, No. 24. October, 1922, pp. xi., 276.

This volume is made up principally of collected papers which report researches completed under Dr. Porteus' direction. In reading these papers one feels that the writer has an intense, human sympathy for the problem of the mentally subnormal, a keen appreciation of the subtlety of the sociological factors making for defectiveness and delinquency, and a trained mind in devising novel ways of attacking the important social and racial problem of the defective through the aid of psychology. In reference, for instance, to the oft-repeated assertion that "every defective is a potential criminal," the author points out that, of the large number of cases he has studied, only "13% show delinquent tendencies," and further that "the really great menaces to society are the dull normal delinquent and the psychopathi groups" (pp. 24, 25).

After giving a brief survey of the aims, methods and materials of his work, he goes on to present his results. There is a significant chapter dealing with new anthropometric studies on brain-capacity and physical growth. There are two valuable chapters on his own maze-tests, and an excellent chapter on a social rating-scale for personality-traits, the results of which show the inadequacy of both the Binet scale and his own maze-test, and by implication of all similar tests, in diagnosing the social worth of an individual. His standpoint on the Binet scale in Chapter VII ought to be read and reread by every worker in the field of testing. Quoting a previous article of his own he writes: "The criticism that may justly be founded on this analysis is that the tests are far too 'literary.' They favor the glib-tongued, quick-thinking child, the child who has had a good educational environment, the child who memorizes readily and to whom expression is easy, and who, therefore, shows good scholastic promise" (p. 188). At the same time, they penalize, as he shows, the shy child, the child of slower, more deliberate thinking, and all those whose chief traits run to the vocational, manual sort. At the close of this chapter he indicates the inaccuracy of attempting classification of feeblemindedness on the basis of the Binet scale alone.

The concluding chapters present material on the use of educational tests with defectives, on performance-tests, and on a new industrial rating scale which he has devised.

The book is readable, and contains a solid array of concrete data of value to the psychologist, sociologist and social worker.

Clark University KIMBALL YOUNG

The Psychology of Social Reconstruction. By G. T. W. PATRICK. Boston and New York, Houghton Mifflin Co., 1920, pp. xii, 274.

The author weaves together in this book an interpretation of contemporary history in the light of the psychology of instinct, a criticism of current programmes of social reconstruction, and some positive suggestions of his own as to proper methods of social reform.

Many recent events—beginning with the dancing craze and ending with the World War—indicate that man's instinctive nature, after a period of "getting civilized too rapidly," is now reasserting itself. So quickly has our social environment changed, while our innate dispositions have remained unaltered, that under present conditions some of our most insistent tendencies find themselves blocked, and the lack of an instinctive basis threatens society with disintegration. If this diagnosis is correct, any attempt at social reconstruction must take the psychology of instinct into account. The popular schemes of reconstruction, however, all derive from a one-sided theory of pleasure-economy.

Education, eugenic regulation, and the application of science to social problems are the remedies that the author prescribes for the malady which he would characterize as a "temporary social fatigue of the higher brain centers."

S. FELDMAN

Cornell University

Unconscious Memory: with an Introduction by Professor Hartog. By SAMUEL BUTLER. New York, E. P. Dutton & Co. [1923.] pp. xxxix., 186. $3.00.

Butler's "Unconscious Memory" was published in 1880, and reprinted with a Note by R. A. Streatfeild and an Introduction by the professor of zoology in University College, Cork, in 1910. The present edition is a reprint of that of 1910, with the addition to the Introduction of a one-page postcript (dated 1920), in which Professor Hartog brings together some of the chief references to Butler's biological work. The book has its place, of course, in the series of Butler's writings; but it seems a pity to recall old controversies; and those interested have long ago made up their minds about Butler and Darwin.

The main interest to the psychologist is the translation of Hering's lecture on memory. Butler's translation of Hering, and for that matter his translation of von Hartmann also, cannot be called accurate; but it is written in admirable English, and might be made the basis of an exceptionally good rendering.

The book would have benefited by editing. Le Dantec and Semon are no longer living; Baldwin is not now professor of psychology at Princeton; and the remark "The lecture is published by Karl Gerold's Sohn, Vienna" is hardly an adequate bibliography of Hering's address. Worst of all, the book has no index.

E. B. T.

Discours de la methode, by DESCARTES. Cambridge Plain Texts: Cambridge University Press, 1923, pp. vi., 74.

In 1899 the Open Court Publishing Co. issued, for twenty-five cents, an edition of Descartes' *Discourse on Method* in Veitch's translation. John Veitch was a professor of logic and rhetoric in the University of Glasgow, who published in 1850–1853 translations of the Method, the Meditations, and selections from the Principles, which combined in a single book went through many editions. The Chicago reprint of the Method has a portrait, a 4-page preface, two pages of 'Descartes literature,' and a two-page index. The portrait and the index are worth while; the bibliography is a failure.

The Cambridge University Press now gives us, for fifteen pence, a reprint of the Method in French, with a two-page introductory note. This introduction, which is partly biographical, is better than the Open Court preface. The French text has a single foot-note, which informs us that "*poêle* = room with a stove;" a fact that we might, perhaps, if only by the aid of *stove* and *Stube*, have arrived at for ourselves. The Cambridge editor, on the other hand, fails to explain the *ce volume* of pp. 27, 63; Veitch properly reminds us that the first edition of 1637 contained, besides the Method, the Dioptrics, the Meteorics and the Geometry.

The right introduction to such reprints is, surely, a combined biography and bibliography, set out in the form, not of an essay, but of a chronological table. The reader—the kind of reader that the books attract—would welcome this summary orientation; undocumented references to St. Augustine and St. Anselm will leave him blank or irritated.

E. B. T.

NOTES

THE LEIPZIG CONGRESS OF PSYCHOLOGY

The eighth biennial congress of the *Gesellschaft für experimentelle Psychologie* was held in the Psychological Institute of the University of Leipzig, April 17th-20th. Professor G. E. Müller, the president of the *Gesellschaft*, served as chairman. There were approximately nine hundred registered visitors, of which number sixty came from countries other than Germany or Austria. Fifty papers were read in the course of the congress, divided between two simultaneous sessions. One session dealt exclusively with problems of general psychology, and the other with the various specialized branches. The papers might be classified according to their content as follows: *Sammelreferat*, dealing with the *Strukturbegriff* in German psychology, 1; *Sammelreferate*, dealing with the problem of personality, the central theme of the congress, 3; general papers dealing with psychological theory and methods, 15; reports on experimental work, 13; applied psychology, 7; social psychology, 3; animal psychology, 3; child psychology, 3; psychopathology, 2. Six of the papers were presented by foreign psychologists, viz., Spearman (London), Grünbaum (Amsterdam), Gudaitis (Kowno), Juhász and Nagy (Budapest), and Sneersohn (Moscow).

Professor Krueger (Leipzig) opened the congress with a *Sammelreferat* dealing historically and critically with the concept of psychical structure, the central doctrine of the *Gestalt*-psychology.[1] The concept, said Krueger, has been developed within the past decade simultaneously from four directions, the psychologists of each direction working quite independently of the others. The time is now ripe for a comparison and conciliation of these various expressions. The four sources to which Krueger referred the doctrine may be briefly characterized.

(1) *The approach through perception.* Wertheimer in 1912, in his report on the perception of movement as a centrally produced conscious content (the *Phi-Bewegung*), proposed the hypothesis that for the *Auffassung* of phenomenal movement it is not the excitation process in the excited cells themselves, nor the sum of the single excitations, which is essential. The principal rôle falls to characteristic cross-processes and total processes, which result as specific wholes (in larger areas) from the excita-

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[1] It should be kept in mind that the *Strukturbegriff* in modern German psychology differs significantly from the concept of structure in American psychology. Whereas in American structural psychology the composition of experience at a given moment is expressed in terms of the 'mental elements,'—such, for example, as sensations, images, and affections,—the very essence of the German *Strukturbegriff* is to deny the existence in experience of these or, indeed, of any mental elements. The mental structure, or *Gestalt*, is in every case something more than a summation of elements. Each experience has a uniqueness, a totality and an indivisibility which are not referable in any sense to 'constituents'. American structural psychology, furthermore, assumes a close correspondence between stimulus and sensation, while the German doctrine emphasizes that the state of the entire nervous system is a factor of great importance in the stimulus-*Gestalt* relationship. The *Gestalt* is in no sense a simple or direct function of the stimulus. A sensation, the alleged function of the stimulus, never exists in experience; it is a psychological abstraction.

It will avoid ambiguity if we designate the German structuralism as *Gestalt*-psychology, but it will be necessary to keep in mind that the original conception of *Gestalt* relates rather narrowly to the sphere of perception, and that the doctrine of the *Struktur* has been extended in other directions.

tion of single cells (the *Einfallstelle*).[2] Sensory data are always to some degree '*gestaltet*', in such a way that the resultant content is possessed of concrete *Ganzeigenschaften*, of *Ganztendenzen*, and of an 'inner' *Gesetzlichkeit* of its own. Working in the same direction as Wertheimer we find Koehler who concerns himself with the theory as it relates to the physical and physiological correlates of perception. According to Koehler *Gestalten* are those psychical states and processes whose characteristic qualities and effects are not composed of the qualities and effects of their so-called parts. Koehler undertakes to establish the *Reiz-Erlebnis* relation which exists between the physical systems in the outer world which he believes possess *Gestalt*-characteristics and the phenomenal or psychical *Gestalten.*[3] Proceeding from this general position of Wertheimer and Koehler is a variety of contemporary research which aims to formulate in terms of *Gestalt*-psychology the laws of limen, phenomenal movement, differential threshold, contrast phenomena, fusion, optical illusion, etc.

(2) *The approach through Geisteswissenschaft.* Dilthey has been influential in directing much psychological thinking away from the explaining and analyzing method, and in establishing a psychology in which the mental *Lebenszusammenhänge* are regarded as indivisible and inseparable functions of the entire personality. The mental life as a whole is regarded as a teleological structure. The most that this new psychology can hope for, says Dilthey, is to establish a *Typenlehre*, or a systematic exposition of the mental life of a typical man.[4] E. Spranger, Dilthey's disciple, defines the psychological task not as a search for the ultimately distinguishable elements, but as a search within a higher level of concepts for the distinguishing characteristics of the *Gesamtsituation*. The practical consequence of Spranger's approach is also a *Typenlehre.*[5]

(3) *The genetic approach.* Krueger, starting with v. Ehrenfels' conception of *Gestaltqualität*—which ascribes to tonal and optical perceptions an independent quality in addition to the sum of the single sensory excitations: *e.g.*, the *Gestalt*-quality in a chord of music—maintains that these *Gestaltqualitäten* in reality represent the essence of the perception, and exist *in place of* any constituent or partial experiences. In every case the 'psychic totality' is a unity quite independent of its parts. Each experience is possessed of a peculiar *Komplexqualität* of its own. Splitting this *Komplexqualität* in any way into elements or atoms destroys the very essence of the experience. In primitive man and in childhood we find that consciousness is predominantly a matter of an undifferentiated, fused, and complex content. Krueger emphasizes strongly the merit of studying the *Gestalt*-principle genetically.[6]

(4) *The approach through Personalismus.* As a supplement to his psychology of individual differences, Stern has stressed of late years the unity of the personality as an indivisible object for psychological study. The mental composition of the individual, the elementary psychical processes, habits, traits, and unit characteristics are by no means the extent of the psychological problem. The entire synthetic side of man's nature, without consideration of which any isolated process or trait would be meaningless, must be considered in all its uniqueness and indivisibility.

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[2] *Cf.* M. Wertheimer, Experimentelle 'Studien über das Sehen von Bewegung *Zsch. f. Psychol.*, 1912, 61. Also Untersuchungen zur Lehre von der Gestalt, *Psychol. Forschung*, 1922.

[3] *Cf.* W. Koehler, *Die physischen Gestalten in Ruhe und im stationären Zustand, eine naturphilosophische Untersuchung*, 1920.

[4] *Cf.* W. Dilthey, *Die Typen der Weltanschauung, Sammelband: Weltanschauung, Philosophie, und Religion*, 1911.

[5] *Cf.* E. Spranger, *Lebensformen*, 1922.

[6] *Cf.* F. Krueger, *Ueber Entwicklungspsychologie; ihre sachliche und geschichtliche Notwendigkeit*, 1915.

Man is *unitas multiplex*. The unity does not consist merely in the addition or summation of traits, but in the harmonization of all psychical processes within the individual (viz., his experience, his acts, his dispositions, and his Ego) into an active purpose. More fundamental than differential psychology, by far, is the problem of the nature, the activity, and the unity of the *total personality*.[7]

The *Sammelreferat* by Krueger may be taken as representative of the Congress as a whole—a unification of the various expressions of the *Strukturbegriff*. There can be no question that this energetic movement is the prevailing influence in contemporary German psychological thinking. The majority of the papers before the Congress assumed the axioms of *Gestalt*-psychology as their point of departure. The theme set for the Congress, the psychological problem of personality, itself indicates the current emphasis upon an undivided, complex psychological object. Selz (Bonn) and Sommer (Giessen) presented *Referate* concerning methods for the determination of personality types, and Peters (Mannheim) reviewed recent work concerning the influence of inheritance upon personality.

Among the specific researches relating to the general *Gestalt*-movement should be mentioned the report of Jaensch (Marburg) concerning the *Eidetiker*-disposition. Jaensch asserts that the primary after-image is the lowest stage in a continuous series of memory-levels. Between this stage and the *Vorstellungsbild* he claims the existence of the *Anschauungsbild*. The *Anschauungsbild* represents a retention of the original undifferentiated perception. The individual whose images are primarily of this type (the so-called *Eidetiker*) possesses the ability to reproduce with astonishing detail the original *Vorlage*, even after a considerable time has elapsed. A greater or less degree of the *eidetische Anlage* is to be found in all children, and frequently (if not always) in poets and artists. Jaensch proposes that a comparative psychology of races might well begin with a study of such a disposition. In this connection he offered the results of his tests for the *eidetische Anlage* in German and in French school-children in the city of Mainz. A further application of the work is in the field of pedagogical psychology where a new understanding is needed of the *Erlebniswelt* of the child. The testimony of the child's *Anschauungsbilder* shows that his world of experience is in no sense that of the adult, and that in general there exists for him a much 'rawer' relation between the *Wahrnehmungsbild* and the *Vorstellungsbild*. The *Anschauungswelt* in its structure is similar to the *Vorstellungswelt*, and yet is itself an exact copy of the *Wahrnehmungswelt;* hence for the *Eidetiker* the image and the perception bear a point-for-point correspondence.

Volkelt (Leipzig) reported his studies of the primitive *Komplexqualitäten* in children's drawing. It is characteristic that a young child, drawing from a model, reproduces not what he actually sees or what he knows to be the nature of the model (*e.g.*, a cube), but an elementary combination of the two, a complex quality; that is to say, he draws the essence of the thing in its totality as it *affects* him.

Katz (Rostock) reported upon a study of intelligence and character differences among animals. Working with chickens, partly with his former methods and partly with the tests devised by Koehler for apes, he arranged rank orders among the animals in regard to intelligence and certain traits of character, notably leadership. The character traits represented individual differences which were definitely not related to intelligence. Ettlinger (Münster) contributed a study concerning the use of tools by animals.

The papers dealing with applied psychology presented research pertaining chiefly to problems of industry and vocational selection. The

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[7] W. Stern, *Die Psychologie und der Personalismus*, 1917. Also *Die menschliche Persönlichkeit*, 3d. ed., 1923.

Gruppe für angewandte Psychologie holds its congress every two years, alternating with the congresses of the *Gesellschaft für experimentalle Psychologie.* This arrangement accordingly reduces in the larger congress the number of papers on applied subjects.

Among the remaining papers of the Congress were reports by psychologists whose general lines of research are fairly well known in America. Many of these papers contained a reshaping of old concepts to the prevalent *Gestalt*-psychology. Very little of the classical German psychology remains in active use. It was characteristic of the Congress that in none of the lectures or discussions was reference made to the name or the teachings of Wundt!

The material of these various papers will appear in due time in the usual periodicals. (Whether a *Bericht* of the Congress as a whole will be issued this year has not yet been decided.) The present report aims to represent the general *Stimmung* of the Congress, and to point out the newest and most characteristic lines of research. The American psychologist who will familiarize himself well with these researches and with the underlying concepts will find them invariably suggestive, and often challenging.

Hamburg GORDON ALLPORT

VISUAL ACUITY AND ILLUMINATION

In a recent paper,[1] Ferree and Rand show that visual acuity increases with increase in the intensity of illumination and that this effect is more pronounced in eyes slightly defective as to refraction than it is in normal eyes. They state further, that "the explanation of the effect of increase of intensity of light on the power to see clearly is to be found, of course, in terms of the resolving power of the retina, not of the refracting media."[2] The correctness of such an explanation may be questioned. Does the resolving power of the refracting media, that is, their ability to form optical images upon the retina, remain constant under changes in illumination? If it does, the resolving power of the retina must be the variable and the explanation is correct. But if the refracting system does not remain invariant under changes in illumination, the explanation given by Ferree and Rand is inadequate to the facts.

The resolving power of the refracting media of the eye depends in part upon the size of the pupil. The more peripheral zones of the lens-system suffer from errors of refraction to a much greater degree than the central.[3] Closure of the iris increases the definition of the optical image thrown upon the retina, just as the use of a diaphragm in the photographic lens increases the definition of the image thrown upon the sensitive film, that is, increases the resolving power of the photographic lens. Moreover, the size of the pupil of the eye varies with illumination, becoming smaller as the intensity of the light increases.[4] This means, of course, that as the intensity of illumination increases the errors of refraction cause less diffusion of the image because the iris cuts off from the refracting system its peripheral zones. The resolving power of the refracting media of the eye thus increases with increased illumination.

We see, then, that an explanation of the effect found by Ferree and Rand in terms of the resolving power of the retina does not do justice to all the facts of ocular response. Change in the resolving power of the refracting system is at least one factor causing acuity to vary with illumination. Whether or not change in retinal sensitivity is a further factor in producing variations of acuity can be determined only by observations in which the size of the pupil is held constant, either by means of an artificial pupil or by paralyzing the iris with atropine or some similar drug.

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[1] C. E. Ferree and G. Rand, this JOURNAL, 34, 1923, 244 ff. [2] *Ibid.*, 244.

[3] E. H. Starling, *Principles of Human Physiology*, 1920, 507; W. H. Howells, *A Text-Book of Physiology*, 1915, 319.

[4] E. H. Starling, *op. cit.*, 507f.; W. H. Howell, *op. cit.*, 327.

Although adaptation to low illumination deceases the amount of constriction of the pupil, it does not eliminate it.

University of Pittsburgh GILBERT J. RICH

OBSERVATIONS ON TAKING PEYOTE

Since writing my paper on Peyote (this JOURNAL, 34, 1923, 267–270) my attention has been called by Mr. Carl Boyer of Philadelphia to a previous article by Havelock Ellis. This article, entitled "Mescal, A Study of a Divine Plant," appeared in the *Popular Science Monthly* (61, 1902, 52–71). Ellis reports in full his own experiences and those of another subject on taking Peyote and also indicates the results of several other subjects. In general Ellis is interested in the visual phenomena, which are generally obtained and which I failed to get to any extent. But he also indicates other results which seemed to be more or less unique in my own case and which have not elsewhere appeared in the literature to my knowledge. Ellis notes the clearness and duration of after-images, and the magnification and increase in clearness of objects in the periphery of the visual field, and insists (as I found) that there is a general "hyperaesthesis to all sensory impressions." For the other subject whose results are reported both time and space were upset; for Ellis himself time was not affected. But Ellis reports an "inability to fix attention long," which corresponds closely to my own findings. Indeed, I attempted to analyse the effects of the drug in my own case in terms of a very vivid but very rapidly changing focus of attention.

SAMUEL W. FERNBERGER

University of Pennsylvania

THE EXPRESSION OF SIMPLE FEELING

In a recent critical Note I called attention to a quotation from one of our Cornell Minor Studies, a quotation so changed from its original, by omission, that I feared it might be misleading.[1] Professor Bentley now charges me with a misstep from logic of the same kind as that which I noted in the quotation to which I was referring: if that quotation was misleading by mutilation, my critical comment is misleading by omission of context. "Titchener should have been warned that Young's 'therefore' (in the quotation 'It is impossible, therefore, to accept Corwin's interpretation . . .') refers backward to the very qualification which Titchener seeks." The next preceding paragraph should thus have put me right.[2]

I still believe that my criticism was justified. But the reader shall decide for himself; I print below the relevant passages. I give (*A*) the paragraph in question; (*B*) the immediately following sentence, containing the mutilated quotation; and (*C*) the quotation restored to its proper form.[3]

(*A*) There can be no doubt that "seeking" movements *may be* associated with P. Corwin's experiment proves the point; but the conditions for such an association involve two factors: (*a*) a weak or receding stimulus, or some other unsatisfactory element in the conditions—a need, irritant, incentive, motive—which makes it necessary for *S* to seek or pursue if he is to react adequately and to maintain conditions under (*b*) stimulation which is normally P. The factors which condition seeking movements are obviously neither necessary nor favorable to the existence of P feeling. The necessity for seeking reduces the intensity of P obtainable and the reduction may go to I or even to U; extreme cases of seeking are U rather than P. If seeking movements express anything, that is an incentive, need or irritant rather than simple feeling.

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[1] This JOURNAL, 34, 1923, 149.

[2] *Psych. Rev.*, 30, 1923, 326 f.

[3] (*A*) and (*B*) are from this JOURNAL, 33, 1922, 522; (*C*) is from 32, 1921, 570.

(B) It is impossible, therefore, to accept Corwin's interpretation that "the direct response of the organism to P is . . . a definite activity of pursuit or of tendencies to pursuit."

(C) The direct response of the organism to P is, as stated above, either relaxation *with a certain degree of expansion*, if the stimulus is weak or stationary; or, if the stimulus is intense, and if the source of the P is withdrawn, a definite activity of pursuit or of tendencies to pursuit.—

I have said that (C) is significantly different from the quotation of (B). Bentley, if I understand him, replies in effect: 'Granted, so far as it goes! but you have missed the point that (B) is made to follow logically from (A), and that (A) contains the very qualification whose omission from the quotation in (B) you criticise.' I reply, in my turn, that (B) expresses, not agreement or acceptance, not even qualified agreement or acceptance, but flat disagreement,—and, to repeat my former criticism, disagreement with something that is significantly different from what Corwin wrote.

E. B. T.

Alcohol, Tobacco, Tea: Postscript

After the MS. of my article had gone to press, I received the recently published book on "Tobacco and Mental Efficiency" by M. V. O'Shea. Not a few of the statements of this book invite criticism.

Is it (scientifically) legitimate to talk of the influence of tobacco—a material substance, a drug—on "meditation, reflection, judgment, alertness, ambition and the creative activity," terms which fit well in any "faculty psychology," but which nobody can conceive of in anatomical-physiological terminology?

Here is a sample of the treatment given the tobacco problem in this book (p. 115) with reference to literary men and women who use tobacco: "He has now associated smoking and creative activity together, and the creative faculties will not function unless the pipe or cigar or cigarette is in hand or in mouth." Is that not old-fashioned phrenology? *Does not the creative faculty declare a sympathetic strike for the benefit of the smoking faculty which has been deprived of its weed?* The quotation is given as "a probable explanation" of the help which some people derive from tobacco. What is the meaning of "an explanation" among men of science? To me the above "probable explanation" is fiction.

Where proposing scientific problems differs so profoundly in method, a fruitful exchange of opinions seems precluded. In another instance, however, an agreement can be reached, since I may have failed to understand a description given by the author of the book. A certain "control experiment" was made at the author's request by Dr. Hull. "The function of the control was to keep all the factors in the experiment uniform except the one factor of the drug effect of tobacco." The reader of my paper recalls that I produced the drug-effect by filling the air with tobacco-smoke. In Dr. Hull's "control" S, blind-folded, had in his mouth an electrically heated, but tobaccoless pipe; and E close to him smoked a pipe filled with tobacco "so that the subject smelled the tobacco which he thought he was smoking, but which was smoked by the experimenter" (p. 181). I fail to comprehend how in this "control" S, who had to breathe the smoky air, *was protected against getting the drug effect of tobacco.*

I have some further experimental data on drugs which I intend to publish at a convenient time in the future.

University of Missouri MAX F. MEYER

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